SCIENCE PROGRESS

IN THE TWENTIETH CENTURY

A QUARTERLY JOURNAL OF SCIENTIFIC WORK & THOUGHT

EDITOR

SIR RONALD ROSS, K.C.B., K.C.M.G., F.R.S., F.R.S.L., D.Sc., LL.D., M.D., F.R.C.S.

ASSISTED BY

D. ORSON WOOD, M.Sc., A.R.C.Sc., F.Inst.P.

VOL. XXIII

1928—1929

LONDON

JOHN MURRAY, ALBEMARLE STREET, W.

1929

Printed in Great Britian by
Hazell, Watson & Viney, Ld., London and Aylesbury.

CONTENTS

CONTENTS

CONTENTS

CONTENTS

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By E. C. TITCHMARSH, M.A., University College, London.

Subharmonic Functions.—A subharmonic function is a function of two real variables x and y, $u(x, y)$ say, such that for sufficiently small values of r

$$u(x, y) \leqslant \frac{1}{2\pi}\int_0^{2\pi} u(x + r\cos\phi, y + r\sin\phi)d\phi.$$

Thus the value of the function at any point is not greater than the mean-value of the function taken round small circles with the point as centre. This class of functions may be regarded as the analogue for two variables of the class of convex functions of one variable, viz. functions $u(x)$ such that

$$u(x) \leqq \tfrac{1}{2}\{u(x - h) + u(x + h)\}.$$

The term subharmonic is justified by the remark that if $u(x, y)$ is harmonic, *i.e.* satisfies Laplace's equation $\nabla u = 0$, then

$$u(x, y) = \frac{1}{2\pi}\int_0^{2\pi} u(x + r\cos\phi, y + r\sin\phi)d\phi.$$

Any solution of a partial differential equation of the form $\nabla u = f$, where f is a positive function, is a subharmonic function.

The theory of these functions is due to F. Riesz, "Sur les fonctions subharmoniques et leur rapport à la théorie du potentiel" (*Acta Math.*, **48**, 1926, 329–43). Riesz shows that subharmonic functions have some of the properties of harmonic functions, *e.g.* they preserve their characteristic property under a conformal transformation; and also, if $u(x,y)$ is a subharmonic function, then

$$I(r) = \frac{1}{2\pi}\int_0^{2\pi} u(x + r\cos\phi, y + r\sin\phi)d\phi$$

is an increasing function of r.

More recently the subject has been extended in two papers by J. E. Littlewood. In the first ("On the Definition of a Subharmonic Function," *Journal London Math. Soc.*, **2**, 1927,

189–92) it is proved that the class of functions defined is unaltered if, instead of assuming that the above inequality holds for all sufficiently small values of r, we merely assume that it holds for *some* arbitrarily small values. Secondly, it is proved that we still obtain the same result if we assume instead the "averaged" inequality.

$$u(x, y) \leqslant \frac{1}{\pi r^2}\int_0^{2\pi} d\phi \int_0^r u(x + \rho\cos\phi,\, y + \rho\sin\phi)\, d\rho,$$

either for some or for all small values of r.

In the second paper ("On Functions Subharmonic in a Circle," *Journal London Math. Soc.*, **2**, 1927, 192–6), the question of the limiting value on the circle $x^2 + y^2 = 1$ of a function subharmonic in the circle is discussed. It is well known that, if $u(r, \theta)$ is *harmonic* for $r < 1$, and for some value of $p > 1$

$$\int_0^{2\pi} |u(r, \theta)|^p\, d\theta$$

remains bounded as $r \to 1$, then there is a function $U(\theta)$ such that (i) $u(r, \theta) \to U(\theta)$ as $r \to 1$ for almost all values of θ, and (ii)

$$\lim_{r \to 1} \int_0^{2\pi} |u(r, \theta) - U(\theta)|^p\, d\theta = 0,$$

or, as we say, $u(r, \theta)$ converges strongly to $U(\theta)$ with exponent p. In the case of a subharmonic function we cannot prove quite so much. Littlewood proves the existence of a limit-function $U(\theta)$ such that $u(r, \theta)$ converges strongly to $U(\theta)$ with any exponent q less than p.

Functions of Two Variables.—A famous theorem of Hadamard asserts that if the functions $f(z) = \Sigma a_n z^n$, $g(z) = \Sigma b_n z^n$, have singularities at the points $\alpha_1, \alpha_2, \ldots$ and $\beta_1, \beta_2 \ldots$ respectively, then the singularities of the function $F(z) = \Sigma a_n b_n z^n$ are to be found among the points $\alpha_p \beta_q$. It seems to have been thought by Hadamard, and others who wrote on the same subject, that the corresponding argument, applied to functions of two variables, led to no result of any interest. It has now been shown by U. S. Haslam-Jones ("An Extension of Hadamard's Multiplication Theorem," *Proc. London Math. Soc.*, **27**, 1927, 223–32), that this is by no means the case. His result is as follows:

Let

$$f(x, y) = \Sigma\Sigma\, a_{mn} x^m y^n, \quad g(x, y) = \Sigma\Sigma\, b_{mn} x^m y^n$$

be two functions of the complex variables x and y, having singularities satisfying the relations $p(x, y) = 0$, $q(x, y) = 0$,

respectively, where p and q are analytic functions satisfying certain conditions. Then the singularities of the function

$$F(x, y) = \Sigma\Sigma\, a_{mn}\, b_{mn}\, x^m\, y^n$$

are to be found among the points given by $x = uu'$, $y = vv'$, where $p(u, v) = 0$, $q(u', v') = 0$, and

$$uv' \frac{\partial p}{\partial u} \frac{\partial q}{\partial v'} = u'\, v \frac{\partial p}{\partial u'} \frac{\partial q}{\partial v}.$$

Suppose for example that

$$f(x, y) = \frac{1}{1 - x - y} = \Sigma\Sigma \frac{(m+n)!}{m!\, n!} x^m\, y^n,$$

$$g(x, y) = \frac{1}{1 - xy} \qquad = \Sigma\, x^n\, y^n.$$

Then Haslam-Jones's rule shows that the singularities of $F(x, y)$ must satisfy $4\, x\, y = 1$. Actually

$$F(x, y) = \Sigma \frac{(2n)!}{(n!)^2} x^n\, y^n = \frac{1}{\sqrt{(1 - 4xy)}}.$$

Some properties of bounded polynomials in several variables are discussed by O. D. Kellogg (*Math. Zeitschrift*, **27**, 1927, 55–64). The general question is, given the maximum absolute value of a homogeneous polynomial, what can we say about its coefficients ? Typical results are as follows :

(i) Let $P_n(x, y)$ be a homogeneous polynomial of degree n, such that $|P_n| \leqslant 1$ for $x^2 = y^2 = 1$. Then the modulus of the coefficient of the term in $x^k\, y^{n-k}$ cannot exceed the binomial coefficient ${}^nC_k$.

(ii) Under the same conditions, the modulus of the derivate of P_n in any direction never exceeds n for $x^2 + y^2 \leqslant 1$.

Bessel Functions.—R. G. Cooke ("Gibbs's Phenomenon in Fourier-Bessel Series and Integrals," *Proc. London Math. Soc.*, **27**, 1927, 171–92) has investigated the Gibbs phenomenon which occurs in the Fourier-Bessel expansion

$$f(x) = \sum_{m=1}^{\infty} A_m\, J_\nu(j_m\, x),$$

j_m being the mth positive zero of the Bessel function $J_\nu\,(z)$. The series represents the function over the interval $0 < x < 1$. As long as we consider internal points of this interval, the behaviour of these series is the same as that of the corresponding Fourier series. Hence if $f(x)$ has a discontinuity of magnitude c at an internal point, the partial sums of the series take

values, near the discontinuity, which differ not merely by c, but by

$$\frac{2}{\pi}\int_0^\pi \frac{\sin v}{v}\,dv$$

times c. This is the phenomenon discovered by Gibbs. The main result of this paper is that, if $f(x)$ behaves like cx^{-a} near the origin, and if we multiply the above series by x^a, we get values of the partial sums, not merely as large as c, but as large as

$$2^{1-a}\frac{\Gamma(1-\frac{1}{2}a+\frac{1}{2}\nu)}{\Gamma(\frac{1}{2}a+\frac{1}{2}\nu)}\int_0^{j_{\nu,1}} J_\nu(v)\,v^{a-1}\,dv$$

times c. Here $j_{\nu,1}$ is the smallest positive zero of $J_\nu(z)$. If $\nu = \frac{1}{2}$ the series becomes a Fourier series, and the "Gibbs ratio" reduces to

$$\frac{2^{3/2-a}}{\sqrt{\pi}}\frac{\Gamma(\frac{5}{4}-\frac{1}{2}a)}{\Gamma(\frac{1}{4}+\frac{1}{2}a)}\int_0^\pi v^{a-\frac{1}{2}}\sin v\,dv.$$

If also $a = \frac{1}{2}$ this reduces to the ordinary result.

Similar results are also obtained in connection with Hankel's formula

$$f(x) = \int_0^\infty u\,J_\nu(xu)du\int_0^\infty t\,J_\nu(ut)\,f(t)dt.$$

This formula is also the starting-point of a paper by J. W. Nicholson ("The Bessel-Fourier Theorem and the Logarithmic Bessel Function," *Quart. J. of Math.*, **50**, 1927, 297–314), in which a number of curious integrals involving Bessel functions are evaluated. It is first shown that for general functions $f(x)$

$$\int_0^\infty \lambda Y_0(\lambda\rho)d\lambda\int_0^\infty f(\mu)J_0(\lambda\mu)\mu d\mu = -\int_0^\infty \lambda J_0(\lambda\rho)d\lambda\int_0^\infty f(\mu)Y_0(\lambda\mu)\mu d\mu,$$

where $Y_0(x)$ is a Bessel function of the second kind. The following are some special results :

$$\int_0^\infty \lambda\,J_0(\lambda\rho)\,I_0(\tfrac{1}{2}\lambda)\,K_0(\tfrac{1}{2}\lambda)\,d\lambda = \frac{1}{\rho}\sqrt{(1+\rho^2)},$$

$$\int_0^\infty \lambda\,J_0(\lambda)\,Y_0(\lambda)\,I_0(\lambda)\,K_0(\lambda)\,d\lambda = -\frac{\pi}{8},$$

$$\int_0^\infty \lambda\,J_0^2(\lambda)\,J_0(\lambda y)\,Y_0(\lambda y)\,d\lambda = 0(y>1),\ \frac{1}{4y}(y<1),\ \frac{1}{8}(y=1).$$

Some more integrals involving Bessel functions are evaluated by G. N. Watson (*Journal London Math. Soc.*, **3**, 1928, 22–7). It was suggested by Prof. E. T. Whittaker, from physical

considerations, that there was a connection between integrals of the form

$$\int_0^\infty f(ct)\, K_1(at)\, J_0(bt)\, dt$$

and the expression

$$\frac{a^2+b^2+c^2}{a\sqrt{\{(a^2+b^2+c^2)^2-4a^2c^2\}}}.$$

Prof. Watson now proves that in fact

$$\int_0^\infty ct\, I_1(ct)\, K_1(at)\, J_0(bt)\, dt = \frac{1}{2a}\left[\frac{a^2+b^2+c^2}{\sqrt{\{(a^2+b^2+c^2)^2-4a^2c^2\}}} - 1\right].$$

Fourier Series.—Prof. M. Riesz's paper "Sur les fonctions conjuguées" (*Math. Zeitschrift*, **27**, 1927, 218–44), has at last appeared. This paper attained fame some years before its actual appearance owing to abstracts in the Comptes Rendus and the London Math. Soc. Records. It is one of the most striking and elegant of recent contributions to the theory. It is principally concerned with the theory of conjugate trigonometrical series. If we are given a Fourier series

$$f(x) \backsim \tfrac{1}{2}a_0 + \sum_{n=1}^{\infty}(a_n \cos nx + b_n \sin nx)$$

then the conjugate series is

$$\Sigma\,(b_n \cos nx - a_n \sin nx).$$

The classical theorems of Parseval and Riesz-Fischer tell us that if $f(x)$ is a function whose square is integrable in the sense of Lebesgue, then the conjugate series is also the Fourier series of a function $g(x)$ with the same property. It remained long in doubt whether a similar result holds if, instead of the square, the pth power ($p > 1$) of $f(x)$ is integrable. This question M. Riesz now answers in the affirmative; in fact there is a constant M depending on p only, such that

$$\int_0^{2\pi} |g(x)|^p dx < M \int_0^{2\pi} |f(x)|^p dx.$$

The proof depends on a simple application of Cauchy's theorem to the complex function of which $f(\theta) + i\, g\,(\theta)$ is the boundary value on the unit circle.

Similar results are obtained for conjugate functions defined over the interval $(-\infty, \infty)$. Such functions are connected by the formulæ

$$g(x) = \frac{1}{\pi}\int_{-\infty}^{\infty} \frac{f(t)}{x-t}\, dt,\quad f(x) = -\frac{1}{\pi}\int_{-\infty}^{\infty} \frac{g(t)}{x-t}\, dt,$$

and Riesz's result is that if one of the integrals

$$\int_{-\infty}^{\infty} |f(x)|^p \, dx, \int_{-\infty}^{\infty} |g(x)|^p \, dx \ (p > 1)$$

is finite, then so is the other.

If we consider the Fourier series of a function, not merely at a particular point, but in the interval $(0, 2\pi)$ as a whole, it is natural to ask under what circumstances the series is convergent almost everywhere in the interval. This question is very difficult, and we are far from being able to answer it completely. It is known that a Fourier series is not necessarily convergent almost everywhere—there is a Fourier series which diverges almost everywhere. It has recently been stated by A. Kolmogoroff and D. Menchoff (" Sur la convergence des séries de fonctions orthogonales," *Math. Zeitschrift*, **26**, 1927, 432–41), that there is a function of integrable square whose Fourier series diverges almost everywhere; but the proof of this is not given. The authors, however, prove that there is a sequence of orthogonal functions $\phi_n(x)$ such that Σa_n^2 is convergent, while $\Sigma a_n \phi_n(x)$ is divergent almost everywhere. The construction of these functions is highly elaborate, and we are left wondering in vain what the Fourier series which has this property may be like. In a still more recent paper (" Sur la divergence de séries de Fourier de fonctions continues," *Comptes Rendus*, Oct. 17, 1927), G. Alexits states that there is a continuous function whose Fourier series diverges in a set of positive measure. Here the proof is given, but it seems to us to be inconclusive. So this interesting subject is left at present in a rather unsatisfactory state.

In " A Note on the Fourier Coefficients of Unbounded Functions," *Journal London Math. Soc.*, **2**, 1927, 151–4, U. S. Haslam-Jones proves that if

$$f(\theta) - \frac{\phi(\theta)}{|\theta|^\nu \{\log |\kappa / \theta|\}^a}$$

where $\phi(\theta)$ is of bounded variation in $(-\pi \ \pi)$, then the behaviour for large values of n of the Fourier cosine coefficients a_n of $f(\theta)$ is given by the asymptotic formula

$$A_n \backsim \pi^{-1}\{\phi(+0) + \phi(-0)\} \Gamma(1 - \nu) \sin (\tfrac{1}{2}\nu\pi) n^{\nu - 1} (\log n)^{-a},$$

and there is a similar formula for the sine coefficients.

A similar result, starting from the coefficients, is given by G. H. Hardy (*Journal London Math. Soc.*, 1928, 12–13). He

proves that if a_n is positive and decreasing, and $a_n \backsim An^{-\alpha}$, where $A > 0$, $0 < \alpha < 1$, when $n \to \infty$, then

$$f(\theta) = \sum_{1}^{\infty} a_n \cos n\theta \backsim A \sin \tfrac{1}{2} \alpha\pi \ \Gamma(1 - \alpha) \ \theta^{\alpha-1}$$

when $\theta \to 0$ through positive values.

A well-known theorem states that any Fourier series may be integrated term by term between any limits. This may not be the case for trigonometrical series which are not Fourier series; but it is so under very general conditions. Prof. E. W. Hobson, in a paper "On the Integration of Trigonometrical Series," *Journal London Math. Soc.*, **2**, 1927, 164–6, proves that a trigonometrical series may be integrated term by term if it converges to an integrable function $f(x)$, except possibly at the points of a set E which is either enumerable, or is unenumerable but contains no perfect set.

S. Pollard ("On the Criteria for the Convergence of a Fourier Series," *Journal London Math. Soc.*, **2**, 1927, 255–62), discusses the relations between the convergence-criteria due to Dini, de la Vallée-Poussin, W. H. Young, and Lebesgue. It is shown in particular that the function considered by de la Vallée-Poussin is the difference between the function considered by Dini, and its mean value.

G. Alexits "Zwei Sätze über Fourier-Koeffizienten," *Math. Zeitschrift*, **27**, 1927, 65–7) obtains the following results: (i) let $f(x)$ be a function with period 2π, and having at most "hebbare" discontinuities *i.e.* such that at any point $f(x+0) = f(x-0)$. Then if its Fourier coefficients a_n, b_n, are such that $na_n \to a$, $nb_n \to b$, it follows that $a = b = 0$. (ii) If $f(x)$ is such that $na_n \to 0$, $nb_n \to 0$ and $f(x+0)$, $f(x-0)$ exist, then $f(x)$ has at most "hebbare" discontinuities.

Interpolation.—Suppose we have a table of values, a_r say, of a function at the points $x = a + rw$ $(r = 0, \pm 1, \pm 2, \ldots)$. Then there are several methods of interpolating between these values to obtain a function $f(x)$, defined for all values of x, and such that $f(a + rw) = a_r$. One such function, $C(x)$, is defined by the formula

$$C(x) = \frac{w}{\pi} \sum_{-\infty}^{\infty} \frac{a_r \sin \frac{\pi}{w}(x - a - rw)}{x - a - rw}.$$

This is called the cardinal function of the table. It was first considered by Whittaker, and by de la Vallée-Poussin. It has the advantage that it is always free from very violent oscillations—in fact if $C(x)$ is analysed into its periodic constituents we get no period less than $2w$.

The following problem now arises. Suppose we perform

a second process of interpolation, starting this time with the values of $C(x)$ at the points $x = rw'$, and from these values form a second cardinal function $C_1(x)$. Is $C_1(x)$ the same as $C(x)$? This question was answered in the affirmative by W. L. Ferrar ("On the Cardinal Function of Interpolation Theory," *Proc. Royal Soc. Edinburgh*, **46**, 1926, 323–33), in the case where $0 < w' < w$, and the series $\Sigma|a_n|/n$ is convergent. This property of the reproduction of an interpolation formula by a second interpolation over a fresh set of values is called the consistency of the formula.

The consistency problem for $w' = w$ is more difficult, but it has now been solved in another paper by Ferrar ("On the Consistency of Cardinal Function Interpolation," *Proc. Royal Soc. Edinburgh*, **47**, 1927, 230–42). There is a curious coincidence in the occurrence of the same formulæ in this problem and in the problems of conjugate functions associated with the theory of Fourier series (see the paper of M. Riesz above referred to). In fact, the consistency problem considered here is much the same as the problem of proving that the conjugate function of a conjugate function is the same as the original function. The method of Ferrar depends on the use of complex integrals and the calculus of residues.

We may note in the same connection a paper by S. Izumi, *Japanese Journal of Math.*, **4**, 1927, 1–6. Here a proof is given of the reciprocal formulæ

$$b_n = \frac{1}{\pi} \sum_{m=-\infty}^{\infty} \frac{a_m}{m+n+\frac{1}{2}}, \quad a_n = \frac{1}{\pi} \sum_{m=-\infty}^{\infty} \frac{b_m}{m+n+\frac{1}{2}}.$$

This is a special case of the above interpolation formulæ.

Integral Functions.—The famous theorem of Picard, that an integral function takes every value, with at most one exception, an infinity of times, seems to be going through endless extensions and ramifications. The latest developments are about what are called "demi-droites de Julia." This means a line on which lies a sequence of points $z_\nu = z_0 R_\nu$ $(R_\nu > 0)$, such that, in the set of circles with centre z_ν and radius $d|z_\nu|$, the function $f(z)$ takes every value with at most two exceptions. It was proved by G. Valiron (*Bull. des Sc. Math.*, **50**, 1926, 168–74), that if a function $f(z)$ is regular and of order ρ in an angle greater than π/ρ, then there is in this angle at least one "demi-droite de Julia." H. Milloux ("Sur le théorie des fonctions entières d'ordre fini," *Comptes Rendus*, Dec. 19, 1927), now obtains the following further results. Let Ω be the magnitude of the smallest angle which includes all "demi-droites de Julia" of an integral function $f(z)$ of finite order $\rho(\rho > \frac{1}{2})$.

(i) Ω is not less than the smaller of π/ρ and $2\pi - \pi/\rho$.

In the remaining results, $\rho > 1$.

(ii) If $\Omega = \pi/\rho$, $f(z)$ is of regular growth in Borel's sense.

(iii) If $\Omega < 2\pi - \pi/\rho$, there are at least two " demi-droites de Julia " such that the exponent of convergence of the zeros of $f(z) - a$, in the direction of each, is equal to ρ, except possibly for one value of a.

(iv) If $f(z)$ has only two " demi-droites de Julia," they make an angle π/ρ.

G. Valiron (" Sur quelques propriétés des fonctions entières," *Comptes Rendus*, Dec. 19, 1927), considers a function $f(z)$ of finite order ρ and regular growth, *i.e.* such that if $M(r, f)$ is the maximum modulus of $f(z)$ for $|z| = r$, then,

$$\lim \frac{\log \log M(r,f)}{\log r} = \rho.$$

If $\rho < \frac{1}{2}$, $\epsilon > 0$, there is a sequence of circles

$$|z| = R_n, \lim \frac{\log R_{n+1}}{\log R_n} = 1,$$

on which

$$\log |f(z)| > R_n^{\rho-\epsilon}.$$

The result is more precise than that of Wiman, which says nothing about the distribution of the numbers R_n. If $\rho \geqslant \frac{1}{2}$ we obtain similar results, but only on certain arcs of circles.

A result which completes those of Milloux noted above is that if $f(z)$ is of order $\rho > 1$, and has only two " demi-droites de Julia," then in any angle which lies between them

$$\log |f(z)| > r^{\rho - \epsilon(r)},$$

where $\epsilon(r) \to 0$ when $r \to \infty$.

On the subject of an integral function F of another integral function f, Valiron obtains the inequality

$$M\{r, F(f)\} > M[(1-\epsilon) M\{(1-\epsilon)r, f\}, F]$$

for every positive ϵ. This is an improvement on Polyá's result (*Journal London Math. Soc.*, **1**, 1925, 12–15), that

$$M\{r, F(f)\} > M \{c\, M(\tfrac{1}{2}r, f), F\}$$

where $0 < c < 1$.

Two functions $G(z)$, $g(z)$, are said to coincide at their a-points if $G(z) - a$ and $g(z) - a$ have the same zeros with the same order of multiplicity. K. Grandjot (*Rendiconti di Palermo*, **52**, 1928, 58–62), gives a new proof of the theorem of Polyá, that if two functions of finite order coincide at their a, b, c, and d-points, where a, b, c, d, are unequal, then the functions are equal.

ASTRONOMY. By W. M. Smart, M.A., D.Sc., Observatory, Cambridge.

The early months of 1928 have been noteworthy for the appearance of three valuable collections of astronomical results. The first is the eagerly awaited *Photographic Atlas of Selected Regions of the Milky Way* (published by the Carnegie Institution of Washington) by the late Prof. E. E. Barnard, to which Prof. E. B. Frost of the Yerkes Observatory contributes an introduction. Vol. i contains fifty-one beautiful photographs (with descriptions) of Milky Way regions, most of which show the obscuring effects of dark nebulosity. Barnard's aim has been to produce a set of photographs of some of the most interesting parts of the Milky Way in such a form that they may be studied towards a better understanding of its general structure. This aim has been most fully realised in the two volumes, the second of which contains charts and tables of the principal stars in the regions concerned.

The second collection of results is vol. xvi of the *Lick Observatory Publications*, containing the measures of the radial velocities of some 2,600 stars, brighter than apparent magnitude 5·5, as determined at Mount Hamilton and at the Lick station in Santiago between 1896 and 1926. In this long-sustained effort no less than 15,000 spectrograms were taken and measured. The introduction contains an investigation into the elements of the solar motion based on this extensive and homogeneous set of radial velocities. The Lick astronomers had determined these elements in 1901 and 1911 from the radial velocity data then available. The right ascension of the solar apex and the velocity of the sun (the value found in 1927 from the results in this volume is 19·6 kilometres per second) show no striking changes with the increasing number of stars available at the different epochs; but this is not so with regard to the declination of the solar apex. The three values derived from the material collected in 1901, 1911, and 1927 are respectively + 20°, + 25°, and + 29°, the latter being very near the result obtained from the proper motion data of the bright stars. It would now appear that the somewhat perplexing difference between the positions of the solar apex calculated from radial velocities and from proper motions has virtually disappeared.

The third collection of results to which reference will be made is Dr. J. Voûte's "Second Catalogue of Radial Velocities," forming vol. iii of *Annalen v. d. Bosscha-Sterrenwacht, Lembang (Java)*. The first catalogue was published by Voûte in 1921. The second catalogue contains the radial velocities of 3,830 stars, 6 novæ, 177 nebulæ, and 19 clusters measured at one

or more of some sixteen observatories. The following table gives the largest velocities observed for stars of the different spectral types, velocities of recession being denoted by (+) and of approach by (−).

Spectral Type.	Km/sec.	Km/sec.
B . . .	+ 101	− 60
A . . .	+ 338	− 350
F . . .	+ 226	− 325
G . . .	+ 307	− 242
K . . .	+ 274	− 162
M . . .	+ 285	− 191
N . . .	+ 39	− 175
R . . .	+ 57	− 383

No. 4 of the *Publications de l'Observatoire National de Prague* is a memoir by V. Nechvile on the proper motions of 3,802 stars. The point of departure of this research is a group of twenty-two photographic plates of different parts of the sky made between 1886 and 1889 by the brothers Henry of the Paris Observatory just before the inauguration of the ambitious and still incomplete project of a systematic photographic survey of the entire heavens. These early plates have been compared with plates of the same regions photographed between 1922 and 1924 with the same instrument ; there is thus the long interval of about thirty-three years for detecting the proper motions of the stars. The old plates had an exposure of about two hours ; the recent plates, about 1½ hours. Stars down to magnitude sixteen have been measured. The author, it is to be remarked, has omitted to discuss the possibility of a magnitude error, the bugbear of investigations of this kind. The probable error of the proper motions in each co-ordinate is the very small one of $\pm 0''.2$ per century. Nechvile gives a list of 402 stars with proper motions exceeding 8″ per century ; this list will be useful to observatories engaged in parallax observations. It is remarkable that only fourteen of these stars are brighter than the ninth magnitude. Then follow diagrams showing the frequency of the proper motions in position angle, for each of the twenty-two regions. These diagrams show up the two star-streams quite conspicuously. Denoting the number of proper motions between μ and $\mu + d\mu$ seconds of arc per century by $f(\mu)d\mu$, Nechvile finds that when μ is greater than 4, the function $f(\mu)$ is extremely well represented by $C\mu^{-2}$. This is the result which emerges from an assumed stellar distribution according to Seeliger's formula a/r. In reaching this statistical formula, Nechvile lumped together the stars of all the twenty-two regions. With a distribution formula of the type just mentioned, this procedure is justified, but with a formula such as Dyson's, namely $\frac{1}{r}e^{-k^2r^2}$,

each region must be considered alone. The trouble about deriving the law of stellar distribution from proper motion data is that the number of proper motions hitherto available in any restricted region of the sky is really inadequate for statistical purposes. Nechvile then proceeds to derive the apices of the two drifts and the apex of the solar motion. His results are generally in agreement with those obtained in similar investigations dealing with faint stars. His value for the declination of the solar apex is + 46°, confirming the value + 44° derived from the Cambridge photographic proper motions. The stars are divided between the two streams in the proportion 4 to 3. The memoir concludes with a careful investigation into the photographic magnitudes of the stars concerned.

In *Monthly Notices, R.A.S.*, **88**, No. 5 (March 1928), Dr. Spencer Jones contributes a valuable discussion of the binary system of Procyon. As in the case of Sirius, the periodic irregularities in the proper motion of Procyon led to the inference that Procyon was in reality a double star, an inference that was not confirmed by visual observations till many years later. The companion is a very faint star, some 13 or 14 magnitudes fainter than Procyon; as the separation is small, the visual observations are of considerable difficulty. Assuming for the moment that the apparent orbit described by the faint companion around Procyon is known, this information combined with the known parallax of the system leads to the value of the sum of the masses of the two components. The parallax of Procyon is large (Procyon in fact is one of the nearest stars, being just 2¼ times more distant than α Centauri, the nearest star of all) and there is little reason to question the accuracy of the parallax value. The period of revolution in the apparent orbit is thirty-nine years, and there is also little doubt concerning the accuracy of this figure. From the periodic irregularities in the observed proper motion of Procyon, the ratio of the masses can be determined; then, from the known sum of the masses obtained from the visual orbit, the mass of Procyon (and of its companion) can be deduced.

The reason which prompted Spencer Jones to a complete rediscussion of the observational data is the not inconsiderable deviation of Procyon from the mass-luminosity curve. In his paper on the "Relation between the Masses and Luminosities of the Stars" (*M.N.*, **84**, 308), Prof. Eddington found that the difference between the observed bolometric magnitude (absolute) and that derived from his theory on the basis of the contemporary value of Procyon's mass was no less than one magnitude. This difference may originate in an erroneous value of the mass.

Spencer Jones's investigation proceeds in three parts. There

is, firstly, the discussion of all available meridian positions of Procyon which, after Auwer's method of treatment, leads to the ratio of the masses of Procyon and its companion. Next, there is a rediscussion of the elements of the apparent orbit. Owing to the circumstances of the case, the inclination of the true orbit to the tangential plane of the celestial sphere is not well determined. The third part deals with observations of the radial velocity of Procyon which are sufficiently sensitive to give a reliable value of the inclination. Then the semi-major axis of the true orbit is deduced and from it and the other known data the sum of the masses of the components. Finally, the mass of Procyon is obtained. When this value of the mass is substituted in the mass-luminosity relation the greater part of the discrepancy between observation and theory disappears.

In *Astronomical Journal*, vol. 28, p. 7, R. E. Wilson contributes a note on the K term in radial velocities. It was shown first by W. W. Campbell in 1911 that the radial velocities of the stars (the radial components due to the solar motion having been deducted) did not show a random distribution; he found, for every part of the sky observed, a systematic velocity of recession which for the B and M spectral types amounted to 4 kms. per second, with smaller values for the intermediate types. Interpreted in terms of these numerical results from the measurement of radial velocities, it appeared, at first sight, that the system of stars was expanding at a rate which depended on spectral class. Campbell pointed out, however, that the existence of the K term might be due to erroneous values of the laboratory wave-lengths used in the reduction of the radial velocities; for example, an average decrease of the wave-lengths used for the B-type stars by 0·06 A would account for the whole of the K term found for these stars. In three papers between 1922 and 1926—*Astrophysical Journal*, 55 (p. 361), (57 (p. 57)), and 63 (p. 277)—S. Albrecht discussed the problem from this point of view and concluded that, as regards the B stars, the systematic errors in the normal wave-lengths could account for a part of the K term, leaving + 2·6 kms. per second still to be explained. Pressure effects and downward convection currents in the stellar atmospheres have been suggested at various times as contributing causes to the phenomenon. In 1915 E. Freundlich proposed as a solution of the K term the relativity displacement of spectrum lines towards the red. Albrecht in 1926 examined this suggestion and showed that the residual velocity of + 2·6 kms. per second in the K term for B-type stars could be interpreted fully as a relativity displacement if the masses of these stars were assumed to be twenty-five times the solar mass with

a density one-tenth that of the sun. For M-type stars the displacement would be comparatively insignificant.

In the paper quoted R. E. Wilson returns to the measurement of the K term for the different spectral classes; for this purpose, the radial velocities of 3,761 stars are available. Grouping the stars in 108 areas, he makes a statistical solution in two ways. The first is by means of the equation

$$V = V_1 \cos \lambda + K,$$

in which V is the average radial velocity of the stars in an area, V_1 the solar motion, and λ the angular distance of the particular area from the solar apex (assumed known). For all the stars, the solution gives 19·6 kms. per second for the value of V_1 and + 3·4 kms. per second for the value of the K term for 384 stars of type Bo – B5, the K term practically disappearing for all the other types. The second solution is made by applying, in the above equation, the mean value of the solar motion found in the first solution. The values of K so found differ very little from the values obtained in the first solution. Thus it would seem that the enigmatic K term disappears almost completely, except for the early B type stars, for which the wave-lengths of the relatively few spectrum lines visible have been shown to be systematically in error, and for which the masses and densities are such that an appreciable displacement due to relativity considerations can be predicted.

In *Astronomical Journal,* vol. 28, p. 56, Messrs. J. J. Nassau and P. D. Wilkins describe a new graphical method of determining the orbital elements of a visual binary. Three graphs are drawn in rectangular co-ordinates. In the first, the observed values of the position angle are plotted against the time; in the second, the observed values of the angular separation are plotted against the time. From the smoothed curves the third curve is obtained, in which angular separation is plotted against position angle. By various graphical devices and the application of certain well-known formulæ, the elements of the true ellipse are obtained. The visual observations of 70 Ophiuchi illustrate the method. The authors point out the several merits of their graphical process, and also one disadvantage under which it labours, namely, that to effect a solution, the complete data for a complete period must, in general, be known.

PHYSICS. By L. F. Bates, B.Sc., Ph.D., F.Inst.P., University College, London.

American Contributions.—R. A. Millikan and G. H. Cameron (*Phys. Rev.*, **31**, p. 163, 1928) contribute a further paper on experiments carried out at high altitudes to test the geographi-

cal, directional, and spectral distribution of penetrating cosmic rays. The experiments were made in the High Andes of Bolivia, and they were designed to test four interesting points. Firstly, it was desired to know whether lakes at high altitudes in the southern hemisphere would give an altitude ionisation curve coincident with that obtained for similar lakes in the northern hemisphere, and the South American lake chosen as most suitable for measurements below water was Lake Miguilla, near Caracoles, Bolivia. This is a snow-fed lake, 125 ft. deep, at an altitude of 15,000 ft., and surrounded by mountains on all sides. The determinations provided very clear evidence that the cosmic rays are independent of geographical position, and hence the possibility that these rays are due to the impact of high-velocity electrons on the outer layers of the atmosphere must be excluded. Secondly, it was important to test the hypothesis, put forward by C. T. R. Wilson, that penetrating cosmic rays are due to the effects of high-velocity electrons generated in thunderstorms passing through the atmosphere. The above-mentioned lake, situated in a deep pocket surrounded by mountains, was completely shielded from electrons due to thunder-storms. The results obtained were the same as those found in areas not screened from thunder-storms. Moreover, sea-level observations, taken on the voyage to Bolivia during heavy thunder-storms, showed no variation in the cosmic ray readings. Thirdly, it was advisable to determine by deep-water readings the zero readings of three new electroscopes of different shapes and sizes, and thus permit the determination of new values of the cosmic ray ionisation at sea-level, for which the values obtained by different observers vary considerably. The new measurements gave a consistent value of 1·4 ions per c.c. per sec. at sea-level. Fourthly, in pockets at very high altitudes the rays are three or four times as intense as at sea-level, and hence it is possible to make reliable tests on the directional effects of the radiation. In particular, two sets of day and night observations, each of three days' duration, carried out at a mining camp at an altitude of 15,400 ft., showed no difference between the radiation incident in the plane of the Milky Way and that incident in a plane normal to it. In this new work the absorption coefficients varied from $\mu = 0{\cdot}25$ to $\mu = 0{\cdot}15$ per metre of water, from which the wave-length of the radiation was calculated from Compton's formula to vary between $5{\cdot}25 \times 10^{-4}$ A and $3{\cdot}2 \times 10^{-4}$ A.

Readers who are working on the collision of electrons with gas molecules will find much to interest them in a paper by Langmuir and Jones (*Phys. Rev.*, **31**, p. 357, 1928). These authors have devised a simple method for the study of ionisation

phenomena with currents so large that the customary methods of examination are useless. The method is based on the fact that when a hot wire cathode is surrounded by a highly ionised gas, we may consider that the main body of the gas is practically field-free, and the potential drop between the cathode and anode is practically confined to a small region—a thin sheath—which covers the surface of the cathode and in which there is a positive space charge. The outer edge of this sheath is well defined, the field of the cathode does not extend beyond it, and it therefore acts as an ideal grid for accelerating the primary electrons from the cathode into the field-free region. In the apparatus of Langmuir and Jones the thin wire cathode was surrounded by a concentric metal cylinder of length equal to that of the wire, and by two insulated circular metal end-plates. The latter could be connected to the cylinder, and either plate or the cylinder could be used as the anode or as the collector. Usually, one end-plate was used as anode to produce strong ionisation of the gas, and the cylinder was used as collector, so that the electrons were projected from the hot cathode in strictly radial directions, and by an examination of the current voltage characteristics for the collector, a good deal of quantitative information on the degree and nature of the ionisation produced in various gases by a stream of electrons of known velocity (50 to 250 volts) could be obtained. It is impossible to give a complete account of their results here, but it may be mentioned that they have developed a theory and have calculated the mean angular deflection for elastic and inelastic collisions between electrons and gas molecules, and they have obtained evidence on the mechanism of ionisation. The current densities of the various Maxwellian groups of electrons and the average temperatures of these groups have been calculated from the data and shown to be in good agreement with data obtained with Langmuir and Mott Smith's probe wire method (*cf.* Sci. Prog., vol. 22, p. 1, 1927). They measured the current density of positive ions by both plane and probe wire collectors, and measured and also calculated the space potential. They obtained evidence in support of negative anode falls of potential, and measured the ions flowing to the anode separately by means of a perforated collector. They measured the maximum number of positive ions produced by an electron with a given velocity before losing its ionising power and showed it to be independent of the current density and pressure, and dependent only on the velocity of the electron and the nature of the gas. In their paper they also give a great deal of information on the mean free path of electrons whose velocity is above that given by the ionisation potential.

English Contributions.—Prof. G. P. Thomson (*Proc. Roy. Soc.*, **117**, p. 600, 1928) describes some very important experiments on the diffraction of electrons. He starts with de Broglie's theory of mechanics, according to which a moving particle behaves as a group of waves whose velocity and wave-length are determined by the speed and mass of the particle. According to de Broglie, if m_0 is the mass of the particle for very slow speeds and v is the speed of a freely moving particle, then the wave-length, λ, is given by $\lambda = \frac{h}{m_0 v}\sqrt{1 - \frac{v^2}{c^2}}$, and the wave velocity, v, is given by $v = c^2/v$, the group velocity being v, the velocity of the particle, and c represents the velocity of light. Thus an electron may be considered as a group of waves, and for electrons which have traversed a fall of potential of 25,000 volts, we find the wave-length calculated on the above formula to be $0{\cdot}75 \times 10^{-9}$ cm., *i.e.* the wave-length is of the same order of magnitude as that of hard X-rays. Consequently, the waves associated with an electron should behave in many respects as hard X-rays. In his experiments, Prof. Thomson sends a beam of approximately homogeneous cathode rays through a very thin film of matter at normal incidence, and he obtains definite diffraction patterns similar to those obtained with X-rays in powder-tube experiments. The film of matter must be so thin that an electron is only once scattered in its passage through the film. Patterns are obtained with aluminium, celluloid, gold and platinum. They consist of a central spot surrounded by a system of concentric rings, and the diameters of these rings are found to be exactly of the magnitude predicted by de Broglie's theory for electrons of the known velocities used in the experiments, and the values of the grating constants of the materials used are in agreement with those found from X-ray data. Sir J. J. Thomson considers these experimental results (*Phil. Mag.*, **5**, p. 191, 1928), and shows that the waves which have thus been shown to exist may be regarded as a consequence of classical dynamics if the electrical charge on an electron is regarded as an assemblage of lines of force starting from the charge and extending into space. Relative displacements of these lines of force result in the emission of waves of electric force.

Prof. T. R. Merton (*Proc. Roy. Soc.*, **117**, p. 542, 1928) describes a new type of striated discharge which he recently discovered in discharge tubes. His tubes were of about 20 mm. bore, and from 20 to 40 cm. long, and they were provided with side-tubes containing phosphorus pentoxide, potassium hydroxide, and potassium permanganate. Carbon electrodes were used, and a palladium regulator was sometimes attached

for the removal of hydrogen. Helium was placed in the tubes at a pressure of 30 to 40 mm. of mercury. In the early stages, the tubes showed the Ångstrom carbon bands, the Swan bands, and the line spectrum of helium, but after prolonged working and heating of the palladium regulator, the above bands were replaced by a spectrum characteristic of the tails of comets and due to carbon monoxide molecules, an intermediate stage with a triplet system of bands being usually observed. A very unstable system of striæ was also sometimes observed. The tubes were capable of attaining a stable condition in which they exhibited an almost uniform green glow, when they were excited by the uncondensed discharge from a high-tension transformer supplied with alternating current at 50 cycles per second. The spectrum of this green glow consisted of the comet tail-bands, a feeble line spectrum of helium, and faint Swan bands. If, under these conditions, the working of a tube was violently disturbed, *e.g.* by the sudden introduction of a condenser and spark-gap into the circuit or by a sudden variation of current, a new type of discharge was started. The glow became irregular, and bright yellow patches were seen to be arranged in a rather irregular spiral along the tube. These patches emitted brilliant helium spectra. On cutting out the condenser and spark-gap, these patches seemed to run together rather quickly, and a symmetrical disc, sometimes 10 mm. thick, was formed. The disc at first wandered about, but ultimately settled down in a fixed position, where it remained for as long as 10 minutes before breaking up with the establishment of the uniform green glow first seen in the tube. If several discs were simultaneously formed they either broke up, or, more frequently, combined to form one single disc. The spectrum of the disc was peculiar in that it showed the comet tail-bands very brightly, but showed no Swan bands. A powerful beam of light was strongly scattered in the neighbourhood of the disc, the scattered light being almost sky-blue in colour, and almost completely polarised. This was undoubtedly due to the presence of small particles of carbon. Examination by a rotating mirror showed the presence of faint, rapidly moving striations throughout the tube. The disc was unaffected by a weak magnetic field. It could be made to travel along the tube with any desired velocity, by connecting a number of accumulators in series with the high-tension alternating current, in order to give the current a bias in a definite direction, the accumulators being connected in the circuit after the disc has been formed. This is an exceedingly remarkable phenomenon, when we remember the high pressure of the gas in the tube. Other gases were used besides helium, but the disc discharge was not established, except on

rare occasions in a neon tube provided with carbon electrodes. The disc seems to require a carbon fog for its formation, and Prof. Merton suggests that the very rare globular lightning discharges which are recorded are examples of single discs descending from a charged cloud to earth. The theoretical discussion of this problem will undoubtedly be awaited with great interest.

The study of pleochroic halos is a very interesting one. As is well known, they represent a kind of radioactive staining of mica produced by a small amount of uranium present in the nucleus of a halo. It is also well known that such halos in biotite disappear on heating for a short time at a dull red heat, and it was somewhat natural to assume that their disappearance was similar to the disappearance on heating of the violet colour in glass which has been produced by exposure to the radiations from radioactive substances. Dr. J. H. J. Poole (*Phil. Mag.*, **5**, p. 132, 1928) has recently examined the effect of heat on such halos. He finds that the halos may always be made to disappear by heat treatment, but they are never reversed, *i.e.* they never become lighter than the surrounding mica, although they sometimes are found reversed in the natural state. He concludes that the progressive darkening of the mica with heat treatment, which results in the loss of contrast between the dark halo and the surrounding mica, is due to loss of water by the mica. He therefore suggests that the X-rays from the radioactive nucleus of the halo produce a similar effect, *i.e.* they decompose the water present in the biotite leaving dehydrated and darkened mica. This of course does not explain the presence of reversed halos, which are presumably due to special causes. In a later note (*Phil. Mag.*, **5**, p. 444, 1928) Dr. Poole points out that an alternative view seems more acceptable, namely, that the oxygen liberated by X-rays in the decomposition of water reduces the colourless ferrous iron present in the mica to the highly coloured ferric state, and that when mica is heated the water present is in the form of a gas well above its critical temperature, and produces a similar oxidation.

The discovery that the band spectra of the halogen salts of the alkaline earths may be very conveniently observed in absorption against a continuous background spectrum, has led G. H. Walters and S. Barratt (*Proc. Roy. Soc.*, **118**, p. 120, 1928) to investigate these spectra thoroughly. They find that by heating excess of the metal with its halogen salt, the band spectrum in absorption can always be produced, and the spectrum is free from oxide bands and other interfering spectra. The experiments are carried out by heating the mixture in a steel tube up to temperatures of 1,000° to 1,200° C., and a

very simple and effective device is adopted to prevent deposition of the vaporised substances on the windows at the ends of the tube. The results show that the band spectra obtained are resonance spectra of molecules of the type MX, which must therefore exist in equilibrium with the metal and its normal salt, MX_2, at temperatures above 1,000° C. A further point of interest is that the CaF band 5292, when examined, under a dispersion of 5Å per mm., appears to have a very different structure in emission and in absorption. In emission it consists of a number of doublet lines with a separation of about 2Å, which conform to a Deslandres formula. In absorption, a large number of fine lines are present in the band, and the fine structure only appears at reduced pressures.

A short time ago, Prof. P. E. Shaw disturbed us by showing how extremely fallacious are many of the dogmatic statements made in connection with frictional electricity. He has continued his work with C. S. Jex (*Proc. Roy. Soc.*, **118**, p. 97, 1928), and they give us the results of rubbing glass rods, prepared in various definite ways, with pure solid elements in an apparatus which ensures constant conditions of pressure and surface during the rubbing. The curves showing the relation between the charge and the amount of rubbing are very interesting. Some elements, Cd, Fe, Pb, Bi, Ag, Cu, Au, Pt, Mg, and W never show negative charge with any type of the glass surfaces used. Other elements, Zn, Sn, Al, Sb, Ni, Co, Te, As, Cr, Tl, and S show an ultimate negative charge after a sufficient amount of rubbing. The influence of layers of acid, alkali, or water on the surface of the glass is very great. In a further communication, Shaw and Jex (*loc. cit.*, p. 108,) give the results of rubbing the elements used in the above experiments with specially cleansed textiles, silk and cotton, wrapped round glass rods. The arrangement of the various elements according as they charge textiles and glass is found closely to correspond with their chemical properties. It is clearly shown that it is impossible to arrange a simple tribo-electric series of one column to include all solids, for the elements Al, Mg, Cr are positively charged when once rubbed under standard conditions with silk and negatively charged when so rubbed with fused glass, and Ni, Zn, Sn, and Co are positively charged when rubbed with silk and negatively charged when rubbed with well-washed glass. Yet silk is always positively charged when rubbed with glass under the above standard conditions. Surface strains play a very important part ; *e.g.* a smooth glass surface rubbed with silks under the above standard conditions is, as we have just seen, negatively charged, but it becomes positively charged when well rubbed, owing to surface strains produced by rubbing, and a matt

glass surface may remain negative to silk even after prolonged rubbing.

German Contributions.—The most important German contribution is probably that made by Sommerfeld (*Zeit. für Phys.* **47**, p. 1, 1928), in which he develops the statistics of Fermi and Dirac, and applies them to the electron theory of metals with such success that this theory is now restored to life and vigour. In fact, other workers have already realised its importance and have made use of it. Thus, R. H. Fowler (*Proc. Roy. Soc.*, **118**, p. 229, 1928) has used the theory to explain the problems pointed out by O. W. Richardson (*Proc. Roy. Soc.*, **117**, p. 719, 1928), namely, that there exists a sharp photoelectric threshold frequency, and that the energy associated with this threshold frequency is equal to the thermionic work function. Sommerfeld shows that the electrons in a metal may be regarded as a perfect gas, but a perfect gas in which the electrons are so concentrated that they are almost tight packed. As a result of the highly degenerate nature of this gas system the contribution of the electrons to the specific heat of the metal is about one-sixtieth of the contribution expected on the older classical theory, so that one of the main objections to the electron theory of metals disappears. The chief distinction between the new statistics and the classical is that to a first approximation the former requires that the mean velocity of the electrons should be independent of the temperature. The new statistics are applied directly to the classical hypotheses of the theory of electrons in metals, and Sommerfeld successfully explains the Wiedemann-Franz law, the Richardson thermionic formula, the emission of electrons by cold metals, and obtains the right order of magnitude for the Thomson coefficient and its variation with temperature. The theory gives, however, the sign of the Hall effect to be always that of bismuth, and is unable to deal with supraconductivity. However, the theory has notable successes to its credit, and we are likely to hear a great deal more of it.

In a long paper (*Ann. der Phys.*, **85**, p. 1, 1928) Karl Kohl examines the method and theory of production of short undamped electric waves by means of thermionic valves. Barkhausen and Kurz (*Phys. Zeit.*, **21**, p. 1, 1920) found that waves less than 1 metre in length could be produced when the plate was negatively charged and the grid positively charged, and they concluded that the vibrations responsible for these waves were due to electrons which moved backwards and forwards through the grid, and, in fact, the mathematical investigation of this idea produced a certain amount of support for this theory; but there existed no satisfactory picture of the production of these electron movements. The shortest waves

produced by Barkhausen and Kurz were 45 cm. in length, and they found the waves to be completely independent of outer circuit connections, but very dependent on the impressed voltage and the filament current, and they observed at times sudden variations in the wave-length and intensity of the waves. Breit (*Phys. Rev.*, **23**, p. 300, 1924) succeeded in producing undamped waves 60 cm. in length when the plate was insulated and the grid positively charged. Now, previous to Kohl's work, short waves produced by reaction methods were of wave-length greater than 1 metre, and the first object of Kohl's research was to develop the shortest possible waves by a reaction method and a high positive plate potential and to determine whether the mode of production of these short waves differed from that of long waves. He came to the conclusion that waves of length down to 75 cm. could be produced in valves with positive plate potentials, if the valve components themselves formed a small oscillatory system. The method for the production of still shorter waves was thus made clear, and he succeeded in producing undamped waves of 60 cm. wave-length in the plate circuit, and of 30 cm. in the grid circuit, by means of specially constructed valves. The time of passage of an electron in the valve had to be of the order of magnitude of the period of the vibration produced, so that the valve possessed a negative resistance, and energy was imparted to the oscillatory circuit by means of induction effects. Kohl is of the opinion that, provided valves can be made to stand the strain, there is no reason why waves of length 10 to 15 cm. should not be obtained.

Krüger, Reinkober and Koch-Holm (*Ann. der Phys.*, **85**, p. 110, 1928) have investigated the residual rays—Reststrahlen—obtained by reflection from mixed crystals. In particular, these workers wished to determine whether the natural frequencies of the two components were affected when they crystallised together, whether the mixed crystals exhibit one definite natural frequency corresponding to a mean value of the two components, or whether several natural frequencies can occur. The pure substances NaCl, KCl, and BbCl were used, together with a mechanical mixture of NaCl and KCl and mixed crystals of NaCl + KCl, KCl + RbCl and KCl + TlCl in a series of different concentrations. The residual rays from these substances were isolated by repeated reflection of the radiation from a Nernst lamp and were examined by a diffraction grating. It was found in the case of the mixed crystals that the residual rays of the two components were not present in the reflected radiation, but each mixed crystal exhibited a definite vibration whose wave-length was between those of the two constituents of the crystal, and which increased as the concentration of the

component with the longer wave-length increased. Within the limits of experimental error, the displacement of the natural frequency of the mixed crystal was proportional to the change in concentration of the two components. Some experimental difficulties were experienced owing to the selective absorption of layers of moisture on the surface of the mixed crystal, and it was found that when a mixed crystal disintegrated, the natural frequency of the mixed crystal gradually disappeared, and the natural frequencies of the pure components appeared in the residual rays.

Some time ago Paschen (*Ann der Phys.*, **71**, pp. 151 and 537, 1923) showed that when a direct current discharge passed from a sputtering aluminium cathode through helium, the first spark spectrum as well as the arc spectrum of aluminium was obtained in the negative glow. Schüler (*Zeit. für Phys.*, **35**, p. 323, 1926), in describing his hollow cathode discharge tube as a source of light, pointed out that collisions of the second kind might occur in these tubes. Frerichs (*Ann. der Phys.*, **85**, 262, 1928) has now used this idea in examining the processes which occur in a mixture of pure rare gases and metal vapours. He first used Paschen's hollow aluminium cathode tube method with helium, neon, and argon, and later extended his observations to the metals Mg, Zn, Cd, and Cu. In addition, he examined the light emitted by the positive column and the light proceeding from a side-tube communicating with that in which the discharge took place. He found that in the glow discharge inside the hollow cathode the energy of the excited rare gas atom more or less accurately represented the amount by which an ion of the metal of the cathode could be excited, *i.e.* in a tube containing argon, the spark spectrum of the metal was limited by terms which required energy corresponding to the first critical potential of argon to be supplied to the ion of the metal, by collision of the second kind between the ion and an excited argon atom. The course of events seemed to be that the rare gas atom was first excited and then gave up its excitation energy to the mixture by means of a collision of the second kind, the excitation energy being imparted either to an atom or to an ion. In the negative glow the ions seemed to acquire this energy the more readily, but the atoms appeared to be the more strongly excited in the positive column.

BIOCHEMISTRY. By R. KEITH CANNAN, M.Sc., University College, London.

The Vitamins. –The interest which has been awakened by certain recent studies in the biochemistry of the vitamins offers a suitable opportunity for a report upon the progress which has attended the work of the past two or three years in this field.

Vitamin A.—Although, at one time, it appeared that we were on a fair way towards the discovery of the chemical nature of this nutritive factor it must now be acknowledged that we seem still to be far from the goal. Claims had, indeed, been made that a material isolated from the unsaponifiable matter of cod-liver oil in the laboratory of the late Dr. Takahashi (cf. this report June 1924) was the pure vitamin. This material which was given the name "biosterin" and the formula $C_{27}H_{42}(OH)_2$ has now been further examined in the same laboratory (Nakamiya and Kawakimi, *Inst. Chem. Research* (*Japan*), 1927, **3**, 62, and **7**, 121). Upon hydrogenation, an operation which destroys the growth-promoting factor, there were isolated batyl alcohol, nonacosane, myricyl alcohol, an unknown unsaturated alcohol and some octadecyl palmitate. On the face of it, there seems to be little doubt that "biosterin" is, therefore, a product similar to that obtained by Drummond from cod-liver oil by similar methods (this report, January 1925) and which was demonstrated to be a complex mixture of unsaturated alcohols and hydrocarbons.

Meanwhile evidence is accumulating that the great reputation of cod-liver oil as a rich source of vitamin A is a reputation which has been too easily won, for it now appears that the liver oils of certain other fish may exceed cod-liver oil one hundredfold in potency, whilst the liver oils of herbivorous animals such as the ox and sheep, and even the oil of human liver, may be ten times as active as a good sample of cod-liver oil (Rosenheim and Webster, *Nature*, 1927, **120**, 440; *Biochem. J.*, 1927, **21**, 111; Wilson, *Biochem. J.*, 1927, **21**, 1054). The industrial significance of this in respect of the enrichment of foods, notably margarine, with inoffensive sources of vitamin A is obvious. This extensive biological survey by Rosenheim and Webster has incidentally assembled a great deal of data in support of the view that the colour reactions described by Rosenheim and Drummond (*Biochem. J.*, 1925, **19**, 753), and believed by them to be due specifically to vitamin A, are indeed to be attributed to it and not to other substances present in the vitamin preparations. Further work by Carr and Price (*Biochem. J.*, 1926, **20**, 497) and by Rosenheim (*Biochem. J.*, 1927, **21**, 386) has elaborated the test and has sought to inquire into its mechanism. Another colour test has been described by Fearon which, at first sight, had many advantages for quantitative work. It appears now to be generally agreed that this test is not due to the vitamin at all but to another substance frequently but not necessarily present in natural fats. This substance, unlike the vitamin, passes, on saponification of the fat, into the fatty acid fraction.

Vitamin D.—It is now a matter of wide knowledge that

anti-rachitic properties may be induced in highly purified cholesterol by irradiation with ultra-violet light (this report, July 1925). The exploitation of this observation has brought us very close to the discovery of the chemical nature of vitamin D. Rosenheim and Webster (*Biochem. J.*, 1926, **20**, 537) were able to remove the inactivated cholesterol from the irradiated material by precipitation with digitonin, thus attaining a considerable concentration of the anti-rachitic activity such that 0·01 mg. of their material per diem was a physiologically protective dose for a rat. The same workers (*J. Soc. Chem. Ind.*, 1926, **45**, 932) demonstrated that cholesterol, which had been regenerated from a specimen of its dibromide, could not be activated by ultra-violet light. This observation led them to the conclusion that there was present in all their " highly purified samples of cholesterol " some impurity which was the true precursor of the vitamin and which was irreversibly destroyed by bromination of the cholesterol. The fact that it was not possible to activate cholesterol if oxidative changes were allowed to occur during the process also pointed to the labile character of the provitamin (Hess, Weinstock, and Sherman, *J. Biol. Chem.*, 1926, **70**, 123). Meanwhile entirely independent evidence was marching to an identical conclusion. Heilbron, Kamm, and Morton (*J. Soc. Chem. Ind.*, 1926, **45**, 932) observed that the ultra-violet absorption spectrum of cholesterol appeared to be a composite one. They succeeded in separating their " pure cholesterol " into two fractions one of which—true cholesterol—gave only general absorption in the ultra-violet, the other giving three well-defined bands. Upon irradiation of the latter sample these absorption bands disappeared and, simultaneously, anti-rachitic properties appeared. While this work was proceeding Windaus and Hess (*Nach. Ges. Wiss. Göttingen*, 1927, **175**, 84) and Rosenheim and Webster (*Biochem. J.*, 1927, **21**, 78) were making a systematic study of the effect of irradiating a large number of substances related to cholesterol. Amongst these they tried the highly labile triply unsaturated sterol, ergosterol, $C_{27}H_{42}O$. When this was irradiated there resulted a very active product whose protective dose for rats was as low as 10^{-4} mg. per diem. That is to say, 1 mg. of this material was equivalent in anti-rachitic activity to about 200 c.c. of a good sample of cod-liver oil. In view of this potency and of the fact that the absorption spectrum of ergosterol is closely similar to that of the substance separated from cholesterol by Heilbron and his associates and in view of the sensitivity of ergosterol to oxidation both Windaus and Rosenheim agree in concluding that ergosterol is the true provitamin D. It is interesting, and important from the point of view of hygiene and the curative effect of sunlight treatment in rickets, that the longer wave

radiations of solar ultra-violet light are capable of activating ergosterol. It will be remembered that there is present in the skin a high concentration of cholesterol and, presumably therefore, important amounts of ergosterol.

Jendrassik and Kaményffi (*Biochem. Z.*, 1927, **189**, 180) differ from the conclusions of the above authors in certain important particulars. They claim to have succeeded in activating cholesterol which has been regenerated from its dibromide and, further, have repeatedly activated cholesterol after removal of the active fraction at each activation by washing and recrystallisation. Jendrassik and Keményffi come to the remarkable conclusion that although cholesterol is not itself the true provitamin, it does give rise to it on contact with water. When the last trace of water is removed from cholesterol they find the product is incapable of activation by means of ultra-violet light. The reconciliation of these conclusions with those of Windaus and Rosenheim should go some way towards elucidating the exact chemical nature of vitamin D and of its precursor substance.

Vitamin B.—The accessory food factor which has hitherto been described as vitamin B is now generally thought to consist of two substances having distinct chemical and physiological properties. In this respect the water-soluble vitamin is following out the history of the original fat-soluble A. The physiological properties which we have learnt to associate with the composite vitamin B are growth-promoting properties and protective properties against avain polyneuritis, human beriberi and human pellagra. Chick and Roscoe (*Biochem. J.*, 1927, **21**, 698) ascribe these characteristics to two distinct factors which they describe as the "antineuritic" (preventing polyneuritis in birds, paralysis in rats and beri-beri in man) and the "pellagra-preventive" (necessary for growth and preventing pellagra in man). Reviewing the literature, they indicate that there are notable differences in the natural distribution of these two factors. Thus the antineuritic factor is predominant in the wheat embryo, whereas in milk, meat, and green leaves there is little of this factor but much of the growth-promoting vitamin. There are, moreover, distinct differences in physical properties, notably in solubility in organic solvents, in facility of adsorption and in stability to heat. An extract of yeast submitted to a temperature of 120° C. for four hours suffers destruction of almost all its antineuritic properties, whereas it remains a potent source of the growth-promoting vitamin. Observations of Goldberger (U.S. *Public Health Rep.*, 1924, **39**, 87; 1925, **40**, 54; 1926, **41**, 297 and 1025) and of Hassan and Drummond (*Biochem. J.*, 1927, **21**, 653) on the distinctive accessory nutritive properties of autoclaved yeast

and of alcoholic extracts of yeast point again in the direction of the composite character of vitamin B. The general agreement in this conclusion is witnessed by the concern of the Accessory Food Factors Joint Committee of the Medical Research Council and the Lister Institute to find a nomenclature suited to these developments. Their tentative suggestions deserve quotation as they appear to be the simplest way out of the present difficulty and, at the same time, are suggestive of a general plan for dealing with a like problem should it arise—as it is likely to do—in respect of other vitamins.

The suggestions of the Committee are as follows: (1) The name "vitamin B" should be retained to describe the group of water-soluble vitamins to which it was first applied by McCollum and Davies in 1915. (2) The name "vitamin B_1" should be employed to describe the more heat labile antineuritic vitamin required to prevent polyneuritis in birds, marasmus in mammals, and beri-beri in man. (3) "Vitamin B_2" should be used to designate the more heat stable component necessary for the maintenance of growth and health and the prevention of pellagra in man and certain characteristic skin lesions in rats. Finally (4) the name "bios" should be retained to describe the factor responsible for encouraging the rapid growth of yeast cells. Adopting this classification, it may now be reported that progress has been made in the purification of vitamin B_1 in England, in Holland, and in Japan (Kinnersley and Peters, *Biochem. J.*, 1927, **21**, 777; Jansen and Donath, *Proc. K. Akad. Wetensch. Amsterdam*, 1926, **29**, 1390 and 1927, **30**, 376; *Suzuki and Sahashi. Sci. Papers. Inst. Chem. Research*, 1926, **4**, 295, and 1927, **5**, 191). Kinnersley and Peters have been engaged upon the purification of a yeast extract—which they call torulin—to the point that 0·15 to 0·30 mg. per diem is a curative dose for polyneuritis in pigeons. Their method consists in removal of impurities by successive precipitations with lead acetate, mercuric sulphate, and baryta and then adsorption of the vitamin on and elution from "norite" charcoal under special conditions. The material obtained by the Japanese workers has been given the name "Oryzyanin." It is curative in pigeons in a dose of 5 mg. When boiled with dilute acids it yields glucose, choline, nicotinic acid, and 2·6 dioxychinolincarboxylic acid. The latter compound is said to stimulate growth of yeast, whilst the related substance 2·6 dioxychinolin hydrochloride is reported to cure avain polyneuritis. Jansen and Donath have obtained from 100 kilo. of rice polishings 1·4 gm. of a substance containing 25 per cent. of the original antineuritic activity of the polishings. It is said to be a base with the empirical formula $C_6H_{10}ON_2$

and is thought to contain a glyoxaline ring. It is to be noted, however, in the latter connection that Kinnersley and Peters observe that the Pauly reaction which is given by torulin becomes progressively diminished as further purification proceeds. With the experience of the purification of vitamin D before us we must follow with extreme caution these tentative feelings towards the identification of the chemical nature of a vitamin whilst it is still contaminated with much inert allied material.

The question of the nature of the physiological rôle played by vitamin B has been experimentally reviewed during the year. In particular the view, contributed to from various but elaborated in particular by Abderhalden and Hess sources (*Z. Physiol. Chem.*, 1921, **117**, 284) that the vitamin plays an essential part in the oxidative mechanisms of the tissues, has been criticised. Drummond and Marrian (*Biochem. J.*, 1926, **20**, 1229) and Drummond and Kon (*Biochem. J.*, 1927, **21**, 632) arrive at the conclusion that there is no evidence for an impaired oxidative capacity in vitamin B deficient animals other than that which is always associated with extreme conditions of inanition. Indeed starvation appears to play a large part in the events following lack of this vitamin. It will be remembered that Mendel and Cowgill have emphasised a curious association of vitamin B with the phenomenon of appetite. No significant difference was found between the nerve lesions in the deficient animals and those which are produced by inanition (Lavrov and Matsko, *J. de Biol. et de Med. Exp.*, 1926, **9**, 71). The cause of the nerve lesions are obscure. They can only be slight, since injection of a vitamin preparation restores the animal to a normal condition in a few hours. A suggestive line of inquiry, which, however, has not yet developed into any clear conclusion, is that the vitamin requirement is governed by the amount of food taken and by the ratio of protein, carbohydrate, and fat in the diet (Drummond and others, *Boichem. J.*, 1926, **20**, 1256; 1927, **21**, 653; Boas, *ibid.*, 1927, **21**, 712), and to the total calorific value of the food (Plimmer, Rosedale, and Raymond, *ibid.*, 1927, **21**, 913).

Vitamins C and E.—Little has been added during the past two years to our knowledge of the biochemistry of the antiscorbutic vitamin C or of the antisterility vitamin E. Bezssonoff (*Compt. rend.*, 1926, **183**, 1309) suggests that vitamin C is a complex of two factors differing in heat stability, but the suggestion has not been developed far. Hoyle and Zilva (*Biochem. J.*, 1927, **21**, 1121) have continued the study of the reducing properties of decitrated lemon juice in relation to the conditions which determine the stability of the vitamin. With regard to vitamin E, Sure (*J. Biol. Chem.*, 1927, **74**, 37

and 71) reports the occurrence of sterility or diminished fertility in rats in which cod-liver oil was the sole source of fat and fat-soluble vitamins. The addition of 10 per cent. of butter or of 0·035 per cent. of the unsaponifiable matter from cotton-seed oil served to restore fertility. Hartwell (*Biochem. J.*, 1927, **21**, 1076) confirms the value of butter and the inadequacy of cod-liver oil. Simmonds, Becker, and McCollum (*J. Amer. Med. Asoc.*, 1927, **88**, 1047), on the other hand, report that liver oils are rich in this vitamin. The latter workers are of opinion that lack of vitamin E leads to death of the fœtus through a defect in iron assimilation. This may be mitigated by administration of ferrous citrate.

Enzymes.—It was reported a few years ago in this review that R. Willstatter was developing a massed attack upon the problem of the chemical nature of enzymes and their actions. Since the inception of this work the output from this laboratory has been prodigious and its significance fundamental. The work has proceeded from a laborious experimental survey of the technique and principles of the problem to a comprehensive definition of the subject which constitutes a great contribution to biochemistry. In a recent lecture to the Chemical Society of this country (*J. Chem. Soc.*, 1927, 1359), Willstatter has summarised the main contributions of his laboratory and outlined his present views upon the nature of enzymes and their activity. The lecture will be widely read.

In the first place, Willstatter's laboratory has been responsible for the development of a variety of new methods of purification of enzymes which, whilst they have failed in their quest of " a pure enzyme," have enabled a degree of separation and characterisation of enzymes without which many fundamental observations on their behaviour could not have been made. The principle whose exploitation has led to the greatest success is that of the fractional adsorption of the enzymic activity and its fractional elution from a variety of adsorbent materials, *e.g.* alumina, kaolin, tristearin, lead phosphate, etc. The advantage of this method lies in the high degree of specificity evidenced in adsorptive processes and in the small amounts of affinity involved in the combination with consequent small risk of irreversibly inactivating the enzyme. Incidentally it has been shown that the adsorption reactions of a particular enzyme vary with the degree of purity of the preparation. They are therefore the properties of the contaminating material rather than of the actual catalytic structure. This conclusion must question the validity of the argument of Michaelis and of Sorensen, who have sought to draw deductions as to the ionic state of the enzyme by observations of the electric charge carried by the surfaces which adsorb the enzyme. Willstatter

believes that the majority of the properties which are said to characterise an enzyme—*e.g.* optimum hydrogen-ion concentration and optimum temperature for activity, behaviour towards electrolytes, colloidal character, etc.—are really to be attributed to colloidal material of a non-catalytic nature which is necessarily associated with the true catalyst.

Amongst the notable successes following the employment of the adsorption technique has been the complete separation of the various enzymes in a pancreatic preparation (Waldschmidt-Leitz and Harteneck, *Z. Physiol. Chem.*, 1925, **149**, 203). The lipase and the erepsin are adsorbed by a special form of alumina known as α alumina and the lipase is then removed by washing with weak alkaline phosphate solution. The erepsin comes out with acid phosphate. After removal of these enzymes the extract is next treated with kaolin or β alumina. The trypsin is thus adsorbed and the amylase is left behind. Suitable refractionations complete the separations. Waldschmidt-Leitz (*Z. Physiol. Chem.*, 1927, **166**, 241) reports that it is possible to separate completely trypsin from enterokinase by means of a form of alumina described as AlO_2H even after the trypsinogen has been activated by the kinase. An example of the efficiency of the adsorptive technique is given in the records of a purification of yeast invertase. The whole of the enzyme in 14 kilo. of fresh yeast was at one stage adsorbed by 1 gm. of alumina. The adsorbate weighed 2·5 gm.

It has been said that, in spite of the refinements which have been developed in methods of purification, no pure enzymes have yet been prepared. On the contrary, the more promising cases in the literature have lost their promise in the more searching light of recent work. The view which was at one time thought to be most plausible that enzymes partook of the nature of proteins has now been shown to be definitely untrue in the cases of lipase, invertase, and peroxidase, which have been prepared in a high state of activity although entirely free from all recognisable traces of protein. The work of Willstatter and Stoll on the purification of peroxidase had progressed so far a few years ago that the most active preparations could be described as consisting chiefly of a nitrogenous glucoside and containing 0·5 per cent. of iron. Further purification has now led to a further fivefold increase in the activity of the preparation accompanied by almost complete elimination of carbohydrate and a diminution of the iron content to 0·01 per cent. Another disappointment is represented by invertase. Euler had reported very good grounds for the belief that phosphorus was an essential and important constituent of this enzyme. Willstatter now states that with the aid of a series of fractional adsorptions invertase preparations have been secured

far exceeding those of Euler in activity yet in which the concentration of phosphorus was below 0·06 per cent. In view of this destructive evidence to what conclusion can we come as to the chemical nature of enzymes? The conclusion to which Willstatter (*Ber.*, 1926, 59 B.1) has been led is that an enzyme is a composite system comprising a specific catalytic molecule or group associated with a non-specific catalytically inactive colloidal carrier. The latter may, in part, be separated from the catalyst though the enzyme then becomes highly unstable. The nature of the colloid probably influences the activity and specificity of the catalyst. In this view Willstatter will find most enzyme chemists inclined to agree. In its obvious analogy with surface catalysts in common laboratory and industrial employment it emphasises the heterogeneous character of enzyme action. Enzyme action is a surface phenomenon and the behaviour of enzymes is conditioned by the peculiarities pertaining at interfaces. Development of the problem along the lines which have proved so successful in elucidating the activity of artificial surface catalysts promises to be the best method of learning more of the nature of the behaviour of nature's catalysts.

One of the most intriguing problems in enzyme chemistry has been the high order of specificity exhibited by enzymes, extending even to complete discrimination between optically isomeric structures. This has always been difficult to understand on the theory of intermediate compound formation between enzyme and substrate or on the theory of adsorption compound formation as long as the latter were regarded as determined by physical forces. Now that we comprehend surface forces as chemical—in terms of the valencies of the surface groups—we can have a proper appreciation of the possibilities of surface orientation and, therefore, of specific adsorptive processes expressed in differences in chemical structure. Kuhn (*Z. Physiol. Chem.*, 1925, **150**, 220; 1927, **168**, 1 Ber, 1926, 59B, 1655) has brought to light a number of extremely interesting examples of this possibility in enzyme chemistry. The invertase derived from Aspergillus is inhibited, like most enzymes, by the products of its activity. But it is only one of the products of hydrolysis of sucrose which is inhibitory, viz. dextrose. Fructose is without effect. In the case of yeast invertase, on the other hand, fructose inhibits the enzyme but dextrose does not. This is clearly not a simple case of displacement of the reacting molecule from the catalytic surface by the products of the reaction. It is rather a case of specific poisoning, *i.e.* the occupation of the active spots on the catalytic surface by the poisoning molecules. The simplest explanation seems to be to assume that Aspergillus invertase

forms an adsorption compound with sucrose by attachment to the dextrose portion of the molecule. Yeast invertase will therefore be regarded as combining with the fructose end of the cane sugar molecule. Striking confirmation of this view derives from the effects of these enzymes on certain trisaccharides. Aspergillus invertase is able to hydrolyse melezitose but not raffinose, whereas yeast invertase succeeds where the other enzyme fails and fails where it succeeds. The structures of these two trisaccharides may be written :

melezitose	dextrose-fructose-dextrose,
raffinose	galactose-dextrose-fructose.

On the assumption that Aspergillus attaches itself to the dextrose molecule and yeast invertase to the fructose molecule, we have an immediate explanation in terms of structure of the specific activity of these two enzymes in the cases quoted. The reason lies in the accessibility of the point of attachment of the catalyst and the substrate molecule. In a word, we perceive that the onus for the specificity of enzyme action is placed as much upon the peculiar structure of the reacting molecule as on the structure of the catalyst molecule. The old lock-and-key simile of E. Fischer materialises in terms of chemical structures.

PHYSICAL CHEMISTRY. By R. K. Schofield, M.A., Ph.D. (Cantab), University, Durham.

The Electronic Theory of Valency.—A feature of the recent developments in the electronic valency has been the use of physical methods to distinguish the different types of linkage. Significant strides have been made by Sugden in the use of molecular volumes in elucidating molecular structure. The variation of molecular volume with temperature has always been a difficulty, and the use of the boiling point as a standard state does not lead to satisfactory results. Sugden has recently proposed two new methods. In the first (*J.C.S.*, 1924, **125**, 1185) the surface tension is used to define the standard state, and a quantity $P = M\gamma^{\frac{1}{4}}/(D - d)$ (where M is the molecular weight, γ the surface tension, D the density of the liquid and d that of the saturated vapour) is obtained to which the name "parachor" is given. In the second method (*J.C.S.*, 1927, p. 1780) the formula $V_0 = M(1 - T/T_c)^{0 \cdot 3}/(D - d)$, (in which T_c is the critical temperature) is used to extrapolate the molecular volume to the absolute zero. V_0 is called the "zero volume." Neither relation has any theoretical basis, yet for all non-associated liquids so far examined, the values of both the parachor and the zero volume are very nearly independent of

temperature. In the case of the zero volume this is also true of at least some associated liquids at sufficiently low temperatures. Both the parachor and the zero volume are found to be expressible to a remarkable degree of accuracy (usually to 1 or 2 per cent.) as a sum of a series of atomic and structural constants, some of which are given below :

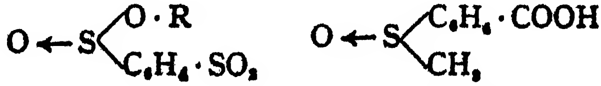
	P	V_0		P	V_0		P	V_0
H .	17·1	6·7	P .	37·7	12·7	Double-bond .	23·2	8·0
C .	4·8	1·1	S .	48·2	14·3	Triple-bond .	46·4	15·0
N .	12·5	3·6	Cl .	54·3	19·3	3-ring . .	17·0	4·8
O .	20·0	5·0	Br .	68·0	22·1	4-ring . .	11·6	3·2
F .	25·7	10·3	I .	91·0	28·3	5-ring . .	8·5	1·8
Si .	12·0		O (in alcohols)		3·0	6-ring . .	6·1	0·6
			N (in amines)		0·0	Semi-polar bond	1·6	0·0

No constants of a single-bond appear in the list, since the atomic constants are arrived at from a consideration of compounds which by common consent are believed to be held together entirely by single-bonds. For instance, the difference between the parachors of successive hydrocarbons gives the parachor for $-CH_2-$. The difference between the parachor of C_nH_{2n-2} and *n* times that of $-CH_2-$ gives twice the parachor of hydrogen, while that of $-CH_2-$ less twice that of hydrogen gives the parachor of carbon. The sum of the atomic parachors is found to give directly the molecular parachor only when all the bonds are single, for every double-bond in which one of the atoms involved is carbon 23·3 must be added and for every triple-bond approximately double this amount. This is true whether the second atom is nitrogen, sulphur, oxygen, or another carbon atom. The parachors of the substances, in the molecules of which the older structural chemistry would indicate a double-bond between an oxygen atom and an atom other than carbon (*e.g.* nitrogen, phosphorus, or sulphur), fall sharply into two classes. In one the double-bond has the same influence as with carbon, while in the other its influence is either imperceptible or causes at most a slight reduction in the parachor. Exactly the same phenomenon is exhibited by the zero volume. This accords most satisfactorily with G. N. Lewis's theory of electronic octet formation by electron-sharing. A carbon atom has four valency electrons, and so can only complete its octet by forming four normal covalent bonds, thereby obtaining a share in four more electrons. In cases where it gives a share in two of its electrons to the same atom, it must also receive from that atom a share in two more. This link by four shared electrons is presumably the normal or non-polar double-bond which gives rise to the increase of 23·3 units in the parachor and 15·5 units in the zero volume. A nitrogen or phosphorus atom only requires a share in three more elec-

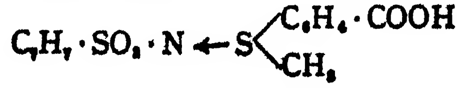
trons to complete its octet, and can obtain this either by forming three single bonds or one single-bond and one (non-polar) double-bond. There remain two electrons of the octet unshared, and a link with an oxygen atom (which requires two electrons to complete its octet) can be effected by sharing this "lone pair" with it. In forming this last link the nitrogen or phosphorus atom takes no share in any electrons originally belonging to the oxygen; it requires none, since its octet is already complete. This linkage is by *two* shared electrons, and thus resembles the single-bond much more than the double-bond. It only differs from a single-bond in the electrical polarity caused by both shared electrons having come from one atom. The fact that whenever Lewis's theory indicates that a bond, formerly considered to be double, is of this kind, no double-bond is indicated by either parachor or zero volume, is therefore most satisfactory. Sugden uses the term "semi-polar double-bond" to distinguish the new linkage, but since it is due to the sharing of two not four electrons the word "double" is misleading. The term "co-ordinated covalency" is preferred by Sidgwick, who depicts this form of linkage in structural formulæ by an arrow pointing in the direction in which the two electrons have been transferred. Sugden's values confirm the idea that sulphur, after completing its octet by forming two normal covalencies, can then enter into a co-ordinated covalency with one or two oxygen atoms, while chlorine, after forming one normal covalency, can similarly co-ordinate with three oxygen atoms.

In a recent paper Sugden and Wilkins (*J.C.S.*, 1927, p. 2517) put forward the view that the structural constants affecting the parachor have values proportional (to a first approximation) to the degree of unsaturation as given by the ratio of the latent valencies to the number of octets involved. Sidgwick believes that with elements of atomic number higher than 10 the octet can in favourable circumstances expand to 10 or 12, as in PCl_5 and $K_2[SiF_6]$. Sugden considers the parachor values so far examined to be at variance with this idea, and favours the view that the octet is preserved even though this involves linkage by one shared electron.

The distinction between a true double-bond and a co-ordinated covalency is supported by work in other fields. The substances:

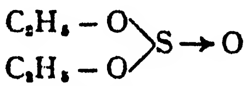


and

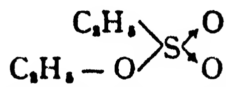


have been resolved into optically active enantiomorphs by H. Phillips and collaborators (*J.C.S.*, 1925, **127**, 2552; 1926, p. 2079; and 1927, p. 188). Molecular asymmetry is not indicated by the older formulæ in which the co-ordinated covalency would be written as a double-bond. The existence of optical activity seems only explicable on the view that the structure is as shown above with only one electron-pair linking the sulphur to the oxygen or nitrogen, and that the sulphur has a "lone pair" whose orbits form with the three attached groups a permanent tetrahedral arrangement.

Sidgwick (*Electronic Theory of Valency*, pp. 123–4) shows that evidence for the existence of co-ordinated covalency is afforded by the increased dielectric constant and decreased volatility of substances in which they occur. Thus:



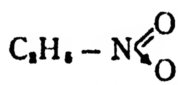
Diethyl sulphite
Dielectric constant 16



Ethyl ethanesulphonate
Dielectric constant 42

$C_2H_5 - O - N = O$

Ethyl nitrite
Boiling-pt. 17° C.



Nitroethane
Boiling-pt. 114° C.

The increased dielectric constant is a direct result of the electric polarity introduced by the co-ordinated link, while the decreased volatility is an outcome of the increased cohesion caused by the electrostatic field in the neighbourhood of the bond.

In his original treatment G. N. Lewis considered that covalency in which two electrons lie midway between the linked atoms may pass by gradations into electrovalency in which both are associated with one of them. This idea was based on the earlier static view of the atom, and Sidgwick (*loc. cit.*, p. 83) regards it as incompatible with the dynamical view now universally accepted. He writes: ". . . if we completely accept the implications of the Bohr atom, in which the electrons are moving in quantised orbits, it would seem to follow that a linkage must be either of one form or the other, and that the transition must involve a quantum change; although in some cases this change may very well be brought about by a change of conditions, as for example of solvent, and there may even be substances which consist, in the liquid or gaseous state, of a mixture of molecules of the two forms in equilibrium."

A number of methods have recently been introduced to distinguish substances which are ionised from those which are

bound together entirely by covalent bonds. Thus Sidgwick (*loc. cit.*, p. 88) cites the following boiling points :

SiF_4	$SiCl_4$	$SiBr_4$	SiI_4
− 90°	57°	153°	290°
NaF	NaCl	NaBr	NaI
1695°	1441°	1393°	1300°
AlF_3	$AlCl_3$	$AlBr_3$	AlI_3
800°	183°	260°	350°

The lower values for the halides of silicon indicate covalency, the increase from left to right being due to increase in molecular weight. The very high boiling-point of the sodium halides is evidence that the liquid is a mixture of sodium and halide ions. In this case the decrease in passing from left to right is attributed to the decrease in electrostatic potential of the anion, due to increase in size, the effect of which in decreasing the cohesion more than counterbalances that of the increase in molecular weight. The case of aluminium is interesting. The fluoride is evidently ionised, but the others are covalent.

Blitz (*Z. anorg. Chem.*, 1924, **133**, 312 ; and 1926, **152**, 267) has collected data regarding the electrical conductivities of a large number of fused chlorides. Of sixty compounds, thirty-one have conductivities between 10 and 0·1 ohm^{-1} cm. at their melting-points, while the values for twenty-six are less than $2 \cdot 10^{-6}$. Only three, $BeCl_2$, $ZnCl_2$, and Hg_2Cl_2, lie in between. This sharp division is in good accord with Sidgwick's contention quoted above.

The elucidation of crystal structure by X-ray analysis is likely to give valuable information about molecular structure, but the interpretation of the data from this standpoint is only beginning. Grimm and Sommerfeld (*Z. f. Physik.*, 1926, **36**, 36) have classified the crystal structure of compounds of the type AB, in which one atom of A and one of B have eight valency electrons between them. Four types are found which are typified in sodium chloride, cæsium chloride, diamond and wurtzite (ZnS). In the first, six, and in the second, eight ions of one sign are symmetrically distributed round one of the opposite sign. In these cases they consider that the valency electrons form an octet round the anion, and that the ions are held together solely by the electrostatic forces between them. In the other two types each atom of A has four atoms of B arranged round it at the points of a tetrahedron, and *vice versa*. In these cases they consider that the valency electrons are all shared, so that these crystals are in reality " giant molecules."

Thus physical methods are again proving to be of great value in the new attack which is being made upon the fundamental problem of chemistry. It is also true to say that these

new ideas of the mechanism of chemical combination are throwing new light on a number of physico-chemical problems Their application is proving particularly successful in dealing with the problems of strong and weak electrolytes and of molecular association. There is not space here to give an account of these recent developments, but the reader can confidently be referred to Dr. Sidgwick's book already cited, which was published last year by the Clarendon Press, and in which an admirable account of these new phases of the subject is to be found.

THE HISTORY OF CHEMISTRY. By E. J. HOLMYARD, M.A., M.Sc., F.I.C., Clifton College.

GENERAL.—A *History of Science Society* was organised in Boston, Mass., on January 12, 1924, and incorporated under the laws of the District of Columbia on January 30, 1925, with a total of 505 Foundation Members. The Assistant Secretary, to whom applications for membership should be addressed, is Dr. F. E. Brasch, Smithsonian Division, Library of Congress, Washington, D.C., U.S.A. The annual subscription is one guinea. Members receive free of charge the official organ of the Society, *Isis*, which is an international review devoted to the history of science and civilisation. It contains articles, reviews, etc., and gives also a very useful bibliography from time to time.

Besides *Isis*, the following journals are wholly or partly devoted to the history of science : *Archeion* (*Archivio di Storia della Scienza*), edited by Prof. Aldo Mieli and published by the Casa Editrice Leonardo da Vinci, Via Casalmonferrato 29, Roma (40) ; *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften*, edited by Profs. Haberling, Wieleitner, and Zaunick for the *Deutschen Gesellschaft für Geschichte der Medizin und der Naturwissenschaften* (Leipzig, Leopold Voss) ; *Archiv für Geschichte der Mathematik, der Naturwissenschaften und der Technik* (Leipzig, F. C. W. Vogel) ; *Beiträge zur Geschichte der Technik und Industrie* (Berlin, VDI-Verlag) ; *Revue de Synthèse Historique* (Paris, La Renaissance du Livre) ; *Geschichtsblätter für Technik, Industrie und Gewerbe*, edited by Graf Karl von Klinckowstroem and Franz M. Feldhaus (Berlin, Tempelhof).

OBITUARY.—Johann Ludwig Heiberg, the historian of mathematics and physics, died at Copenhagen on January 4, 1928, at the age of seventy-four. His chief work was the publication of Greek mathematical texts, but his little book *Mathematics and Physical Science in Classical Antiquity* (Oxford University Press, 1922) made his name familiar to a wide circle of readers.

Eilhard Wiedemann was born on August 1, 1852, at Berlin,

and died on January 7, 1928, at Erlangen. For the last thirty years he had been almost constantly engaged upon the history of science and technology in medieval Islām, and had published numerous papers on this subject. He was the son of Gustav Wiedemann, the physicist, and the grandson of Mitscherlich. Since 1886 he had been director of the Physical Institute of the University of Erlangen.

Antiquity.—Sir Flinders Petrie has published an interesting and important monograph on *Ancient Weights and Measures illustrated by the Egyptian Collection in University College, London.* After observing that the whole subject of ancient metrology "has been badly confused by the speculative metrologists, who have wasted much paper by theorising," Sir Flinders points out that some writers have been led astray by placing too much reliance upon the small minority of marked weights, as giving a greater certainty of meaning. Unfortunately, this reliance is misplaced, as marks often show what a weight was not, instead of what it was; they are, indeed, frequently secondary, being added to a weight of one standard in order to show what was its equivalent on another standard. The material investigated shows that we must recognise eight standards as having been in use in Egypt, *viz.* the *peyem*, the *daric*, the *stater*, the *qedet*, the *necef*, the *khoirīnē*, the *beqa*, and the *sela*. As to instruments, the steelyard was unknown in Egypt until Roman times, but an apparently prehistoric balance-beam of red limestone is described. It is 3·35 inches long, 0·16 to 0·20 wide, and 0·17 to 0·20 deep. The middle hole for suspension has a short tube rising from it, and the balance is consequently very rigid, so that equality can be seen only by the exact level; but it is possible to observe a change in level of 1 in 500.

A companion volume on *Arabic Glass Stamps and Weights* affords evidence of the remarkable accuracy obtained by medieval Muslim craftsmen in the standardisation of weights. Thus, three half-dinar weights of the year 780 show the astonishing result of agreeing to within a third of a milligram, being respectively 32·662, 32·665, and 32·667 grains. How such accuracy was reached is not known.

A *Catalogue des Manuscrits Alchimiques Grecs* is in course of publication (Brussels, Maurice Lamertin). Three volumes have appeared so far: I. *Les Parisini, décrits par Henri Lebègue, en appendice les manuscrits des Cœranides et tables générales par Marie Delcourt:* II. *Les Manuscrits Italiens, décrits par C. O. Zuretti avec la collaboration de O. Lagercrantz, J. L. Heiberg, I. Hammer-Jensen, D. Bassi et Æ. Martini:* III. *Les Manuscrits des Îles Britanniques, décrits par Dorothea Waley Singer avec la collaboration de Annie Anderson et William J. Anderson, en*

appendice Les Recettes Alchimiques du Codex Holkhamicus éditées par Otto Lagercrantz.

Dr. R. Campbell Thompson has published an important work upon *Ancient Assyrian Chemistry* (Luzac and Co., 46 Great Russell St., W.C.). It gives the texts, with transliterations and translations, of several clay tablets from the royal library of Ashurbanipal, King of Assyria in the seventh century B.C. These texts describe in detail the processes of glass-making as practised in Assyria 2,600 years ago, and are of great interest to the chemist. The book is written in such a way that it is not necessary to be a cuneiform scholar to appreciate it ; it is undoubtedly one of the chief contributions to the history of chemistry made during the present century. *Die Metallkunst der alten Ägypter*, by George Möller (Berlin, Ernst Wasmuth, A.-G., 1925), gives an account of the methods and results of metal-working in ancient Egypt. It forms a useful supplement to Lepsius, *Les métaux dans les inscriptions égyptiennes* (Paris, 1877), and to Sir W. M. F. Petrie, " The Metals in Egypt " (*Ancient Egypt*, 1915, p. 12). An interesting but rather fanciful theory of the origin of alchemy is given by Frau Ingeborg Hammer-Jensen in *Die Älteste Alchymie* (Copenhagen, *Det. Kgl. Danske Vindenskabernes Selskab, Historiskfilologiske Meddelelser*, iv, 2, 1921). Another theory of the birth of chemistry is advanced by M. Stéphanidès, " La naissance de la chimie," in *Scientia* (Bologna, 1922, pp. 189–97). He places the origin of chemistry at the time of the Ethiopian invasion of Egypt, 718 B.C.

ISLĀM.—Prof. J. Ruska has published the first two volumes of his series of *Arabische Alchemisten*. Volume I (Heidelberg, Carl Winter, 1924) deals with Khālid ibn Yazīd (died A.D. 704), the reputed founder of alchemy in Islām. Volume II (Heidelberg, Carl Winter, 1924) is on Ja'far al-Ṣādiq, said to be the teacher of Geber (Jābir ibn Ḥayyān). It contains the text and a translation of a work ascribed to Ja'far. Both volumes are well worth reading, and may in general be taken as authoritative, although some of Ruska's views on Ja'far and Jābir are probably incorrect. By the same author is a paper on Rhazes, the celebrated Persian physician and chemist who died in 925 ; it is called *Über den gegenwärtigen Stand der Rāzī-Forschung* (*Archivio di Storia della Scienza*, v. (1924), 335–47). It gives a sketch of the life of Rhazes, and is based largely upon original research. R. Reitzenstein, in his *Alchemistiche Lehrschriften und Märchen bei den Arabern* (Töpelmann, Giessen, 1923), gives, *inter alia*, the story of the Forty Viziers, which is of interest for the history of ancient alchemy. On the Emerald Table, Ruska has published a detailed study of practically the whole of the relevant literature under the title *Tabula*

Smaragdina; ein Beitrag zur Geschichte der Hermetischen Literatur (Heidelberg, 1926, Carl Winter's Universitätsbuchhandlung). Attention may also be drawn to the article by Robert Steele and Dorothea Waley Singer read to the Royal Society of Medicine on November 2, 1927, and to Martin Plessner's article " Neue Materialien zur Geschichte der Tabula Smaragdina " (*Islām*, Bd. xvi, Heft 1/2, pp. 77–113). It has become clear that the Emerald Table is of very ancient origin. The earliest text of it occurs in an authentic work of Jābir ibn Ḥayyān (*ca.* A.D. 722–813), and in its original form it may go back as far as Apollonius of Tyana.

A good deal of work has been carried out upon Geber (Jābir), *e.g.* Darmstaedter, *Die Alchemie des Geber* (Berlin, Julius Springer, 1922), and *Liber Misericordiæ Geber* (*Archiv für Geschichte der Medizin*, Bd. xvii, Heft 4, 1925, pp. 181–97). Darmstaedter's work, which is of the highest excellence, attacks the problem from the Latin side. From the Arabic side, progress has been made by Ruska, who is editing the Arabic text of the celebrated *Liber LXX*, of which two manuscripts were discovered in Cairo in 1926 by Prof. Max Meyerhof.

The authenticity of the tractate, well known to medieval Latin alchemists as *Avicennæ Mineralia*, has been established by the discovery of the original Arabic text ; this has been published with a translation by D. C. Mandeville and the present writer (Geuthner, Paris, 1927).

A lengthy and illuminating article on " Chemistry in Irāq and Persia in the Tenth Century A.D." was published in 1927 by H. E. Stapleton, the late R. F. Azo, and N. Hidāyat Ḥusain (*Memoirs of the Royal Asiatic Society of Bengal*, vol. viii, No. 6, pp. 317–418). Incidentally, these authors stress the similarity between Arabic alchemy and early Chinese alchemy.

Other papers on the history of chemistry in Islām are (*a*) Ruska, " Sal ammoniacus, nušādir und Salmiak," in the *Sitzungsberichte d. Ak. Wissensch., phil.-hist. Klasse*, Heidelberg, 1923, Abh. 5, p. 23 ; (*b*) E. Wiedemann, " Zur Geschichte der Alchemie," in the *Sitzungsber. der physik-. mediz. Sozietät*, Erlangen, **52**, 126–8 (1922).

Middle Ages.—A book with remarkable illustrations, from ancient MSS., is Giovanni Carbonelli's *Sulle fonti storiche della chimica e dall' alchimia in Italia*, Rome, Istituto Nazionale Medico Farmacologico, 1925. For figures of medieval chemical apparatus this book is invaluable. On the relation between magic and science, Lynn Thorndike's *History of Magic and Experimental Science in the First Thirteen Centuries of our Era* (London, Macmillan, 1923), is an excellent study, with full bibliography. For the origin and relations of the names of the metals a useful paper is that of E. Crivelli, " Le Vicende dei

nomi dei Metalli " (*Archivio di Storia della Scienza*, vol. ii, 1921, pp. 1–45), where the subject is treated in a masterly fashion. On the discovery of ether see Guy K. Tallmadge, *Isis*, vol. vii, 1925, pp. 394–411, where the text (with an English translation) is given of the third part of Valerius Cordus' *de Extractione*. In the same number of the same journal, pp. 478–85, Prof. C. H. Haskins deals with *Arabic Science in Western Europe*.

An interesting, and beautifully illustrated, historical sketch of " The Beginnings of Pharmacy " occurs on pp. 797–816 of vol. 106 (1927) of *The Chemist and Druggist*. Although this paper is not supplied to the general public, the number in question can usually be obtained through the good offices of the local pharmacist.

Dorothea Waley Singer, in her article, " The Alchemical Testament attributed to Raymund Lull " (*Archivio di Storia della Scienza*, vol. ix, pp. 43–52, 1928), definitely proves that the alchemical works credited to Lull are spurious.

LATER TIMES.—On the history of chemistry in France Hélène Metzger has written *Les Doctrines Chimiques en France du début du 17e à la fin du 18e Siècle* (Paris, Les Presses Universitaires de France, 1923). For the origin of the word " gas " see William A. Kirby, *Chemistry and Industry*, **42**, 1923, p. 325, and L. N. Brown, *op. cit.*, p. 349. The discovery of red phosphorus is dealt with by R. Winderlich in *Chemiker Zeitung*, vol. 47, 297 (1923). On chemical symbols and formulæ see Sir James Walker, *Nature*, iii, 1923, pp. 883–6. An interesting biographical note on *Lemery*, by Lucien Leroux, is in *Isis*, 1925, vol. vii, p. 450.

Dr. Edgar F. Smith's *Old Chemistries* (London, 1927) deals mainly with those early textbooks which exerted an influence on the development of chemistry in America. It is very attractively produced and may be commended to the chemical bibliophile.

Prof. Tenney L. Davis has an article on " Boerhaave's Attitude towards Alchemy," in *Medical Life*, vol. 33, 261 (June 1926), and another on " The First Edition of the *Sceptical Chymist* " in *Isis* (vol. viii (1926), pp. 71–6. In the latter he shows that the first edition of the celebrated chemical classic was published in London in 1661, and has two title-pages, the second occurring between pages 34 and 35. Contrary to the statement made by Jaeger and Stillman, the author's name is given, and the book therefore did not appear anonymously.

In the *Classics of Scientific Method* Prof. J. R. Partington has just published (London 1928) a volume on *The Composition of Water*. The author has given us the benefit of his unrivalled knowledge of the literature, and even the most experienced historian of chemistry will find something new in the book.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., Ph.D., University, Glasgow.

The Earth's Crust.—The principles of isostasy are discussed by Dr. W. Bowie in a new book (*Isostasy.* New York: Dutton & Co., 1927, 275 pp.). After expounding the general theory of isostasy, the author discusses the connection between gravity anomalies and local geological conditions. The study of the larger questions of the support and structure of the earth's crust is based on two main assumptions: (1) that nearly perfect equilibrium is maintained at all times; (2) that important disturbances of isostasy arise only from shifts of surface load through erosion. Many of Bowie's conclusions are not in accord with the facts of geology. Thus he explains folding and thrusting as incidental effects of vertical movement, with which very few students of tectonics will agree. Good criticisms of the book from the geological side by C. R. Longwell and R. T. C. are to be found in the *Amer. Journ. Sci.* (xv, 1927, p. 177), and *Journ. Geol.* (xxxvi, 1928, pp. 181–4) respectively.

Dr. G. R. Putman has published a paper entitled "Notes and Reflections on Isostasy" (*Science*, March 11, 1927, lxv, pp. 245–8), which is an abstract of a work published in *Journ. Wash. Acad. Sci.*, June 4, 1926. In effect this is a restatement in view of recent work of a method of reduction of gravity observations developed by Dr. Putnam in 1895.

In a discussion of the outer shells of the earth (*Amer. Journ. Sci.*, xv, 1928, pp. 108–35) Prof. R. A. Daly concludes that since the emergence-velocities of earthquake waves have been shown to be higher than expected at and near the surface of the *Grundgebirge*, some degree of departure from perfect elasticity of subcrustal material is indicated. The main question of the paper is the validity of the hypothesis that the material below the "50 to 60 km. discontinuity" is basaltic. According tc Daly the seismological facts do not forbid belief in a highly rigid, but hot and uncrystallised, basaltic substratum for the earth's crust. This view is believed to make the meaning of the term, the "crust of the earth," sharper and more concrete than under any other hypothesis.

In his paper on "Some Problems of Physical Geology, and the Earth's Thermal History," Prof. A. Holmes discusses such topics as the thickness of the continents, the growth of geosynclines, volcanic phenomena, mountain-building, and the cooling of the earth, in relation to the radioactive theory of the earth's thermal history (*Geol. Mag.*, lxiv, 1927, pp. 263–78). He concludes that the cooling of the earth is a discontinuous process. The accumulation of radioactive heat leads to the generation of magmas which ascend to higher levels in the crust, or are

discharged at the surface and thus rapidly lose their heat. He rejects Dr. H. Jeffrey's conclusion that an alternating process of this type cannot be deduced from the premises involved, and that a state of thermal equilibrium is ultimately established.

Prof. Holmes's letter on "Oceanic Deeps and the Thickness of the Continents" (*Nature*, December 3, 1927, pp. 804–5) deals with a related topic. Recent seismological evidence points to the floor of the Pacific being composed of gabbro. If so, an explanation can be offered of the occurrence of oceanic deeps bordering the mountainous edges of continents. The compressive forces which, acting on the *sial* of the continents, form mountains, would simply transform the suboceanic gabbro into its high-pressure facies eclogite; and in accordance with the principle of isostasy, such a change would lead to marked subsidence. The thickness of the sial may then be estimated, by a simple calculation, at about 30 kms.; and since radioactivity appears to set a limit of 15 to 20 kms. for this thickness on the assumption of uniform distribution of radioactive matter, it becomes necessary to assume that the radioactivity of the sial falls off rapidly in depth. Dr. Holmes offers an explanation of this falling-off based on the practical invariability of the atomic weight of ore-lead, showing that the dioritic base of the sial must be extremely poor in the radioactive elements.

Structural, Tectonic, and Igneous Geology.—The phenomena of jointing in relation to the mechanical principles of stress and strain are dealt with by C. O. Swanson (*Journ. Geol.*, xxxv, 1927, pp. 193–223); and Dr. B. Sander has published an excellent review of tectonic strength-relations of rocks (*Neues Jahrb. f. Min.*, B.-B. lii, Abth. B. Heft 3, 1925, pp. 365–401).

According to S. Taber (*Journ. Geol.*, xxxv, 1927, pp. 577–606) fault troughs may be classed as superficial or profound. Superficial fault troughs result from earthquakes, volcanic activity, or tension. Tensional fault troughs may be further classified as due to differential uplift, rotational strain, or the tilting of large fault blocks. The faults associated with major fault troughs extend to profound depths and reach the zone of flowage. Several factors have probably contributed to their formation. Tension, resulting in normal faulting and upward tilting of the bordering masses, is perhaps the most important. Other causes may be extrusion of lavas and increase of subcrustal density through the expulsion of gases and the crystallisation of magmas.

In an interesting paper on "Recent Lines of Fracture in the Färoes in Relation to the Theories of Fiord Formation," Dr. M. A. Peacock (*Trans. Geol. Soc. Glasgow*, xviii, pt. 1, 1928, pp. 1–26) represents the Färoe plateau as a tabular block of basaltic lavas which has suffered a right-handed twist from north to

south, and which is broken by a " fiord system " consisting of rectilinear, steep-sided straits and fiords whose principal direction is N.W. to S.E. The islands are further intersected by a younger system of major joints which develop into deep, straight-sided clefts known as *goes*, and these in turn become valleys which, in many instances, control the drainage. Goes tend to occur in collinear series which often cross adjacent islands, and the straits which separate them. The goe system comprises a network of rectilinear fractures, the majority of which lie east and west, and the remainder at various angles to this direction. The fiord system is regarded as an adolescent of mature goe system, related to an episode of crustal movement which took place after the volcanic period, but before the Ice Age. The principal result of this study is to confirm in the Färoes the tectonic theory of the origin of fiords.

The " Sea Lochs of the Outer Hebrides " described by Prof. J. W. Gregory (*Trans. Geol. Soc. Glasgow*, xviii, pt. 1, 1928, pp. 27–39) fit in well with those of the Inner Hebrides and the western Scottish mainland. " They are arranged on four directions which are correlated with the Alpine movements of the Oligocene and Miocene, and with the main tectonic lines in the North Atlantic. . . . East and west valleys and tectonic lines are the oldest connected with the fiord valleys, and they are part of the geographical features in N.W. Europe due to the Alpine foldings. Subsidences in the N.E. Atlantic led to the volcanic eruptions of the Hebrides, and made fractures trending N.W. to S.E., which were injected by basalt. Subsequent subsidence to the north subjected northern Scotland to torsional stress along a N. to S. axis. This torsion fractured the foundation on intersecting diagonal lines trending north-easterly and north-westerly." These movements were probably all earlier than the Middle Pliocene, when a general uplift of the British region caused the gaping of the four lines of fractures. The clefts thus formed across the Outer Hebrides were widened by denudation into the valleys occupied by the main sea-lochs.

A similar theme, but on a larger scale, is treated in Prof. Gregory's paper on " The Fiords of the Hebrides " (*Geogr. Journ.*, lxix, 1927, pp. 193–216). He points out that the orientation of the fiords is remarkably regular, and is to a great extent independent of the geological structure. The main point of interest in the present connection is the discussion of the relation of the fiord clefts to Alpine foldings, Atlantic subsidences, and the correlated igneous activity. Prof. Gregory points out the general parallelism of the fiords to the tectonic lines of the North Atlantic. He further shows how, in the neighbourhood of the local centres of volcanic upheaval in Skye and Mull, the fiord directions have been modified in

accordance with radial and concentric systems of fracture. The paper closes with an extremely useful tabular summary of Kainozoic events in the West of Scotland, in which the great volcanic eruptions of the North Atlantic from Greenland to Antrim are placed in the Upper Miocene.

Prof. F. E. Suess has discussed the igneous and tectonic geology of the Variscian Mountains of Late Palæozoic age in Central Europe (" Intrusionstektonik und Wandertektonik im variszischen Grundgebirge." Berlin, Gebr. Bornträger, 1926, pp. 268). In the zone of " intrusion tectonics " the rocks have been deformed and metamorphosed by great granite masses. Gneiss blocks in metamorphic disharmony with their surroundings are interpreted as thrust masses resting on less metamorphosed rocks. The " Wandertektonik " ideas of the older Suess are here utilised in combination with modern views of igneous intrusion and metamorphism. Reviews of this important work are to be found in *Amer. Journ. Sci.*, **13**, 1927, pp. 365–6; and *Journ. Geol.*, xxxv, 1927, pp. 575–6.

The great memoir by Hall and Molengraaff on the Vredefort Mountain Land (SCIENCE PROGRESS, xxi, 1926, p. 221) has led Mr. E. B. Bailey (*Geol. Mag.*, lxiii, 1926, pp. 481–95) to make an instructive comparison between the Vredefort Dome and the much smaller but very similar structure afforded by the Northern Granite of Arran. As pointed out by the late W. Gunn, and further emphasised and illustrated by Mr. Bailey, the strata around the Arran Granite up to the New Red Sandstone have been steeply uptilted, and in at least one locality overturned. On all sides save the north-east, where there was a structural weakness due to a fault mosaic, the strike of the schists surrounding the granite boss is dragged into approximate parallelism with the margin of the intrusion. Where, as to the N.W., the schists dipped towards the granite, fine synclinal structures have been developed by the uplift. In his discussion of the Vredefort Dome Mr. Bailey points out that the assumption of centripetal pressure as the major cause of the updoming is unnecessary, as central upheaval, accompanied by its correlated centrifugal pressure, is perfectly competent to produce the observed effects.

Mr. J. E. Richey has made a brilliant addition to knowledge of igneous tectonics in his study of the Mourne Granites of Northern Ireland (*Quart. Journ. Geol. Soc.*, lxxxiii, pt. 5, 1928, pp. 653–88). In the Eastern Mournes the three main intrusions are arranged concentrically with a marked excentricity towards the S.W. In each case a dome-shaped roof turns abruptly into a bounding wall which is either vertical or dips outwards at 70°. The Western Granite everywhere around its periphery extends below a domed roof, and no limiting wall is seen. " It

seems probable that the Eastern Mourne granites have come into place . . . by the subsidence of a block of country rock bounded by a ring-fracture, and separated from the roof by a cross-fracture, and by the influx of magma from below into the ever-widening fissure and into the space left below the roof." Continued subsidence of the block initiated new ring-fractures each more centrally placed than the earlier, and it is easy to understand therefore why the central intrusion should be the youngest of the series.

A small ring-dyke structure, consisting of dolerite, olivine-dolerite, and pegmatites (crinanitic), with a central mass of allivalite, has been described by J. Holmes from Loch Fyne, Argyllshire ("The Dolerite Boss of Sithean t-Sluain, Loch Fyne," *Trans. Geol. Soc. Glasgow*, xvii, pt. 3, 1927, pp. 426–37). It is intruded into the Dalradian schists, and has exercised a very considerable metamorphic effect upon them, as the boss is surrounded by an internal zone of buchite with corundum and spinel, and an external zone of hornfels. The petrographical characters of the intrusion suggest that its age is Kainozoic.

Prof. F. F. Grout has performed a useful service in briefly outlining the fundamental problems that await solution in regard to batholiths, and methods for their attack (*Journ. Geol.* xxxv, 1927, pp. 311–18). Prof. R. T. Chamberlin and T. A. Link have applied experimental methods to the study of batholithic intrusion (*ibid.*, pp. 319–52). Liquids forced from below through artificial strata did not progress straight upward, but followed inclined pathways. If conditions permitted they spread out laterally. From this work a theory of laterally-spreading batholiths has been formulated, following Iddings and Cloos rather than Daly.

A study of kimberlite dykes at Cayuga Lake (New York) by P. G. Sheldon (*Journ. Geol.*, xxxv, 1927, pp. 353–67) indicates a close relation between the intrusion of the dykes and stress at fault planes. In one case the pattern formed by a group of dykes crossed by a fault suggested that tension resulting from fault stresses tended to open joint fractures, and thus facilitate the entrance of dyke stringers. In another case fault stresses caused a constriction in a dyke. Above and below the constriction the magma removed the country-rock for the entire width of the dyke by stoping. Other structural features showed that disastrophic forces were active during the intrusion of the dykes.

By E. J. Salisbury, D.Sc., F.L.S., University College, London.

Anatomy.—Of the 170 species of *Clematis*, 138 have been examined respecting their stem anatomy by Miss E. P. Smith

(*Trans. R.S. Edin.*, vol. xv, No. 26). Of these 67 per cent. exhibit a twelve-bundled stem. The twelve bundles consist of foliar traces, namely, a median bundle and two lateral bundles from each of the nearest leaves and a corresponding set of six bundles, from the pair of leaves, above which are smaller and alternate with the larger. In 17 per cent. of the species only six bundles are present, whilst in 12 per cent. many bundles are present. Examples of the three types are respectively furnished by *Clematis vitalba*, *C. japonica*, and *C. Armandi*. The cork cambium develops in the pericycle except in *C. smilacifolia*, where it is sub-epidermal. Subsequently a phellogen develops each year in the phloem and the older layers, splitting in correspondence with the primary rays, produce the "string-bark" so characteristic of *C. vitalba*. Adventitious roots from the interfascicular cambium are formed on both nodal and internodal cuttings being accelerated on the latter by etiolation.

From an examination of the roots of *Zizania aquatica*, Stover (*Ohio Jour. Sci.*, xxviii, January 1928) concludes that the xylem is centrifugal in development, although lignification is centripetal. From the examination of other Monocotyledons, and having regard to the observations of Janczewski (*Ann. Sci. Nat. Bot.*, p. 208, 1904), it is suggested that this is true of roots in general.

Cytology.—A number of papers have recently appeared reporting the results of determinations of chromosome number in different species of the same genus. Mutzing (*Hereditas*, x, p. 241, 1928) finds that in the sub-genus *Ladanum* of *Galeopsis* the haploid number is eight, whilst in the sub-genus Tetrahit *G. tetrahit* and *G. bifida* have sixteen chromosomes, whilst *G. pubescens* and *G. speciosa* have eight. In the same journal Hakansson (pp. 282–91) records the haploid numbers for a number of Cyperaceæ, which show considerable variation. In *Eriophorum vaginatum* and *E. polystachyum* the number is twenty-nine, in *Scirpus sylvaticus* thirty-one, in *S. maritimus* fifty-two, in *S. setaceus* thirteen, in *S. palustris* nineteen, in *S. uniglumis* twenty-three, and *S. multicaulis* ten. Roscoe finds that the haploid number in *Typha latifolia* is fifteen. In *T. angustifolia* the number is larger, due to univalent chromosomes, and a high proportion of the pollen is sterile. *Typha angustifolia* thus exhibits the irregular chromosome distribution and sterility characteristic of hybrids (*Bot. Gaz.*, lxxxiv, p. 392).

R. Jaretzky has examined cytologically a number of Cruciferæ, and concludes that the basal haploid chromosome number in this family is eight. Sixteen chromosomes occur in *Arabis hirsuta* and *Alyssum calycinum*, the latter of which, though genotypically of the "gigas" type, is phenotypically a dwarf

species. Heilborn showed that within the one genus *Draba* eight (*D. nivalis*), sixteen (*D. incana*), twenty-four (*D. rupestris*). and thirty-two (*D. alpina*) haploid chromosomes occur, *Stenophragma* (Sisymbrium) *thalianum* possesses only five chromosomes, and this aberrant number and its peculiar combination of characters is held to be perhaps due to its origin by hybridisation between *Sisymbrium* and *Arabis*. Seven chromosomes are found in *Erophila verna*, *Cheiranthus cheiri*, *Matthiola incana*, and *Coringia orientalis*, whilst multiples of this number are met with in *Hesperis* (fourteen) and *Camalina* (twenty-one) (*Jahrb. f. wiss. Bot.*, p. 1, 1928).

Tjebbes (*Hereditas*, x, pp. 328–31) reports twelve chromosomes as the haploid number for *Geranium pratense* and *G. sylvaticum* ; six for *Plantago lanceolata* and *P. major* as also for *Linaria vulgaris* and *L. repens*.

Darlington (*Jour. Genetics*, January 1928) concludes that the basic chromosome number in the genus *Prunus* is eight, but finds a very wide range of somatic numbers. Varieties of *Prunus avium* showed 17–19, of *P. acida* and *P. cerasus* thirty-two, whilst *P. persica* and *P. amygdalus* have sixteen. Varieties of *Prunus institia* showed 40 and 48. A number of sweet cherries examined showed 17, 18, or 19 somatic chromosomes, according to the variety, and are self-sterile. The sour and Duke cherries, however, have thirty-two somatic chromosomes, and these tetraploid types are self-fertile.

The " enclaves proteiques " described by Vaudendries as present in the epithelial cells of the Cruciferous embryo-sac have been studied by Lachertowa (*Acta. Soc. Bot. Pol.*, vol. v, No. 1), who finds that they are tanniferous vacuoles similar in character to those described by Dangeard as present in Gymnosperms.

Ecology.—According to Sukachev the spruce communities of European Russia occur throughout about half its area and before the advent of agriculture occupied a considerable part of the area now under Oak and Pine (*Jour. Zoology*, February 1928). On well-drained clays and loams on poor but non-acid soil there is a continuous and abundant moss flora, and a sparse shrub layer. Herbs are here few, amongst the most characteristic being *Goodyera repens*, *Circæa alpina*, *Linnea borealis*, *Maianthemum bifolium*, and *Trientalis europæa* more rarely with *Epipogon aphyllum*, *Pyrola uniflora*, etc. Various associations of this type are recognised in which *Oxalis*, *Myrtillus*, *Atrogene sibirica*, *Polytrichum*, and *Empetrum* become respectively important. On similar but poorly drained soils the Spruce is mixed with Birch and *Polytrichum commune* forms a continuous carpet. Here the herb-layer is poor, but *Equisetum sylvaticum* characteristic. On flat, ill-drained soils *Sphagnum*

with or without *carex* becomes conspicuous. In depressions with flowing water a dense herbaceous flora develops with *Ulmaria palustris*, *Angelica sylvestris*. Only on rich soils near limestone deposits in the south of the spruce area is there a dense shrub layer developed with *Tilia cordata*, *Euonymus verrucosus*, etc., and deciduous trees such as Oak, Aspen, and Birch are present which, as the soil becomes impoverished, become replaced by Spruce.

By raising seedlings of various conifers at different distances from an artificial source of light Bates and Roeser conclude that *Sequoia sempervirens* can utilise light of as low an intensity as 0·75 per cent. that of sunlight. Engleman Spruce and Douglas Fir appear to require a minimum of about 1·2 to 1·5 per cent., *Pinus strobus* and *P. ponderosa* about 2 per cent., *Pinus flexilis* about 2·5 per cent. and *Q. edulis* 6·3 per cent. Evidence was, however, obtained of marked individuality amongst seedlings of the same species with respect to their photosynthetic efficiency (*Amer. Jour. Bot.*, March 1928).

Stapledon (*Jour. Ecol.*, Feb. 1928) from an extensive study of *Dactylis glomerata* finds that the habit may be tall, or cup-like, either strain exhibiting both lax and dense growth types. Tussocked and pasture types are also recognised, and breeding experiments indicate that most of these are probably genotypically distinct. There are seven types of inflorescence with the panicle either erect, semi-erect, or drooping, and the lowest branch is either reflexed or diverges at an acute angle. Broad-leaved and narrow-leaved strains can be distinguished, and the sheath may be glabrous or pubescent. The species is self-fertile, and though there is generally a diminution of vigour with repeated selfing, yet in some offspring the original vigour is maintained.

The importance of the " litter " in forests as it affects the temperature of the lower stratum of air is shown by the results obtained by Firbas (*Beih. z. Bot. Centralbl.*, xliv, p. 179). Owing to the low specific heat of the litter and its poor conductivity, especially when dry, it may attain a considerably higher temperature than that of the air generally, so that the herbaceous carpet in spring may be surrounded by an atmosphere having the optimum temperature for assimilation, whereas the general air temperature may still be below the minimum.

Krzemieniewska has studied the occurrence of myxobacteria in the soils of Poland (*Acta Soc. Bot. Pol.*, vol. v) and finds that their distribution is largely correlated with the soil reaction. Some species occur only in alkaline to slightly acid soils p.H. 6·0–8·0 (e.g. *Myxococcus virescens*), whilst others are as definitely restricted to soils that are markedly acid, p.H. 3·6–6·4 (e.g. *Sorangium septatum*). Others, such as *Myxococcus rubescens* and

S. compositum, exhibit a wide range of tolerance. Members of this group are more abundant in lowland than in upland soils.

As a result of studies on *Polygonum amphibium*, *Marsilia quadrifolia*, *Limnophila heterophylla*, and other marsh plants, Wolterneck (*Flora*, p. 30, 1928), confirms the importance of the relation between organic and inorganic food supply in determining the nature of the foliar type. By stimulating the rate of assimilation the land type of leaf can be induced even under water, whilst the water-form of leaf is induced by checking assimilation and providing conditions favourable to the intake of inorganic salts.

The efficacy of montane culture of plants in increasing their resistance to disease is discussed by Costantin (*Ann. Sci. Nat.*, December 1927), who deals at length with the increased resistance of Sugar-cane to Sereh consequent upon culture at high elevations. The author suggests that the climatic conditions induce the production of substances analogous to antitoxins.

Tincker (*Ann. Bot.*, clxv, p. 101, 1928) finds that a shortened period of illumination may cause *Avena sativa* to function as a biennial. Several species, namely, *Anthoxanthum odoratum*, *Dactylis glomerata*, *Phleum pratense*, and *Trifolium pratense*, which under a shortened day failed to flower during the first season, under natural conditions flowered earlier in the second season.

From a study of the Banana, Skutch finds that the lateral buds arise opposite the axils of the leaves, although in the closely allied *Ravenala* the buds occupy the normal axillary position. The phyllotaxy varies from 1/3 to 4/9 on the adult. Stomata are orientated parallel to the vein and show the usual increase in number from the base of the lamina to the apex and from the midrib to the margin (*Bot. Gaz.*, lxxxiv, pp. 337–91).

PLANT PHYSIOLOGY. By Prof. Walter Stiles, Sc.D., F.L.S., F.R.S., The University, Reading.

Permeability and Related Questions.—In the course of the last discussion on Permeability in these pages (Science Progress, **20**, 417–23, 1926) reference was made to work with the marine alga *Valonia* by Matilda M. Brooks. The same worker has since extended her observations with this plant and published a number of fresh papers recording her results. She finds ("The Permeability of Protoplasm to Ions," *Amer. Journ. Physiol.*, **78**, 116–20, 1926) that when plants of *Valonia macrophysa* are immersed in solutions containing 0·002 M. of arsenic and arsenious acids for various periods, the concentration of arsenic in the cells may be greater than that left in the

external medium. Her results militate against the view expressed by Osterhout that only undissociated molecules enter living cells. A number of papers by the same writer deal with the penetration into the same alga of the dye 2–6–dibromophenol indophenol. In one of these papers (" Studies on the Permeability of Living Cells, VI : The Penetration of Certain Oxidation-reduction Indicators as influenced by *p*H : Estimation of the *r*H of Valonia," *Amer. Journ. Bot.*, **13**, 360–79, 1926) it is recorded that the penetration of the dye follows the same course as a bimolecular reaction and the amount of dye in the sap when equilibrium is attained is proportional to the amount of undissociated dye in the surrounding medium. The dye is present in the sap in the reduced form only, but the sap alone will not reduce it ; hence it is concluded that the reduction is probably brought about by the – SH group of the protoplasm. Increasing the acidity of the sap resulted in the penetration of more dye. With higher temperatures or with lower concentrations of dye the intake of the dye follows a course similar to that for two consecutive unimolecular reactions. Methylene blue is found in the sap only in the oxidised form and the amount present is not influenced by changes in acidity. The oxidation-reduction potential of *Valonia* was found to lie between *r*H 16 and 18.

In another paper Miss Brooks (" Studies on the Permeability of Living Cells, VII : The Effects of Light of Different Wavelengths on the Penetration of 2–6-dibromophenol indophenol into Valonia," *Protoplasma*, **1**, 305–12, 1926) records that when plants of *Valonia* are placed behind screens which transmit a limited range of light-waves, the amount of the dye entering the cell sap increases as the wave-length of the light decreases. The wave-lengths examined varied from 300$\mu\mu$ to 700$\mu\mu$.

In yet another paper Miss Brooks has recorded the results of experiments on the influence of a number of salts on the penetration of a dye, dahlia, into cells on the alga *Nitella* (" Studies on the Permeability of Living Cells, VIII : The Effect of Chlorides on the Penetration of Dahlia into Nitella," *Protoplasma*, **2**, 420–7, 1927). She found that all the chlorides examined, those of sodium, potassium, calcium and magnesium, antagonise the entry of the dye, of those examined sodium being the least effective and magnesium the most effective. An antagonism was observed between the action of sodium and calcium, but in presence of both these salts the entry of the dye was not equal to that in absence of them.

Marian Irwin has also obtained data relative to the penetration of dyes into cells, in this case those of the alga *Nitella*. She finds that when cells of this alga are placed in a solution of brilliant cresyl blue containing ammonium chloride, the dye

accumulates in the cells less rapidly than when no ammonium chloride is present, and this may occur without any increase in the alkalinity of the sap (" Accumulation of Brilliant Cresyl Blue in the Sap of Living Cells of Nitella in the Presence of NH_3," *Journ. Gen. Physiol.*, **9**, 235–53, 1926). This decrease in the rate of intake of the dye appears to be related to the presence of ammonia in the sap and does not appear to occur when ammonia is present only in the external solution. In a second paper (" Exit of Dye from Living Cells of Nitella at Different *p*H Values," *Journ. Gen. Physiol.*, **10**, 75–102, 1926) the same worker reports that experiments on the exosmosis of brilliant cresyl blue from living cells of *Nitella* immersed in solutions of various *p*H value but containing no dye, support the view that the quantitative relations between the amount of dye in the external solution and in the cell interior depend on the fact that the dye exists in two forms, one of which passes readily through the protoplasm while the other penetrates slowly or slightly. The quantity of the former increases at the expense of the latter with increasing alkalinity. Thus the rate of exosmosis of the dye is increased when the alkalinity of the sap is raised by penetration into it of ammonia.

G. W. Scarth (" The Mechanism of Accumulation of Dyes by Living Cells," *Plant Physiology*, **1**, 215–29, 1926) has published data relative to the way in which dyes accumulate in cells. Where basic dyes are strongly absorbed and so stain deeply they combine with colloidal material in the sap as well as with protoplasmic material and the cell is then incapable of further vital staining. The staining of the cell sap is greatest when the sap is slightly acid, with *p*H between 5 and 6. If made more acid the dye leaches out from the cells and the material in the sap acquires an affinity for acid dyes. If the sap is made more alkaline there is also a slight loss of basic dye. The maximum condensation of the colloidal material appears to take place when the *p*H is slightly higher than that of the isoelectric point as indicated by staining. It is concluded by Scarth that the colloidal material is an ampholyte and that it appears to be partly hydrated lipoid substance, probably surrounded by a mono- or bi-molecular film.

A long paper has been published by K. Höfler and F. Weber on the effect of ether narcosis on the permeability of plant cells to urea (" Die Wirkung der Äthernarkose auf die Harnstoffpermeabilität von Pflanzenzellen," *Jahrb. f. wiss. Bot.*, **65**, 643–737, 1926). The plant cells employed were the cortical cells of the upper part of the flower-stalk of *Hemerocallis flava* and *H. fulva*, and of the stem of *Callisia repens*. For determining the permeability of the cells the plasmometric method of Höfler was employed in which the volume changes in the

protoplasts of individual cells are determined. It was found that with all three kinds of cells permeability to urea is practically doubled by adding ether. Control experiments in which sucrose was used in place of urea showed that the more rapid recovery from plasmolysis in the case of etherised cells is actually due to the penetration of urea and not to the production of fresh solutes in the cell under the action of the narcotic.

The permeability of the protoplasm of the cells of onion-bulbs, Dahlia tubers, and some other kinds of cells is dealt with in a contribution by W. S. Iljin ("Die Durchlässigkeit des Protoplasmas, ihres quantitative Bestimmung und ihre Beeinflussung durch Salze und durch die Wasserstoffionkonzentration," *Protoplasma*, **3**, 558–602, 1928). From these researches it appears that more sugar is left in onion-bulbs and more fructose and inulin in Dahlia tubers after they have been soaked for two days in a salt solution than after they have been exposed for a similar time to distilled water. The salts examined, which all produce this effect, are the chlorides of sodium, potassium, magnesium, calcium, and manganese. That this result is to be attributed to the salt reducing the permeability of the protoplasm to the sugar or inulin, as the case may be, is shown by the fact that analysis of the external solution shows that less of the carbohydrate in question diffuses into a salt solution than into water in the same time. Of the salts examined calcium chloride is apparently the most effective in reducing permeability to sugar. If too high a concentration of salt is employed an increased exosmosis of sugar may be observed ; this is attributable to toxic action of the salt in high concentration. Similar results were obtained with regard to the influence of salts on the permeability of the protoplasm to potassium. Mixtures of salts were found to be more effective than single salts in reducing the permeability of cells of the onion to sugar. With Knop's solution, however, a position of maximum permeability was observed in a certain low concentration of the solution ; with increase or decrease in concentration the permeability was less. The reaction of the external solution exerts a very considerable influence on the permeability of the protoplasm. In passing from acid through neutral solutions to an alkaline medium the permeability decreases to a minimum and then increases. The position of the minimum varies with different molecules and ions. For sugar it was found to lie at about *p*H7 and for potassium at about *p*H5·6.

The present writer has investigated the exosmosis of solutes from slices of various storage tissues into distilled water ("The Exosmosis of Dissolved Substances from Storage Tissue into Water," *Protoplasma*, **2**, 577–601, 1927). The course of

exosmosis is entirely different if the tissue is kept in still water in closed bottles from what it is when the bottles are kept in movement. In the former exosmosis continues until the tissue is completely dead, whereas under the latter conditions, and if the stoppers of the bottles are removed at least once a day, an exosmosis of electrolytes is followed by a reabsorption of the electrolytes previously given out. This difference in behaviour is attributed to the removal of diffusion shells of oxygen and carbon dioxide in the case of the shaken tissue. It is thought that the course of exosmosis and absorption observed is due to two processes: a diffusion of solutes from cut surface cells and cells bordering them, and an absorption of these solutes by the uninjured living cells in the interior of the tissue slices. The observed results point to the possibility that solute absorption by plant cells is to be regarded as a double process, an adsorption of solute by the protoplasm and a diffusion of solute through the protoplasm into the vacuole.

The influence of various factors on the capacity of cells of *Nitella* to accumulate halogens in the cell sap has been investigated by D. R. Hoagland, P. L. Hibbard and A. R. Davis ("The Influence of Light, Temperature, and other Conditions on the Ability of Nitella Cells to concentrate Halogens in the Cell Sap," *Journ. Gen. Physiol.*, **10**, 121–46, 1926). By means of a special technique the amounts of chlorine and bromine in very small quantities of sap, usually about 1 c.c., were determined. Both halogens accumulate in the sap so that at equilibrium the concentration of these may be very much higher in the sap than in the surrounding solution. It was found that the electrical conductivity of the sap can be increased by this accumulation and the latter is not explicable on the basis of combination of the halogen with a cell sap constituent. Energy relations appear to be involved, for it was found that the accumulation is dependent on illumination, although there does not appear to be a direct light effect involved. The temperature coefficient of the rate of accumulation of halogen in the sap is of the order of that characteristic of a chemical reaction. It is concluded that the usual conceptions of permeability are inadequate to explain the absorption of mineral salts by plant cells.

Absorption of mineral salts by seedlings of *Citrus* and walnut has been investigated by A. R. C. Haas and H. S. Reed ("The Absorption of Ions by Citrus and Walnut Seedlings," *Hilgardia*, **2**, 67–106, 1926). In agreement with earlier work by other investigators on other plants these workers find that the two ions of a salt are not absorbed in equivalent quantities. In nearly every case the kation of a salt was found to be absorbed more than the anion, although the amount of the one ion

absorbed appears to be dependent on the nature of the other. Thus more kation would be absorbed from a nitrate than from a chloride of the same metal. The absorption of calcium was found to be retarded by the presence of potassium and much less so by the presence of sodium. The absorption of potassium was not retarded by the presence of sodium. Potassium and nitrate were generally absorbed in large amounts. The absorption of the latter was retarded by the presence of chloride but not by other ions. In all cases an exchange of ions between roots and solution was observed, generally resulting in a change in the hydrogen-ion concentration of the solution.

Finally reference may be made in this connection to a paper by H. G. M. Jacobson (*Journ. Agronomy*, **17**, 577–86, 1925), in which it is reported that wheat plants 100 days old changed the reaction of a culture solution in 12 hours from pH3·9 to pH6·3, this decrease in acidity being attributed partly to selective absorption of the nitrate-ion of potassium nitrate, excess potassium ions being left in the solution, and partly to root excretion. Rice plants brought about a change in the reaction of the culture solution in the reverse direction, from pH5·0 to pH3·0 in three days, this change in this case being attributed to excess absorption of the kations of sulphates and excretion of carbonic acid from the roots.

ENTOMOLOGY. By H. F. Barnes, B.A., Ph.D., Rothamsted Experimental Station, Harpenden.

General Entomology.—P. A. Buxton and G. H. E. Hopkins (*Researches in Polynesia and Melanesia, Parts* 1–4, *Medical Entomology*, published by the London School of Hygiene and Tropical Medicine) have produced an interesting work dealing with the islands, their flora and fauna (part 1), the climate of Samoa (part 2), the myriapoda, arachnida and insecta (part 3) and an account of experiments made with mosquitoes (part 4). G. H. Carpenter (*The Biology of Insects*, xv + 473, 16 pls., 88 figs., 16 pp. refs., Sidgwick & Jackson, Ltd., 1928, 16*s.*) has written a readable and well-illustrated book on insect biology. E. Hubault has valuable information regarding the biology of aquatic insects in his excellent work entitled *Contribution à l'étude des Invertébrés Torrenticoles* (*Supplement* 9 *of Bull. Biol. France et Belgique*, 1927). He emphasises the importance of the supply of oxygen to all forms living in this ecological habitat. *A List of the Insects of New York* (Memoir 101, Cornell University Agricultural Experimental Station, 1928, 1121 pp.) is a useful list of insects, spiders, gall-mites and Diplopods arranged under orders. In all, 430 families, 4,797 genera, and 16,124 species are recorded. A fourth fully revised

edition of Rostrup's book on agricultural pests has been produced by S. Rostrup and M. Thomsen (*Vort Landbrugs Skadedyr*, August Bangs Forlag, Copenhagen, 1928, xix + 348, 224 figs.). It is a great pity there is not a textbook in English on similar lines to this one. The figures are especially good.

W. Horn (*Deut. Ent. Mus.*, **12**, 1926, 133 pp.) has done a useful piece of work in compiling the location of entomological collections alphabetically by authors.

Observations et réflexions d'un naturaliste dans sa campagne (Librairie Kundig, Geneva, 1928, 211 pp.) is the title of a book by F. Brocher dealing in delightful language with the habits and biology of a number of common insects and original observations and experiments. O. W. Richards (*Biological Reviews*, **2**, 1927, 298–364) deals with sexual selection and allied problems and appends a list of over 400 references. Monstrosities or abnormal structures, particularly of Orthoptera, are the subject of an exceedingly interesting work by P. Cappe de Baillon (*Recherches sur la Tératologie des Insectes*, 1927, 291 pp., Lechevalier, Paris). The writer deals with the embryology, morphology and biology chiefly of the Phasmid *Carausius morosus*, and has a critical review of the bibliography of teratology. W. R. Thompson and H. L. Parker (*Ann. Soc. Ent. France*, **96**, 1927, 113–46) give an account of the life and morphological adaptation of parasitic insects.

H. M. Morris (*Ann. Appl. Biol.*, **14**, 1927, 442–63) finds that artificial manures have little effect on the soil fauna as represented by insects and other invertebrates, but dung considerably increased the numbers.

G. Fox Wilson (*Trans. Ent. Soc. London*, **74**, 1926, 243–54) showed how attractive sap-exudations are to certain insects and more recently D. Keilin (*Parasitology*, **19**, 1927, 368–74) has shown how limited spaces, *e.g.* sap-exudations and tree wounds, in this case on a horse-chestnut tree, may be of the greatest importance in the infection of larvæ with various forms of parasites.

The prevention of insect pests as opposed to the cure of insect pests is the subject of a very readable paper by G. Fox Wilson (*Jl. R.H.S.*, **52**, 1927, 235–45). To quote the writer: "Within recent years we have heard a great deal about preventive medicine but far too little about the prevention of insect pests and fungus diseases"; and again, "It is important to prevent the presence of insects . . . in gardens and farms, not only for the direct damage they do to plant life . . . but for the indirect damage that is done by (i) lowering the vitality of plants . . . and (ii) the danger of disease transmission directly through their presence." In this connection it is interesting to note that K. M. Smith (*Ann. Appl. Biol.*, **14**, 1927, 113–31)

has been successful in transmitting mosaic disease of potato by means of two species of greenfly.

Insects have long been known to play an important part in the pollination of fruit-trees, and G. Fox Wilson (*Jl. R.H.S.*, **51**, 1926, 225–51) has now provided us with more definite conclusions as to what insects may be considered essential. In a series of experiments on the sensibility of insects to chemical stimulants R. Poutiers (*Ann. des Epiphyties*, **13**, 1927, 181–94) shows that the addition of acetic acid and ethyl alcohol to beer increases the attractiveness of the mixture.

A. O. Larson (*Ent. News*, **38**, 1927, 47–51 and 65–70) shows how motor-cars, besides acting as carriers of insects, also reduce the number of insects, by (1) diminishing the quantity of food available for houseflies, (2) dripping oil which acts as a mosquito larvacide, (3) dusting the hedgerows and so destroying breeding places, and (4) running over immense numbers and striking others while in flight.

The history of the introduction of beneficial insects into Hawaii for the destruction of insect pests is reviewed by P. H. Timberlake (*Proc. Hawaii Ent. Soc.* 1926, **6**, 1927, 529–56), and it is pointed out that the main reason for the success obtained is the fact that the insect fauna of these islands is extremely simple compared with other regions. In view of the development of the artificial exportation of insects in connection with the biological control of weeds and insects, Miss N. M. Payne's paper (*Jl. Morphol.*, **43**, 1927, 521–46) on the freezing and survival of insects at low temperatures, with a list of references, is noteworthy.

Some Pests of Water Lilies is the title of an interesting paper by G. Fox Wilson (*Jl. R.H.S.*, **52**, 1928, 81–91) in which the larvæ of caddis-flies, aphides, the waterlily beetle, and the larvæ of the Brown China moth and a midge are the chief insects with which the writer deals.

Orthoptera.—The second volume of *Thysanoures, Dermaptères et Orthoptères de France et de la Faune Européene* (Gaston Doin, Paris, 1927) by C. Houlbert completes the account of the Orthoptera. The numerous illustrations should prove useful to the field naturalist for purposes of identification. The first volume was published in 1924.

Coleoptera.—In an endeavour to throw light on the means by which the cotton plant attracts the cotton boll-weevil, N. E. McIndoo (*Jl. Agr. Res.*, 1926, **33**, 1095–1141) has made a thorough study of the anatomy of the sense organs of the weevil and compiled all the available information. H. C. James, in a paper (*Ann. Appl. Biol.*, **14**, 1927, 479–81) on the so-called sensory pits of the 9th abdominal segment of the wireworm, comes to the conclusion that the structural evidence favours the view

that these pits are a pair of muscular impressions; no structures associated with them indicate any sensory receptions and there is no evidence that they are a pair of modified spiracles as suggested by Horst (1922). In addition, some notes on the internal anatomy are given—there are no salivary glands and the mesenteron is devoid of gastric cæca. The development of the mid-gut of insects has been the subject of many investigations, and there are two views—(*a*) the mid-gut epithelium is endodermal in origin, (*b*) the mid-gut epithelium is derived from proliferations of the inner ends of the stomodæum and proctodæum and therefore is ectodermal. From investigations on *Calandra* K. Mansour (*Q.J.M.S.*, **71**, 1927, 313–52) considers that the larval mid-gut epithelium in the rice-weevil and most pterygote insects is derived from the inner ends of the stomodæum and proctodæum and so ectodermal in origin (*cf.* Eastham, 1927).

A valuable study of the immature stages of water-beetles has been made by H. Bertrand (*Les Larves et Nymphes des Dytiscides, Hygrobiidæ et Haliplides*, vi + 366, 1928, Lechevalier, Paris). Nearly all the larvæ described by J. C. M. Gardner (*Identification of Immature Stages of Indian Cerambycidæ* II, *Indian Forest Records*, **13**, 1927, 1–37) are of great importance as pests of forest trees. L. Falcoz (*Ann. des Epiphyties*, **12**, 1926, 109–29) gives an important account of Curculionid larvæ of the genera *Cleonus*, *Lixus*, *Ceutorrhynchus*, and *Baris*.

M. H. Hatch, in a summary of our knowledge of fossil Coleoptera (*Bull. Brooklyn Ent. Soc.*, **21**, 1926, 137–44), points out that probably the beetles have been a dominant animal group since the beginning of the Mesozoic era.

Lepidoptera.—A revised edition of E. Meyrick's book on British Lepidoptera (Messrs. Watkins & Doncaster, 1928) has appeared; that part of the volume dealing with the Micros has been rewritten. It will be noticed that about 100 species have been added to the British list. W. J. Kaye and Sir Norman Lamont, Bt., have compiled a valuable catalogue of the Trinidad moths (*Mem. Dept. Agric. Trinidad and Tobago*, No. 3, viii + 144, 1927); 1,016 species of moths are recorded, but the list is only a preliminary one, and it is felt that many more will be recorded. From an economic point of view twenty-eight species are definitely ascertained to be habitual or occasional pests to horticulture or agriculture.

Butterfly migration in South India and Ceylon is the subject of a paper (*Trans. Ent. Soc. London*, **75**, 1927, 1–33) by C. B. Williams.

Since so little information on the embryology of insects has been written in English, L. Eastham (*Q.J.M.S.*, **71**, 1927, 353–94) has done a useful piece of work by his paper on the

embryology of *Pieris rapæ*. It appears that the vestigial endoderm develops earlier in *Pieris* than in *Calandra* (K. Mansour, 1927), and so there is perhaps no great distinction between a mid-gut that arises from anterior and posterior mesenteron rudiments (endodermal) and one which arises from the blind ends of the stomodæum and proctodæum (ectodermal).

An important paper on the grasserie disease of silkworms and the wilt disease of tent caterpillars by R. W. Glaser has appeared (*Ann. Ent. Soc. America*, **20**, 1927, 319–43).

Hemiptera.—The second volume of F. V. Theobald's valuable monograph on the British Aphides has appeared (*Ashford, Kent, Headley Bros*., 1927, 411 pp.). This volume deals with the subtribe Aphidina (the last of the tribe Aphidini) and the first two subtribes, Callipterina and Monaphidina, of the tribe Callipterini. J. Davidson (*Jl. Linn. Soc*., **36**, 1927, 467–77), after dealing with intermediate forms of *Aphis rumicis* (forms possessing morphological features which place them as intermediate between alate and apterous viviparous females), comes to the view that they are produced by conflicting physiological influences which tend to produce apterous forms; further, that the apterous condition, as well as involving germinal changes, is the result of physiological conditions. This appearance of intermediates appears to be parallel with Goldschmidt's intersexes in silkworms. R. Le Pelley in a study of the resistance of apple to woolly aphis (*Jl. Pom*., **6** 1927, 209–41) comes to the conclusion that whatever determines immunity is inherited and probably segregates in a Mendelian manner. T. Esaki and W. E. China (*Trans. Ent. Soc. London*, **75**, 1927, 279–95) have raised a new family, Helotrephidæ, to hold the three genera *Helotrephes* Stal, *Idiocoris* and *Paskia*, the latter two genera of water-bugs being discovered in Lake Tanganyika. *Fascicle* 1 *of the General Catalogue of the Hemiptera* (Membracidæ by W. D. Funkhouser, Smith College, Northampton, Mass., 1927, 581 pp.) sets a very high standard for the remaining fascicles of this work; in this fascicle each species has been recorded under each genus in which it has ever been placed and a reference is given to the correct location. S. Hughes-Schrader has previously shown that the species of Coccid, *Icerya purchasi*, comprises hermaphrodites—capable of self-fertilisation and which correspond in general body form and behaviour to the females of related species, and which arise from fertilised eggs —and occasional males, haploid in chromosomal constitution, which arise from unfertilised eggs. In the present paper (*Zeitschr. f. Zellforsch. Mikros. Anat*., **6**, 1927, 509–40) the writer describes the development of the hermaphrodite gonad. The germ-cells of the hermaphrodite embryos contain the diploid number of chromosomes (4) from the time of the first

segregation of the germ-cells up to the time of the emergence of the nymph from the egg. During the early part of the first nymphal instar, usually before the attachment of the nymph to the host plant has occurred, haploid nuclei appear among the diploid nuclei of the gonad. Breeding data indicate that separate strains or races differing in respect of the presence or absence of the facultative parthenogenesis by means of which the haploid males are produced do not exist. M. Thomsen (*Zeitschr. f. Zellforsch. Mikros. Anat.*, **5**, 1927, 1–116) gives a detailed account of the cytological study of *Lecanium hesperidum* and *L. hemisphæricum*, *Trialeurodes vaporarium*, *Aleurodes proletella* and *A. nephrolepidis*, together with a general discussion on the question of parthenogenesis. S. Minkiewicz (*Mém. d'Instit. Nat. Pol. d'Écon. Rur. Pulawy*, **114**, 1927, 457–528) states that the hatching from the egg of the apple-sucker (*Psylla mali*) is dependent on the early or late development of the buds—hatching occurs on different varieties of apples at different dates, but at the period when the buds of these varieties are in a similar state of development. He maintains in connection with the hatching period that Theobald's and Lees' hypotheses concerning the heat generated by the flow of sap and the physiological connection between the egg and tree are untenable on account of numerous observations such as the position of eggs on scales, the hatching at different dates of eggs on the same flower-stalk, etc., and puts forward the hypothesis that the different duration of development may be due to the fact that the eggs are laid at different times and that the ones laid earliest may have reached an advanced stage of development before winter weather sets in.

Hymenoptera.—A comprehensive study of the biology of this group, so far as is at present known, by H. Bischoff (*Biologie der Hymenopteren*, Springer, Berlin, 1927) has appeared. In *The Origin of Instinct*, by E. Bugnion (Kegan Paul, Trench, Trübner & Co., Ltd., 1928), translated by C. K. Ogden, we have an admirable account of the wars between the ants and the termites which will be extremely useful to all students of instinct. A. H. Hamm and O. W. Richards (*Trans. Ent. Soc. London*, **74**, 1926, 297–331), in a paper on the biology of the British Crabronidæ, bring together many valuable observations, collected during twenty years, on the prey of this group.

The first of a series of papers on the muscles of the bee by G. D. Morison has appeared (*Q.J.M.S.*, **71**, 1927, 395–463) and deals with the somatic musculature, histology of muscle-fibres, physiology and histology of the nervous system in relation to the musculature and the mechanics of respiratory movements. This paper is particularly valuable as it is the precursor, with two other papers on healthy muscles yet to

appear, of one on the pathological muscles caused by diseases of adult bees. St. Soudek (*Bull. de l'école supérieure d'Agronomie, Brno*, 1927), from a microscopical and cytological study of the structure of the pharyngeal glands and their physiological condition in bee workers of different ages and activities, concludes that the function of the glands is the production of brood-food. This agrees with the usually accepted view of Rösch and others. In *Die Biene*, by F. Leuenberger (152 pp., 104 figs., 1928, Sauerländer & Co., Aarau) we have an admirable and well-illustrated handbook on the structure and life of the honey-bee. To E. F. Phillips and L. M. Bertholf (*Jl. Agr. Res.*, **35**, 1927, 385–428 and 429–52) we are indebted for two important papers on the utilisation of carbohydrates by honey-bees and larvæ respectively. The former writer shows that enzymes of the amylase-dextrase group are absent in adult bees, and this fact, together with the work recorded, supports the view that dysentery in winter is caused by the indigestibility of dextrins in the food supply. Bertholf shows that bee larvæ cannot use starch or glycogen, and that diastase is not present in the lumen of the gut, but is present in the cœlomic fluid. L. R. Watson (*Jl. Econ. Ent.*, **20**, 1927, 530–44) has successfully demonstrated instrumental insemination of the queen bee.

O. W. Richards (*Trans. Ent. Soc. London*, **75**, 1927, 233–66), while studying the specific characters of British Humble-bees and in particular in comparing the genera *Bombus* and *Psithyrus*, has come to the conclusion that the specific characters of humble-bees do not appear to have any direct connection with the life-histories of the species. T. H. Frison (*Jl. Econ. Ent.*, **20**, 1927, 522–6) has been able to fertilise and hibernate certain species of queen humble-bees under control conditions.

Among other studies in tropical wasps by F. X. Williams (*Bull. Expt. Sta. Hawaiian Sugar Planters' Association*, **19**, 1928, 1–179), the writer deals with the habits of the wasps of the genus *Larra* which particularly prey on mole crickets. Nine species attacking these insects have been studied ; the various *Larra* species are not indiscriminate in their tastes, but favour a particular species of mole cricket for their young. Mole crickets are agricultural pests in some parts of the world. Descriptions are added of new species of fig wasps which are of special interest as they act as pollinating agents for the species of fig they inhabit. The same writer (*Proc. Hawaiian Ent. Soc.*, 1926, **6**, 1927, 425–64), in an interesting paper on the habits of the bees and wasps of the Hawaiian Islands, showed how, while in certain endemic genera there are large numbers of species, e.g. *Nesoprosopis*, with over fifty species, yet in world-wide genera there are very few species, e.g. *Megachile*, with

only five species at the most. F. Claude Joseph (*Ann. Sci. Nat.*, **9**, 1926, 113–268) has written an interesting and well-illustrated account of the biology of the bees of Chile.

Diptera.—The British Tachinidæ and Platypezidæ are very similar to those of Denmark and so W. Lundbeck's latest volume (*Diptera Danica*, part 7, Platypezidæ, Tachinidæ, 1927) will be indispensable to dipterists in this country. The arrangement of the present volume is based on Villeneuve's recent researches. Four more volumes of *Faune de France* have appeared; No. 13 (1926) by E. Séguy dealing with the Stratiomyidæ to the Omphralidæ; No. 14 (1926) by L. Falcoz on the Pupipara, it will be noticed that the Braubidæ are no longer contained in this group; No. 15 (1927) by M. Goetghebur on the subfamily of midges Tanypodinæ; No. 17 (1927) by E. Séguy on the Asilidæ. All these volumes reach the high standard set by the preceding volumes. H. F. Barnes (*Jl. S. E. Agric. College, Wye, Kent*, **24**, 1927, 65–146) summarises our present knowledge of the British gall midges (*Cecidomyidæ*) known to attack cereal crops, fodder crops, fruit, vegetables, and miscellaneous plants. A new botfly (*Cuterebra bæri*) parasitic in monkeys from British Guiana has been described by R. C. Shannon and C. T. Greene (*Zoopathologica*, **1**, 1926, 285–90). A. L. Tonnoir (*Ann. Mag. Nat. Hist.*, **19** (**9**), 1927, 291–300) describes the acephalous larva of a remarkable nematocerous Dipteron (*Canthyloscclis antennata*) described by F. W. Edwards (*Ann. Mag. Nat. Hist.*, **9** (**9**), 1922, 267). The affinities of this genus are very difficult to determine, since it differs very much from most other Scatopsidæ, Bibionidæ, and Cecidomyidæ. O. Kriebel (*Pub. de la Faculté des Sciences de l'Université Masaryk*, 1927, 1–15) shows that the adipose tissue of Chironomids is composed of two layers—the exterior and interior being quite different from the point of view of morphology and physiology. The interior is to be regarded as having nutritive function, while the exterior has an excretory function. Miss A. D. Bergner (*Jl. Expt. Zoo.*, **50**, 107–154) finds that prolonging the larval or pupal stages or inducing temporary sterility of adult female *Drosophila* causes an increase in the amount of crossing over. It has already been shown that temperature and X-ray does the same. An important account of the circulatory system of Tipulid larvæ with special reference to *T. selene* has appeared by O. Wettinger (*Zeits. f. wissens. Zool.*, **129**, 1927, 453–82).

It is very important in the study of questions relating to the distribution and phylogeny of parasites to know the agencies or conditions which break down natural barriers to their spread from host individual to host individual and host species to species. The Hippoboscid *Ornithomyia avicularia* is known to

carry lice attached to its body, but in each case the host bird has been a different species, ranging from the European blackbird to the European black grouse. It is amazing to find that in nearly all the records of Hippoboscids carrying lice only one species of *Ornithomyia* is involved. H. E. Ewing (*Ann. Ent. Soc. America*, **20**, 1927, 245–50) comes to the conclusion that there must be something in the habits or nature of *O. avicularia* that causes the lice to select this species alone.

Other Orders.—A. E. Emerson (*Zoologica*, **7**, 1926, 69–100) has studied moulting termites in relation to the development of the soldier caste of *Nasutitermes cavifrons* and its phylogenetic significance. R. S. Bagnall (*Ann. Mag. Nat. Hist.*, **20** **(10)** 1928, 304–307) has raised a new superfamily and family for an interesting new species of thrips (*Mymarothrips ritchianus*) from Tanganyika. C. H. N. Jackson (*Ann. Mag. Nat. Hist.*, **19** **(9)**, 1927, 485–97) describes some new Collembola found in a guacharo cave in Trinidad. E. Handschin (*Zeitschrift für wiss. Ins.-Biologie*, **22**, 1927, 295–310) has made a study of the ecology of Collembola on mountain moors.

PREHISTORIC ARCHÆOLOGY. By L. J. P. Gaskin, Librarian to the Royal Anthropological Institute, London.

(*Mr. J. P. T. Burchell is responsible for the section on the "Sligo Palæolithic Implements"*)

Antiquity (March 1928).—Prof. V. G. Childe contributes a well-illustrated article on the "Lausitz Culture," one of the several "urnfield" cultures of Europe which mark the later phases of the Bronze Age. Its distinctive mark is the biconical ossuary. This culture spreads from Silesia and Poland to Moravia and Lower Austria and westward into Thuringia and northward to the Spree; traces of it are to be found in Northern Hungary, Macedonia, and Slovakia. Not the least of the achievements of the "Lausitz Race," which flourished in the second millennium B.C., was the diffusion of the socketed celt.

In an article on "Prehistoric Cart-tracks in Malta" Prof. T. Zammit advances the theory that they were constructed by the energetic Neolithic folk of the island who, as the fields in the valleys could not satisfy their increasing numbers, carried the surplus soil to the hills, made fields and built walls round them. In evidence of this he points out that, though the fields have long since been washed away, traces of the walls still exist.

Journal of the Royal Anthropological Institute (July–December 1927).—In the "Huxley Memorial Lecture," 1927 ("The Neanderthal Phase of Man"), Dr. Hrdlicka conceives Neander-

thal Man as indistinguishable from the " man and period of the Mousterian culture." His argument is that since the phase of Neanderthal Man began towards the end of the warm main inter-glacial period, he would be confronted with the difficulties of hard winters, necessitating search for shelter, and changes in the fauna, necessitating search for food. Climatic changes would, therefore, require greater physical and mental exertion on the part of Neanderthal Man ; the less favoured would die and the more advanced would approach the status of primitive " Homo Sapiens." The skeletal remains of Neanderthal Man bear evidence of such progression, and in art forms he is not far removed from the lower Aurignacian period as the implements from La Quina and the decorated bone fragment from La Ferrassie bear witness. Dr. Hrdlicka believes, therefore, that the phase of Neanderthal Man is a stage in the evolutionary process to " Homo Sapiens," and that he is not, as has heretofore been imagined, of a different race to his immediate successors—the Aurignacians.

L'Anthropologie (tome 38, Nos. 1–2).—Dr. Henri Martin writes on the Solutrean Gallery of Roc (Charente) and its sculptured frieze. The sculptures represent horses and animals of a bovine character. In one scene a man is represented as running in front of a bull, and in another a horse is drawn in bold and vigorous relief together with an animal of bovine character, resembling a bison without the horns. Most of the animals represented are " femmes à gros ventre " which suggests some kind of Fertility Cult. The technique is full of expression and life.

In a note on Palæolithic fishing tackle found in the " Grotte des Harpons " at Lespugue, Dr. R. Saint-Perier draws a comparison between the fish-hooks he has found there and those used by the Esquimaux.

Antiquaries Journal (April 1928).—Excavations in the Mendips. Mr. H. E. Balch reports finds of bronze fibula and bone pins in Wookey Hole. Flint-scrapers, knives and implements from the Rock Shelter at Ebbor ; the most striking implement was a leaf-shaped blade to which Mr. Reginald Smith assigned a Proto-Solutré origin. A neolithic celt and pottery fragments were found in Soldiers Hole, Cheddar Gorge. The results of these excavations show the first evidence for a neolithic occupation of the Caves at Mendip.

Journal Russe D'Anthropologie (tome 15, parts 1–2).—M. Kouftine, in an article on the neolithic station near the village of Lialowo in the district of Moscow, points out that M. Joukow was in error in comparing the Lialowo culture to the early Baltic cultures. As evidence he suggests that the flint material has not been classified by the classes visible, but on a theoretical

plan of M. Joukow's own, and that picks without hafts do not correspond to the Maglemose culture, as according to Johansen they are found bone-hafted. M. Kouftine, on consideration of the perfection of the technique of the pottery, assigns a date not far in advance of the second period of Montelius to the Lialowo culture.

Zeitschrift für Ethnologie (1927, parts 1–2).—In assigning the Palæolithic Industry at Kosten near Lichtenfels (S. Germany) to the Solutrean period, Herr F. Wiegers points out the close proximity of other Solutrean sites, notably those at Neu-Essing and Pottenstein. Solutrean sites are not very frequent in Germany, but it is probable that in the diffusion of this culture from Hungary it passed through the southern parts of Bavaria. The importance of Kosten lies in the fact that it is the most northerly of the Solutrean sites as yet discovered in Germany.

Archæologia (vol. 77, 1927).—Mr. J. Graham Callander writes on recent archæological researches in Scotland. The discovery of harpoons of Azilian type at Oban and in other parts of Argyll indicates a pre-neolithic population. Nor does the evidence for palæolithic man in Scotland end here, for the pygmy implements found near Dryburgh are strongly reminiscent of Tardenoisian sites in England.

Bulletin de la Société préhistorique française (March 1928).—M. G. Drioux has compiled a useful inventory of the Megaliths of the " Haute-Marne," to which has been added an index of the " communes " in which the Megaliths are to be found under the usual headings of Dolmens, Menhirs, Cromlechs, etc. Mdlle A. Hure reports on a palæolithic (Aurignacian) site between Le Portel and Cape D'Alprech (near Boulogne-sur-Mer); shells associated with the industry are to be found on the adjacent littoral. Mdlle Hure has also found neolithic flints in the vicinity.

Revue Anthropologique (January–March 1928).—MM. J. Hamal-Nandrin and J. Servais have made an exhaustive study of the flint industries of the neolithic sites of Fouron-Saint-Pierre, Fouron-Saint-Martin, and Remersdael and the neolithic sandstone industry of the forests of Fontainebleau and Montmorency. The similitude existing between the implements of the two industries is very striking; they belong to the early neolithic period, usually known as Campignian. The article is well illustrated.

Anzeiger für schweizerische Altertumskunde (1927).—Herr G. Kraft in a long, well-illustrated article on the position of the Swiss in the Bronze Age cultures of Central Europe, surveys the whole of the Bronze Age cultures of Switzerland and compares them with those of Europe. He notes that there were

two different incursions into Switzerland following one upon another; one from Upper Italy (Mels-Rixheim), which he correlates with the middle of the Hallstatt period, and the other from the northern side of the eastern Alpine provinces (Oberendingen), which he compares with an early type of Hallstatt culture.

Præhistorische Zeitschrift (18, parts 1–2).—Contains a long and well-illustrated article by Hubert Schmidt on Scythian horse-trappings. Herr Schmidt discusses the technique of the art forms employed, and points out traces of influences from Greece, Babylonia, Mesopotamia and the East. He dates the Scythian culture as between the end of 700 B.C. and the beginning of 600 B.C.

Bullettino di Paletnologia Italiana (46, parts 2–3) contains an article by I. Cafici on the prehistoric site at Scalona, Syracuse (Sicily). In attributing the finds to the Mousterian period, he points out the affinities existing between the palæolithic industries in Northern Africa and those in Sicily. The article is illustrated.

Mitteilungen der anthropologischen Gesellschaft in Wien (58, parts 1–2).—In a long and profusely illustrated article, Dr. W. Jenny discusses the cultural spheres of the Danubian decorated pottery. In tracing the influences on the ornamentation and motive of this pottery, Dr. Jenny instances the effect of the Northern and Near-Eastern forms of ornamentation. For his Northern group he includes the pottery of Scandinavia, N.W. Germany, Thuringia, Saxony, Bohemia, and Moravia, and for the Near-Eastern and Central groups the pottery of N.W. Hungary, Slovakia, Bulgaria, the Ukraine, and Asia Minor.

Pravek (part 3, 1927).—MM. Chleborad writes on two prehistoric cemeteries at Sardicky near Bucovice in Moravia. The period assigned to these cemeteries is that of the Aunjetitz Culture, early Bronze Age (2000–1200 B.C.), which preceded the Lausitz Culture. Eighty-nine graves have been excavated and 157 vessels, associated with the graves, were found; these have been listed and placed in the Museum at Brno (Brünn).

Eurasia Septentrionalis Antiqua (I, 1927).—A. M. Tallgren contributes a bibliography of the Archæology of East Europe, 1918–26. This consists for the greater part of Russian works, though Prof. Childe's valuable *Dawn of European Civilisation* and *The Aryans* make their appearance. It is well annotated.

Prof. P. Rykov writes on the Chvalynsk Culture of the Lower Volga. This is a Bronze Age culture which Prof. Rykov connects with that of the Caucasus; the frequent appearance of Siberian cultural elements is very marked.

In an article on the cemetery at Abachevo, Prof. V. Smoline connects the bronze finds with the Chvalynsk Culture and with the Bronze Age Culture of the Caucasus; he assigns them as a date the second half of the second millennium B.C.

The Sligo Palæolithic Implements

On August 20 last Mr. J. P. T. Burchell put forward the claim, in *Nature*, of having discovered, on the west coast of Ireland, limestone implements of the Early Mousterian period. The artifacts were found under three specific circumstances:

(*a*) Incorporated in present-day beach material.
(*b*) Under what Mr. Burchell claims to be a collapsed rock-shelter situated just within reach of the sea during severe winter storms.
(*c*) Embedded in the Lower Boulder Clay of the neighbourhood.

These specimens were carefully examined by Mr. J. Reid Moir, who, upon archæological grounds, endorsed Mr. Burchell's contention. Following upon these pronouncements a committee consisting of Prof. R. A. S. Macalister, Prof. J. Kaye Charlesworth, Dr. Lloyd Præger, and Mr. A. W. Stelfox visited the sites and arrived at the following conclusions:

(*a*) That the specimens in question were nothing more than angular pieces of beach material resulting from modern sea-action.
(*b*) That the rock-shelter was nothing more than a modern storm-beach.
(*c*) That in view of the nature of the Boulder Clay deposits themselves the possibility could not be admitted that any of the specimens collected therefrom were of human origin.

It should be pointed out that no member of this committee, at the time of the issue of the above report, had examined the artifacts collected by Mr. Burchell. In view of this adverse opinion a committee consisting of Mr. A. L. Armstrong, Mr. M. C. Burkitt, Mr. H. Dewey, Miss D. A. E. Garrod, and Mr. Reginald A. Smith made a critical examination of a typical series of specimens recovered from the rock-shelter site. It was unanimously decided that the specimens represented human artifacts. The question as to the cultural age of the artifacts did not form part of the inquiry. Subsequently, at the request of the Irish Committee, Prof. P. G. H. Boswell of Liverpool University, and Prof. O. T. Jones of Manchester University, examined the sites in dispute and endorsed the geological views

previously expressed by this committee. In the middle of April this year Mr. Burchell carried out further investigations in the Sligo area, accompanied by Mr. C. Blake Whelan of Belfast, and it is understood that the discoveries arising therefrom are of considerable importance. Mr. Burchell will make public his results in a paper to be read before the Society of Antiquaries of London in November next, when he will reconcile the geological and archæological evidences.

Norsk Geologisk Tidsskrift (vol. x, part 1, 1928).—In 1905 the geologist N. Hartz, whilst engaged in investigating the neighbourhood of Harebjerg, one kilometre south of Brorup station in Jutland, found a flint implement lying twenty-three feet deep in drift, which for the most part was laid down during the glacial period. This find, hitherto unpublished, provides the long-sought evidence of man's existence in Scandinavia during one of the inter-glacial periods of the Palæolithic Age. In view of the occurrence of the implement, *in situ*, in the drift, the geologist could have no doubt that it dated from a period prior to the last glaciation, though he could not decide with certainty to which inter-glacial it belonged. It may be regarded as certain, however, that the specimen belongs to the latest. The implement agrees remarkably with the geological evidence, being a small *coup-de-poing* of fairly rough workmanship, carefully though coarsely chipped on the side edges. It is clear that here we have an artifact of a type unknown in Denmark, but for which there are plenty of parallels in the Palæolithic period of Western Europe.

NUTRITION.

By Leslie J. Harris, D.Sc., Ph.D., Nutritional Laboratory, Cambridge.

Mode of Action of Vitamin D. Question of Regulation of Gut Acidity

The present output of papers on the vitamins—A, B_1, B_2, C, D, E—shows a popular predilection for D. More papers have been published on this vitamin during recent months than on all the others put together. The B vitamins make a good bid for the second place.

Notwithstanding the large number of workers, the multitude of fresh observations recorded, and the undoubted importance of many of the new facts brought to light, it must be confessed that but little headway has been made towards any adequate understanding of the mode of action of vitamins. Thus recent intensive work by Drummond and his collaborators has served only to show that almost all the symptoms hitherto attributed specifically to vitamin B deficiency are actually caused by

starvation through failure of appetite; the primary question as to the mode of control of appetite by the vitamin remains unanswered.

Vitamin C remains largely a neglected field. Vitamin A having been only recently disentangled from D, its effects have been inadequately studied free from complications due to the latter. Vitamin E is still in the descriptive stage,—descriptive of the foodstuffs containing it and of the net, *final* effects of absence. A suggestion that the efficacy of the Minot-Murphy liver treatment for pernicious anæmia was due to the presence of vitamin E and a large iron content (Simmonds, Becker and McCollum, *J. Biol. Chem.* **74**, 1927, p. lxviii) has been disproved (Cohn *et alia*, *ibid.*, p. lxxii).

In the case of vitamin D deficiency we are beginning to learn something about the chain of phenomena which terminate in the avitaminosis, *i.e.*, in rickets.

Nature of Rickets

A word of explanation may be necessary for the reader who is learned in chemistry more than biology. It is sufficient to define rickets rather loosely as an insufficient deposition of calcium salts (calcium phosphates mainly) in the bones, a consequent insufficient rigidity of the bones and a resulting deformation.

What is " the cause " of the disturbed calcification ?

Cause not in the Bone

The enzyme discovered some years ago by Robison in bones, the " function " of which is the hydrolysis of the phosphoric esters with which the bones are supposed to be supplied by the blood, is not at fault in rickets; its activity has been shown to be fully normal (Robison *et alia*, *Biochem. Jour.*, 1924, 1925, etc.). Bones from rickety animals, moreover, are found to calcify normally when immersed (a) *in vitro*, in suitable calcium solutions, or (b) in normal *blood* serum itself (Shipley, Kramer and Howland, *Amer. J. of Dis. of Children*, **30**, 1925, p. 37). The inquiry must therefore be pushed a stage further back. It seems likely that one contributory cause at least of rickets must be a deficiency in the supply of some forms of phosphorus or calcium, or both, in the fluid which bathes and feeds the bones, the blood.

Chemistry of the Calcium and Phosphorus of the Blood

Kramer and Howland claimed a few years ago to show that in rickets there is always a subnormal concentration of either

or both these elements in the blood (*Bull. Johns Hopkins Hospital*, **33**, 1922, p. 313). Now Holt, La Mer and Chown (*J. Biol. Chem.*, **64**, 1925, pp. 509, 567) have maintained that the blood contains calcium phosphate and carbonate in the form of a *metastable supersaturated solution* which would readily deposit the solid phase on coming in contact with bone. Robison, however, considers it "unjustifiable to regard blood as such a supersaturated solution until one knows more concerning the factors which control the degree of ionisation and solubility products of the various components of such complex systems." More detailed physico-chemical studies have been published recently on the solubility of calcium salts in salt solutions and in biological fluids under various conditions, by Sendroy, Hastings and Murray (*Journal of Biol. Chem.*, **71**, 1927, pp. 723, 783, 797). These authors provide a detailed mathematical treatment of mass law equations under varying conditions of pa_{H}^{+}, and they deal at length with the relations between dissociation constants, activity coefficients, solubility products and ionic strength. They conclude that the theory of supersaturation is inadequate; that calcium exists in serum in abnormal amounts bound to some substance or substances which hold it in solution in unionised form. Many other papers have appeared on the non-diffusible calcium in the blood serum (Upergraff, Greenberg and Clark, *J. Biol. Chem.*, **71**, 1926, p. 89) on the influence of proteins upon the solubility of calcium phosphate (Csapo, *ibid.*, **75**, 1927, p. 509) and on the ionic condition of phosphorus in blood (Grollnan, *ibid.*, **72**, 1927, p. 565), and so forth, to quote rather at random. While much is still in dispute we may venture to assume with confidence that the major defect in rickets, as indicated above, lies not in the ossifying tissues themselves, then, but in the calcium or phosphate or both in the blood.

Proceeding with the analysis, we must inquire the reason for this defective supply.

Calcium and Phosphate Retention

It is still believed by some authors that there is no reason to suppose that the total calcium (or phosphorus) of the blood is appreciably altered in rickets (see Pryde, *Recent Advances in Biochemistry*, London, 1928, p. 192). Evidence is, however, rapidly accumulating to the contrary. Thus, with rachitic rats as experimental animals, a greatly diminished retention of calcium in the body is observed, compared with normal rats. Likewise the amount of phosphorus retained falls; in fact, the balance may become negative, more phosphate being actually lost from the body than is taken in by the growing animal in

the food (Yoder, *J. Biol. Chem.*, **74**, 1927, p. 329). Indeed in a paper just published Shohl and Bennett (*Journ. Biol. Chem.*, **76**, 1928, p. 633) claim that measurements of calcium and phosphorus retention are preferable even to bone analyses for a quantitative measurement of rickets. Their conclusions are based on work with dogs as well as rats.[1]

Pediatricians no less, like Parsons in this country and Holt in America, have investigated the failure of the infant to assimilate calcium or phosphorus in cases of rickets and marasmus.

The final stage in our present analysis must be to inquire the causes for this faulty retention of phosphate and calcium. There is strong evidence as the result of recent work to suggest that it is due to faulty absorption into the blood-stream from the gut, and that this in turn is related to intestinal reaction—*i.e.*, the p_H of the contents. Further than this one cannot go in the present state of our knowledge.

The Acidity of the Gut as a Limiting Factor

Zucker and Matzner discovered in 1923 that the *fæces* of rats became alkaline when the diet was rachitogenetic; that normal rats had acid fæces; and that as soon as rickets was cured or prevented, as, e.g., by the addition of vitamin D, the fæces were again acid (*Proc. Soc. Exp. Biol. Med.*, **21**, 1923, p. 186). This was followed by the observation that the same type of changes occurred in the contents of the *intestine* of the rat. These important conclusions have been abundantly confirmed (Jephcott and Bacharach, *Biochem. J.*, **20**, 1926, p. 1350; Redman, Willimott and Wokes, *ibid.*, **21**, 1927, p. 589; Yoder, *J. Biol. Chem.*, **74**, 1927, p. 321). Strangely enough little attention has been attracted to such work in this country, and not more than three or four papers have appeared here having any direct bearing upon it.

Grayzel and Miller (*J. Biol. Chem.*, **76**, 1928, p. 423) have just shown that in dogs there is the same transformation, the contents of the whole length of the intestine becoming more alkaline in experimental rickets, and reverting to the more acid, normal reaction when a cure is instituted by cod-liver oil (vitamin D) or by ultra-violet light. The same result has been obtained with the guinea-pig by other workers. The rapidity with which the reaction of the intestinal contents is altered by means of ultra-violet light is remarkable.

In the current issue of the *Biochemical Journal* (vol. 22, 1928,

[1] In the rat there is a second factor in addition to lack of vitamin D (or ultra-violet light, its generator) which is causative of experimental rickets —viz. a diet ill-balanced in its calcium and phosphorus content.

p. 61) Jephcott and Bacharach go so far as to utilise the phenomenon for the quantitative estimation of vitamin D, on the basis that the lowering of fæcal p_H of rats is a function of the amount of vitamin D administered.

On the other hand, Redman (same Journal, p. 15) fails to find any definite correlation between the p_H value of the fæces of rachitic patients and the severity of the rickets. This disappointing result appears to be contrary to that reached by Flemini (*La Pediatria*, **34**, 1926, p. 625). His experimental method was the determination of the quantity of soda or sulphuric acid necessary to neutralise fæces: he concluded that the administration of vitamin D to infants receiving calcium lactate did transform the intestinal contents from alkaline to acid. He also claims that the *blood* was in all cases rendered more alkaline by this treatment.

The reality of the connection between the degree of acidity of the intestinal contents and the amount of absorption through the wall of the gut is attested along a number of independent lines of inquiry.

Nature of the Mechanism by which p_H controls Absorption

(i) Decreasing alkalinity is known to *increase the solubility* of calcium, and it must obviously affect also the solubility of the phosphate and other ions (Holt, *J. Biol. Chem.*, **64**, 1927, p. 578).

(ii) There is direct evidence that this change in solubility of the materials in the intestine is in turn reflected in their *absorption* into the blood or ultimate loss (*e.g.* Irving and Ferguson, *Proc. Soc. Exp. Biol. Med.*, **22**, 1925, p. 527).

(iii) Experimental ingestion of large excess of acid or alkali has been found to affect calcium and phosphate metabolism. Addition of alkali (sodium carbonate) to a "borderline" diet is sufficient to convert it into a rickets-producing diet, while addition of a potential acid (ammonium chloride) is sufficient to convert the same diet into a non-rickets producing one (Zucker *et alia*, *Proc. Soc. Exp. Biol. and Med.*, **20**, 1922, p. 20). The addition of hydrochloric acid to the diet of rickety children has a definite curative effect.

(iv) Similarly the ratio of amounts of mutually precipitating substances, calcium and phosphate (or fatty acid), in the diet has been shown to affect their utilisation in the body.

Any attempt to proceed further and indicate by what means a mere trace of vitamin D in the diet is able to make the alkaline gut acid is little better—in to-day's lack of knowledge—than pure conjecture.

Possible Modes in which Vitamin D may Regulate the Intestinal Reaction

In the first place it may be noted that the recent paper by Grayzel and Miller referred to above—where incidentally, as in Yoder's paper, a useful bibliography may be found—reports that the *in vitro* digestion of (i) anti-rachitogenetic and (ii) rachitogenetic diets yields a product of the same p_H in both cases. Further, any variation of the normal (non-rachitogenetic) diet within ordinary limits, while having naturally some slight effect on the intestinal reaction in dogs, did not bring it into the abnormal alkaline zone characteristic of a rachitogenetic regime.

Various speculations might be ventured of possible alternative mechanisms. The first three below are due to the last-named authors :

(*a*) There may be some change in the p_H of the gastric or intestinal *secretions* (peptic and pancreatic juices).

(*b*) There may be some change in acid or base *absorption* by the gut, governed—like (*a*)—either (1) by nerve control, or (2) by direct action of some substance on the epithelial cells themselves.

(*c*) There may be some change in the acidic or basic *excretion* into the gut, involving systemic conditions, since the intestinal tract appears to be involved in the maintenance of the acid-base equilibrium of the body.

(*d*) In the present writer's view the rôle of vitamin D should be considered in relation to the properties of the emulsifying agents at a colloidal interface, affecting the stability of the emulsion, and in turn the stability of the phasic system, the solubility of the components, and hence the acid-base mass law equilibrium. We have to realise that the liquid phase with which we are dealing is a massively buffered system, so that the considerable changes in its acid-base equilibrium brought about by infinitesimal quantities of reagent can only be effected by polyphasic means.

Any temptation to take an unduly simplified view or to seek too one-sided an explanation may be checked when it is realised that we are dealing with a complex series of interrelated phenomena, in which are involved the influences of other factors so diverse from our earlier discussion as, for example, the thyroid secretion (Hammett, *J. Biol. Chem.*, **72**, 1927, pp. 505, 527, etc.), or even, it is claimed, the gonads (Mirvish and Bosman, *Q.J. of Expl. Physiol.*, **18**, 1927, p. 10). Nor have we emphasised the many-sided nature of the mechanism regulating the acid-base equilibrium of the body, nor alluded to the effects of calcium upon the permeability and irritability of muscle and nerve cells.

To sum up, we may say that there is considerable evidence for the idea that "the primary defect in rickets which is corrected under ultra-violet treatment" and by the other curative agents "may be the absorption of phosphate (or calcium, or both) from the gut, with the hydrogen-ion concentration as a limiting factor." Graphically,

> Vitamin D → increased acidity in gut → increased solubility of phosphate → increased absorption into blood-stream → increased deposition on bone.

The path of the first arrow is completely obscure.

ARTICLES

ULTRASONICS

By PROF. R. W. BOYLE

University of Alberta, Canada

WAVES of the same type as waves of sound, but at pitches too high for audibility, are ultrasonic. Just as physicists, since Ritter's discovery of ultra-violet light in 1801, have had the interest to investigate a span of light-waves beyond the visible, so they now may investigate an easily producible range of sound-waves beyond the audible. The frequency at the threshold of audibility, at this higher end of the scale, is about 20,000 vibrations per second; above this the frequencies for human ears are ultrasonic.

From long ago a few experiments, mostly qualitative, on high-pitch longitudinal waves have been performed at spasmodic intervals. The sources of such vibrations were unable to supply acoustic energy in appreciable amounts; but they were useful in a few types of laboratory demonstration to exemplify the principles of wave physics. Among such sources may be mentioned high-pitched short pipes, Galton's whistle, short metal rods vibrating longitudinally or transversely when hit, high-pitched tuning-forks, bird-calls, electric arcs.

This article cannot include accounts of the few older types of work; it confines itself to newer methods devised in recent years, and at that must be limited to the principle of production and properties of the waves, rather than to their engineering and other (*e.g.* naval and navigational) application.

The broad physical principle involved in the modern production of ultrasonic beams is that of Diffraction. The late Lord Rayleigh [1], without foreseeing ultimate developments, was the first to associate diffraction principles with the production of high pitch-sound.

The development of modern practice in ultrasonics virtually began in 1912, just after the disaster to the famous Atlantic steamship *Titanic* by collision with an iceberg. Not long after this event a proposal to employ the echoes of high-pitched acoustic waves for preventing such disasters was put forward

by Lewis Richardson [2]. Although the devices then suggested would not have accomplished their purposes in any practical sense, Richardson's specifications clearly outlined the essential features of under-water detection and location, by ultrasonic "beams," of submerged objects, like rocks, wrecks, mines, submarines, and also of secret, directive, under-water signalling. All the results which Rayleigh, and, later, Richardson, foresaw and predicted from optical analogies and acoustic theory, to-day have been accomplished by experiment, and in addition a mass of information unforeseen by anyone has been acquired.

Ultrasonic Beams

The greater the frequency of a system of waves in any medium the shorter will be the wave-length, for frequency and wave-length are related by the simple product $V = n\lambda$, V being velocity, λ being wave-length, and n being frequency. The velocity of *longitudinal* elastic disturbance (sound-waves) in water is about 1,500 metres per sec. ($1{\cdot}5 \times 10^5$ cm. per sec.), so that at a frequency of 50,000 ∽/sec. the wave-length is about 3 cm. In air the velocity is $0{\cdot}34 \times 10^5$ cm. per sec., the wave-length at the same frequency would be about 7 mm. Contrast these lengths of wave with those at ordinary frequencies. A note of, say, frequency 500 ∽/sec. would have a wave-length in water of 300 cm. or nearly 10 ft., and in air 68 cm. or over 2 ft. It can be grasped immediately that it is hardly practicable to design sound transmitters and receivers, for ordinary frequencies, and keep their linear dimensions of about the same length or larger than a wave-length. Yet this is an important condition in any system of sound radiation, for on it depends the *directivity* of the radiated beam.

The theoretical case is somewhat (but not quite) similar to the older one of Optics. When *plane* waves of *monochromatic* (one wave-length) light pass through a tiny circular opening in an opaque screen, the light disturbances in the aperture will result in the production beyond the screen of approximately conical, co-axial, zones of alternate maximum and minimum intensity around a central axis. When the impinging wave-fronts are parallel to the plane of the aperture, the central axis of the diffracted "beam" will be at right angles to the aperture through its centre. The ideal criteria by which these diffraction effects may be achieved are: (1) that the diameter of the aperture or source should be large in comparison with a wave-length, and (2) that all points in the plane of the aperture or source should vibrate with the same amplitude and in the same phase. As far as concerns sound the first condition is readily fulfilled if we cause any practicable size disc, say up to 20 inches

diameter, to vibrate longitudinally at such a high pitch that the emitted wave-length is short in comparison. Thus frequencies from 20,000 to over 1,000,000 ∽/sec. have come to be employed depending on the object required. The second condition is almost impossible to achieve practically for reasons which will be indicated later, but an approximation will prevail if the disc is set vibrating by the methods later described.

If ultrasonic beams are employed in sea-water, calculations, now supported by experiment, show that the surfaces of submerged rocks and reefs, and the hulls of mines, submarines, and surface ships, act as nearly perfect reflectors, and in consequence their proximity may be disclosed by the "echo" produced by their reflections of the waves. The proximity of icebergs, shores, submerged wrecks, under certain conditions may be detected in the same way. The distance of the reflecting obstacle from the transmitting source may be estimated from the interval of time it takes for the signal to go and the echo to return. Thus the beam may be made a means to disclose the distance and bearing of a submerged object from the emitting source.

Also the beam may be employed as a device for under-water signalling, and such a system has the advantage of being secret and directive. Within the range of transmission a submarine suitably equipped could telegraph while submerged to another equipped submarine or surface ship, thus rendering it unnecessary to come to the surface as would be the case to send a wireless message. It is possible also to telephone under water along the beam, by using the ultrasonic waves as carrier and modulating them through a microphone by the fluctuations of the voice in precisely the same way as electro-magnetic waves are modulated in radio-telephony.

All these phenomena have been realised in practice. The basic ideas contained in Richardson's original specifications were revived in 1914 by M. Chilowski, and in a short time were brought to the notice of Prof. Paul Langevin of Paris, with the result that in 1915 Langevin and Chilowski began experimental work. In 1916 Langevin and his assistants succeeded in producing the vibrations in the water of a laboratory tank and later in the water of the Seine. In a series of brilliant experiments [3] they obtained a range of under-water signalling of three kilometres, and a detectable "echo" by reflection from a large iron plate at a distance of 100 metres. The frequency employed was about 100,000 ∽/sec., a special carbon microphone being used for reception. The method was an electric one, the ultrasonic generator consisting of an electric condenser with a very thin mica sheet as dielectric inserted between an insulated steel plate and the water. A vacuum

was necessary between the plate and mica. Electric oscillating voltage of the required high frequency was imposed on the condenser, and the periodic compressional stresses thus set up by the oscillating electric field produced longitudinal vibrations which were communicated to the water. Reception was accomplished by the microphone connected to a "tuned" receiving circuit of inductance and capacity as in the methods of wireless telegraphy. In addition it was necessary to magnify greatly the received effects by using an electron valve-amplifier of high power, which instruments were just then coming into service. In this way the modern aspects of ultrasonic research took shape, and it is proper to remark that to Langevin more than any other are the subsequent successes in this subject due. The writer enjoyed the privilege of attending and assisting at some of his experiments. The subject expanded in France and England, and later in America. English workers, among whom may be mentioned Messrs. Rawlinson, B. C. Smith, Anderson, G. Stephenson (deceased), Hunter, Bacon, Pew, Nightingale, Lunt, Trigle, and in later years others, have pushed forward theoretical and practical researches with great skill and vigour; it is unfortunate that as yet their results have not appeared in print. Were this article to be concerned with the applications and engineering rather than with the physics of ultrasonics, it would not be at all complete without including a full account of the great services rendered by the group just mentioned.

Ultrasonic Generators

Many methods have been tried to produce a satisfactory ultrasonic generator, that is, one capable of emitting significant amounts of energy and at the same time be amenable to adjustment and control. Basic principles of mechanical impact, electrostatics, electrodynamics, magnetostriction, electrostriction, have all been invoked; but not much more than a laboratory device was obtained until the principle of the piezo-electricity of quartz was hit upon. The idea arose in France and England at about the same time, and Langevin on trying a piece of quartz instead of a carbon microphone as an ultrasonic receiver found it to answer well when coupled with a receiving amplifier of high power. Immediately quartz transmitters were found to work, and from the useful properties of piezo-electricity it follows that the same instrument may be used as receiver and transmitter.

To-day there exist various types of ultrasonic generators large and small, weak and strong, answering to different purposes. Mention may be made of the small metal bar, held at the centre and vibrating longitudinally when struck on the end

by a hammer [4]; the electrostatic transmitter of Wente [5]; the electro-dynamic oscillator of Hewlitt [6]; and the air-jet oscillator of Hartmann [7]. All these and other types of small generator are no more than laboratory devices. The type of generator still in commonest use in laboratory work and application is that based on the principles of piezo-electricity of quartz. Eventually quartz may be superseded, for its use under water involves certain difficulties, and it can be obtained in sufficient purity mechanical and electrical only by accompanying expensive waste. It is desirable to be able to construct, out of ordinary materials and without depending on the use of a naturally occurring and defective crystal, a practical oscillator of sufficient power. For adequate energy emission at frequencies above 20,000 vibrations per second, the quartz oscillator is best; but if the service it has thus far rendered may be performed at high *sonic* instead of *ultrasonic* frequencies, say 10,000 to 15,000 ∽/sec., the electro-magnetic oscillator may come into its own. Such devices become more difficult to construct and maintain, and the radiation from them becomes weaker and less controllable, as the frequency of vibration is advanced.

Piezo-Electricity [8]

All expositions of piezo-electricity beyond the following short statement must be omitted from this article, for lack of space. A crystal displaying the phenomenon when subjected to pressure along a certain axis (the "electric axis"), develops opposite electrifications at its ends. On being subjected to equal tension the same quantity of electricity is liberated but the polarity is reversed. This is the underlying principle of ultrasonic receivers. Reversibly, if a quantity of electricity is applied to the faces of a plate of the crystal, cut perpendicularly to the electric axis, thereby establishing a difference of potential, there will be a dilation of the plate in the direction of the axis; if the electric field is reversed there will be equal contraction in the same direction. This is the principle of ultrasonic generators.

Taking all factors into consideration quartz is the most suitable crystal for practical ultrasonic instruments. Its use involves certain complications because of the facts that the quartz crystal has *three* electric axes, at 60° to one another, and there are *two* perpendicular mechanical vibrations produced by an alternating electric field applied along any one. Harrison [9] has shown that four sets of vibrations result from an alternating field on a quartz plate appropriately cut. In ultrasonic instruments the plates are cut perpendicularly to

an electric axis, and conditions of use are so arranged that the vibration more particularly required is made dominant.

Construction of Transmitters and Receivers

Quartz crystals for use are carefully selected, and the plates cut from them carefully tested; pieces turning out to be worse than a chosen quality are cut away. Thus, generally, layers of active quartz consist of mosaics, the constituent pieces being

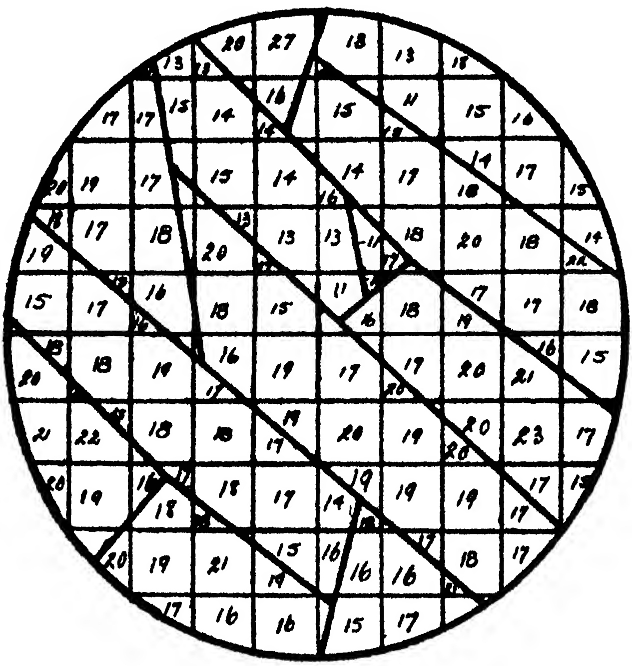


FIG.1

either of irregular shape or of standard regular pieces like rectangles or squares. Fig. 1 represents a quartz mosaic (10 inches diameter), with comparative numbers on it to indicate its variation of piezo-electric quality.

The instruments used for both transmission and reception are made alike, and the same instrument may be used for both purposes. In general a transmitter or receiver consists of an electric condenser, in which a quartz mosaic is the dielectric and metal plates the electrodes. The quartz layers should be of uniform thickness, smooth, and preferably polished. Since varying pressures must be transmitted across the interfaces

FIG 7.

FIG. 4.

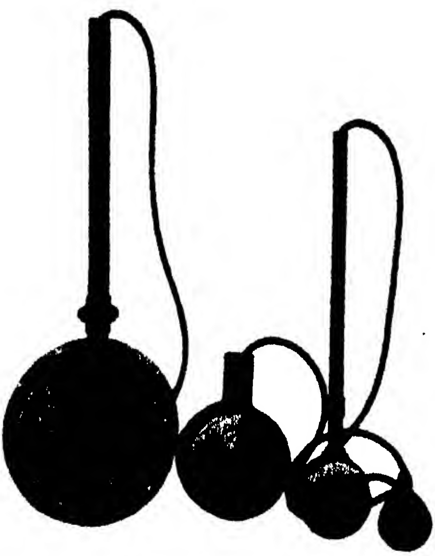


FIG. 2.

FIG. 5.

PLATE I.

between the quartz and metal, the closer the plates are pressed and cemented together the more energy dissipation is avoided. Hence polishing the plates, binding them together with good insulating cement, exclusion of air bubbles, and application of static pressure to bind the whole together in firm contact, all help towards copious energy emission.

The condenser may consist of a single metal plate with the quartz layer between it and a thin metal protecting plate in contact with the propagating medium. When the instrument is small a thin sheet of mica may serve for protection. Or, the condenser may consist of two similar metal plates with the quartz between them ; this was the type first introduced by Langevin to increase the energy emission. Or, it may

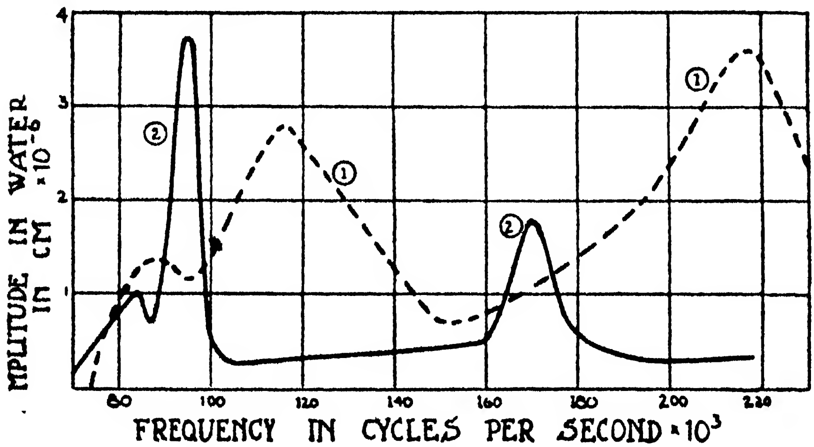


FIG. 3

consist of a pile of quartz and metal plates interleaved and alternately connected as in a " pile of plates " condenser ; the quartz plates in this case are situated at the nodes of vibrational displacement. In all cases the condenser is enclosed in a metal case fitted with an insulated cable connected to one electrode, and the case and other electrode generally are earthed. All empty spaces inside the case are filled with insulating mixture Fig. 2, Plate I, is a photograph of four laboratory transmitters, the largest of which has a radiating face of 10-inch diameter, the smallest 1·5 inch.

The piezo-electric condenser, when excited by an oscillating voltage, constitutes a mechanical vibrating system of appreciable thickness, possessing a fundamental resonant frequency of *longitudinal* vibration and many overtones. The most copious energy emission occurs at the fundamental resonant frequency,

but the emission has other maxima at the frequencies of overtones. At frequencies away from resonance the energy emission is relatively low. Curve 1, Fig. 3, indicates the energy emission at varying frequency in a certain case of a single plate transmitter, Curve 2 from a double plate. (These cases are not comparable.)

In the single-plate instrument, if the quartz layer is thin in comparison with the thickness of the metal plate, fundamental resonance occurs when the metal plate is a quarter wave-length thick, and at this frequency there is greatest radiation of energy. In the double-plate instrument the resonant frequency occurs when the plates are each a quarter wave-length, the total thickness being then a half wave-length. In any ultrasonic condenser the internal vibrating system is complicated. Undoubtedly there are internal nodal regions caused by internal multiple reflections and interferences, and there is loss of energy through the sides by transverse and other vibrations.

Researches

Detection of Ultrasonic Beam

When an ultrasonic generator is immersed in a propagating medium, for example water, and excited by electric voltages of the required high frequency, ultrasonic energy is radiated into the medium mostly in the form of a " beam " concentrated around the central axis. Various methods have been devised to detect and follow the course of this radiation, but those most useful are based on the phenomenon of radiation pressure. The Rayleigh disc, depending on another phenomenon [10], has been used by the writer in both air and water ; but for convenience in quantitative measurements a torsion pendulum operated by the radiation pressure of the waves is preferable.

Maxwell was the first to deduce that light radiation exerted a forward pressure on objects on which the light shone. Rayleigh [11] and also Larmor [12] deduced that the same phenomenon must exist in the case of sound, and Altberg [13] first applied it when he measured by radiation pressure the absolute intensity of the sound radiated from the end of a Kundt's tube. With ultrasonics it is easier to demonstrate the existence of the pressure, and recourse may be had to this phenomenon for either comparative or absolute exact measurements. If pieces of cork or hollow celluloid balls are suspended like inverted pendula in front of an ultrasonic generator of moderate power in a tank of water, on driving the generator these light bodies will be deflected from the generator and tend forward in the direction of the beam [14]. A stream of air bubbles released in the beam will be deflected from its vertical path and sent in

the direction of projection of the beam. In magnitude the radiation pressure upon a perfectly reflecting object is equal to *twice the energy density* in the incident waves, while upon a perfect absorber the pressure is equal to the energy density alone.

Visualisation of the Beam

A helpful *visualisation* of the formation of an ultrasonic " beam," and the manner in which its energy is distributed in various zones, is obtained by a " dust figure " method used by the writer and assistants [15]. If small, light, dust particles are sprinkled in front of a generator in water, the radiating face of the generator being vertical, the particles as they sink are driven forward by the pressure of the radiation. The particles should, of course, be of specific gravity greater than the water, preferably just greater, and of small enough size to sink *slowly*. As the sinking particles are driven forward a majority of them will be jostled from the regions of maximum to the regions of minimum energy intensities. If a flat table or tray be held to catch these particles at a level just below the centre of the generator, they settle on the tray in definite lines, which are the traces of the surfaces of minimum intensity on the plane of the tray. In other words, the particles mark out the traces of the interference nodal surfaces of the complex energy field on this plane. After the experiment the water can be drained from the tank leaving the dust figures exposed on the tray, and photographs of them may be taken. This method, though qualitative only, helps to envisage what takes place in an ultrasonic field close to a generator, and indicates also the presence of the " side-beams," or outer zones of energy. It also assists in a clear visualisation of the manner in which the main, central beam is formed, indicating at the same time the position of maximum and minimum points of energy intensity along the central axis. In spite of the fact that the dimensions of the dust particle employed may be small in comparison with the wave-length, this experiment can be carried out quite successfully. As the length of wave increases the size of particles employed should also be increased, provided they are not too large for slow sinking through the water ; if the particle is too small the wave *diffracts around it* and the radiation pressure on it becomes less effective. (Of the different kinds of particles yet employed the most satisfactory have been the dust of burned and washed coal cinders, which will just sift through sieves of 60 and 70 meshes to the inch.)

Figs. 4 and 5, Plate I, illustrate two dust figure representations of an ultrasonic " beam." Both were produced from the same generator, the diameter of radiating face being 15·3 cm.

In the first figure the operating frequency was 129,000 ∽/sec.; in the second 178,000 ∽/sec. In the first case the ratio $\frac{\lambda}{D}$ was 0·076, and in the second 0·055; it is on this important ratio that the form of the figure largely depends.

The photographs show that in the case of the lower frequency the main beam is wider than in the second case where the frequency is higher. They show also that the main beam virtually begins at some distance from the generator. Also it can be noticed that the nodal surfaces in the water reach up to the emitting face. Since the generator itself has appreciable thickness there can be no doubt that complicated nodal regions exist inside it, the position and form of the surfaces depending on the dimensions and the frequency of vibration. We see that the generation of ultrasonic beams is much more complicated for mathematical solution than the ideally simple analogy of a pin-hole " aperture " in optics.

In the above experiments the reflections of the waves from the tray which catches the dust figures interferes to some extent with the primary beam from the generator, but not enough to spoil a good representation of the beam formation.

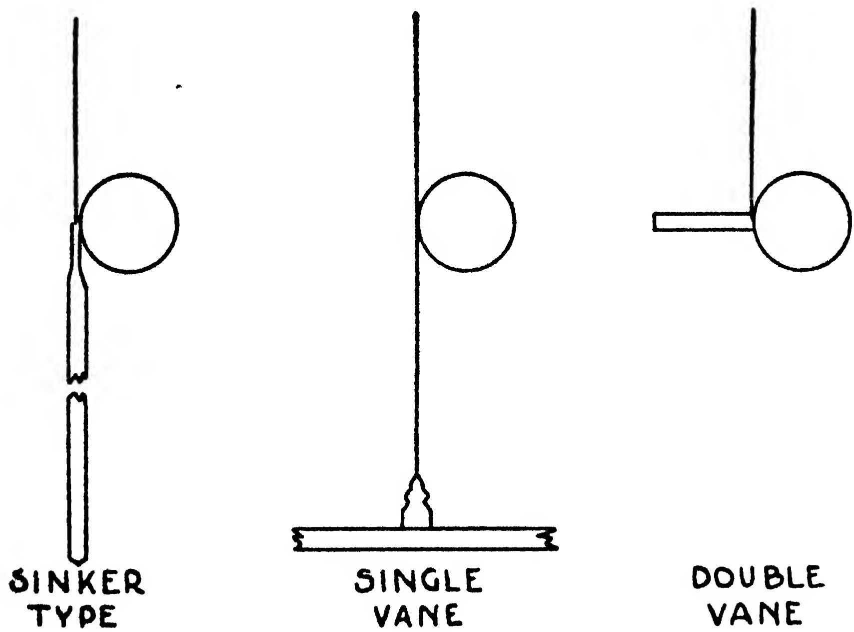
Audibility Method of Tracing the Beam

This method, at times very convenient for quick qualitative work, is also based on radiation pressure and makes use of a simple mechanical listening device [16]. A small thin-walled rubber teat, or better still a small capsule with very thin sides of mica, attached to the ear-tubes of a good stethoscope, is quite adequate. Ultrasonic waves themselves are inaudible, but the position and path of an ultrasonic beam in a liquid may be detected and followed by this device if the waves are emitted of sufficient strength *in pulsations* constituting a " tonic-train." When the primary source of electrical potential on the generating circuits is not continuous, but alternating, the intensity of ultrasonic waves emitted will pulsate, the pulsations forming a tonic-train of pitch equal to twice the pitch of the alternating source. On placing the capsule of the listening device in the ultrasonic beam each pulse of radiation will distort it, and produce in the attached tube a note of the same pitch as the tonic-train. In an ultrasonic energy field excited by *continuous* electrical potential no sound could be produced in any mechanical listener.

Torsion Pendulum Method of Measurement

The torsion pendulum consists of a small reflecting vane of metal, or other material, suspended from a torsion head by

a fine metal or quartz strip or fibre. The radiation pressure of ultrasound impinging on this vane causes a torsional deflection, which may be measured by twisting the suspension until the vane returns to zero position, the angle of twist being read on the torsion head. Fig. 6 shows various pendula that have been employed by the writer. The method of mounting and use of these pendula have been described [15] (p. 179). With these devices both comparative and absolute values of ultra-



TORSION PENDULA

FIG. 6

sonic energy have been determined, and energy surveys of beams and of their reflections have been conducted. Work of this kind, when the waves are propagated in water, must be carried out in a *large* tank, and care must be taken that reflections from the water surface, sides, bottom, and ends of the tank will not introduce serious errors in the results. Fig. 7, Plate I, is a photograph of apparatus and a tank used by the writer. The tank is made of wood, 15 ft. × 5 ft. × 3½ ft., fitted with a side-bay, 5 ft. × 5 ft. × 3½ ft., for experiments on reflection. In order to avoid errors in measurements,

caused by reflections from the ends of the tank, dissipating screens of wood are set in at the ends at an angle of 45° to the wall. The spaces between these screens serve to trap the energy arriving there and dissipate it by multiple reflections [15].

Form of Beam

Investigation of the form of beam emitted from many oscillators shows that their energy distribution can only approximately be predetermined from any optical formula. In the case of the diffraction of plane waves of monochromatic light through a circular pin-hole, in order to avoid mathematical difficulties, the solution is always limited to finding the energy intensities at distances a large number of wave-lengths from the aperture. In this case the relative energy intensities are given by [17]

$$I = \pi^2 R^4 \left[1 - \frac{m^2}{2(1!)^2} + \frac{m^4}{3(2!)^2} - \frac{m^6}{4(3!)^2} + \cdot\ \cdot\ \cdot\right]^2$$

where I is the intensity at a distant point whose angular divergence from the central axis is α, λ is the wave-length, and $m = \frac{\pi R}{\lambda} \sin \alpha$. The value of the bracketed expression becomes alternately positive and negative, and therefore must pass through zero values. Calling θ the divergence of the beam (angle between the central axis and the boundary of zero intensity), $\sin \theta = 0{\cdot}61\frac{\lambda}{R} = 1{\cdot}22\frac{\lambda}{D}$.

Experimentally the more closely practical conditions can be made to approximate to ideal standards the more closely will the experimental curves of energy distribution approximate to those deduced from optical formulæ; but one encounters many instances of ultrasonic generators radiating well-defined beams in air or water not definable by optical analogy. All beams, however, show a narrowing of the angle of the beam with diminution of the emitted wave-length and increase of the radiating diameter. Fig. 8 shows the results of an investigation in which special care was taken to make conditions as ideal as possible. The transmitter was a single-plate instrument, radiating directly into the water from a clearly outlined circular face, 15·3 cm. in diameter. It was driven at its resonant frequency of 131,500 cycles per second (λ in water = 1·14 cm.). The resonant vibration of the steel plate of the generator no doubt smoothened out some of the irregularities due to varying piezo-electric quality of the active quartz, and provided greater uniformity of vibration over the emitting face. Pendulum measurements were taken in a horizontal

plane across the beam, at right angles to the central axis, and about 70 wave-lengths from the transmitter. In this case the agreement between the predicted and actual energy distributions is not bad [15].

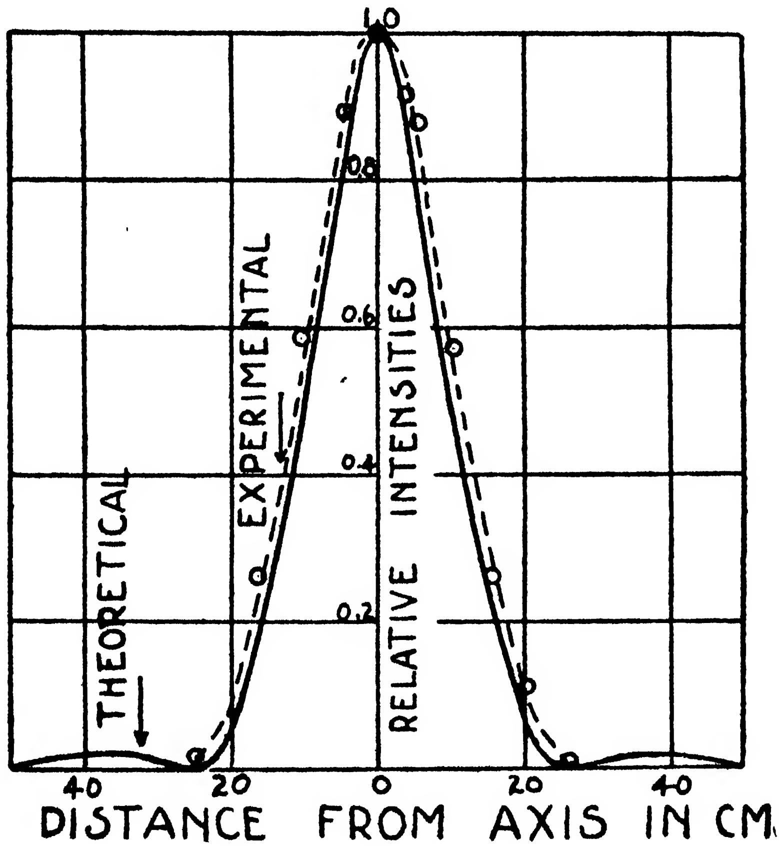


FIG. 8

Energy Distribution near the Source

Here obliquity counts. Except for points on the axis in the analogous optical problem of the pinhole this problem has never been tackled mathematically. Lommel [18] gave some attention to the reverse problem of the optical shadow cast by a circular disc. Experimentally in optics the problem cannot be solved, for it is impossible to place observing instruments *a few wave-lengths* of light, or a fraction of a wave-length, from a pin-hole. With ultrasonics an experimental investiga-

tion is possible, and has been carried out [15] (pp. 183–7) by Boyle and Reid. Summarising such experimental investigations for ultrasonics, it is sufficient here to remark that the energy distributions depend on individual oscillators, and even in the best of conditions cannot be said to agree with deductions from any optical formula.

Interferences of Wave-trains

These can be illustrated very well by the dust figure method already described. Two transmitters in a water tank face nearly in the same direction, their beams being projected horizontally. By connecting them *in parallel* to the same oscillating electric circuit they are made to radiate waves of the same length. In Figs. 9 and 10, Plate II, are represented two cases where, though the generators were not of the same size, the emitted energies were of about the same magnitude. The imposed high-frequency voltage was 2,500 volts and the frequency was 96,000 ($\lambda = 1{\cdot}5$ cms.). In the first case the angle between the two radiating faces was 175°, and in the second 120°. On allowing the beams to interfere the figures shown in the photographs were obtained [19]. On setting the transmitters face to face in the tank (60 cms. apart in this particular experiment) the figures of regular stationary waves were obtained, as shown in Fig. 11, Plate II.

Audible Beats from Inaudible Sources

Two vibrations of any kind, of frequencies n and n_1, when excited together, may produce a whole series of difference and summation tones, though generally it is only the difference tone ($n - n_1$) or " beat " which can be detected easily. The phenomenon is often demonstrated with sounds of ordinary pitch, and nowadays it is commonly applied in the " Heterodyning " of radio-telegraphy. The phenomenon may easily be realised with ultrasonic waves, and audible beats have been obtained from two inaudible sources [16]. Two generators, excited from *separate* electrical sources, are placed facing one another in a water-tank. The circuits are adjustable so that the radiated frequencies of each may be varied within a range, say from 30,000 to 150,000 vibrations per second. Audible beats, the pitch of which may be varied by altering the frequency difference (through a range from 0 to about 20,000 ∿/sec.) between the two primary tones, are easily detected by means of the listening device already described. As the detector is moved along the axis of the interfering beams, positions of maximum and minimum intensity of beat note may be perceived, thus indicating the possibility of the formation of

Fig. 9.

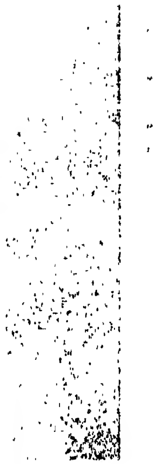


Fig. 14.

Fig. 10.

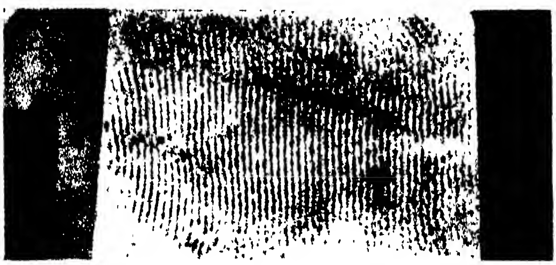


Fig. 11.

Fig. 12.

Fig. 13.

PLATE II

stationary waves. When the pitches of the primary trains are brought to the same value regular stationary waves result. This method of producing and detecting stationary waves was first used by Boyle, Lehmann and Morgan [20] to measure the velocity of ultrasound in liquids.

Reflections of Wave Trains

Reflections occur readily and most materials are good reflectors. The photographic method by dust figures may be employed to help visualise what is involved in the phenomenon of reflection, in which case it is necessary to place the reflector fairly close to the source so that the strengths of incident and reflected radiations may be sufficient to distinguish clearly the tracks of maximum and minimum intensities. The generator radiates weaker outer zones of energy as well as a main central beam ; hence dust-figure representations of these outer zones and their effects, as well as that of the central beam, may be observed in the photographs [15]. Figs. 12 and 13, Plate II, illustrate two cases of reflection in water, the reflector being a thick steel plate, placed in a vertical plane 60 cm. from the transmitter, and the frequency being 173,000 cycles per second ($\lambda = 0{\cdot}87$ cm.). The photographs bring out some interesting points. In both, the reflections of the main beam are disclosed by a light streak in the photographs. It can be seen that the central axis of the main beam and that of its reflected portion make equal angles with the normal, or " the angles of incidence and reflection are equal." The representations of several sets of stationary waves are also in view. In the first photograph those next the reflector are due to the interference of a portion of the main beam with its own reflection ; here the nodal lines are parallel to the reflector. The sets of stationaries farther away are due to the interference of the direct " side-beams " with the reflected portion of the main beam ; here the nodal lines are inclined at an angle to the reflector and as the distance from the reflector increases the angle between successive nodal lines opens out. This is all in accord with ordinary interference theory. The second photograph was taken when the " side-beams," which otherwise would interfere with the reflected portion of the central beam, were cut off near the transmitter by placing in their path an obstructing screen. The stationaries now formed by the incident main beam and its reflected portion are clearly shown near and parallel to the reflector. (Such experiments in ultra-sound are analogous to the Lloyd's mirror and Wiener's experiments in optics.)

Since the field near the reflector is filled with interference surfaces it follows that any quantitative survey of the energy

distribution in any plane across the reflected beam will show evidence of maxima and minima; in other words, reflection curves will be jagged and not very smooth. This is what quantitative investigations by the torsion pendulum disclosed [15], (p. 193).

Reflection from *ice* is an important practical problem, and from experiments carried out by the writer and G. B. Taylor [21] ice seems to be a somewhat better reflector than would be indicated by calculations based on older quoted physical constants.

Measurement of Velocity and the Relation of Velocity to Frequency

The easiest method of measuring velocities is by stationary waves. By reflecting the waves emitted from a transmitter, either in a vertical or horizontal liquid column, Boyle, Morgan, and Lehmann [16], [20], were able to measure the velocities of ultrasound, and consequent adiabatic compressibilities, in certain liquids at various frequencies and temperatures. The changes of velocity with change of temperature in water and in sodium chloride solutions were also determined. In this work either the listening tube detector or dust figures were used to mark the nodal planes by which the half wave-lengths were measured. Both methods are possible, but the one by dust figures was considered to be more certain and reliable, since it is possible to view the figures and examine visually whether or not the nodes are perfectly uniform and regular.

There are theoretical reasons for expecting that in unconfined bodies of liquid there will be no significant change of velocity with change of frequency. Boyle and Taylor [21], [22] have shown this to be the case for water and castor-oil—over a range of frequency from 29,000 to 600,000 cycles per sec. Fig. 14, Plate II, is a photograph of dust-figure stationary waves representations in water, at a frequency as high as 570,000 ∿/sec.

Velocities in gases have been determined by Pierce [23], who made refined measurements in air and in carbon dioxide by the method of stationary waves. Oscillators of only small size and power were employed, the emitted waves being made to strike a reflector fairly close to the source. The position of the reflector could be adjusted delicately by a micrometer screw. By connecting the oscillator electrically to the grid and plate of the generating electron tube the oscillator itself could be made to act as detector of the nodal planes. For when the reflector was moved through a node by means of the micrometer screw it registered the nodal position by causing

an electrical reaction on the grid of the generating tube, which produced a throw of a microammeter needle in the plate circuit. Pierce's observations, conducted mostly at very high frequency, showed very small though peculiar variations of velocity of ultrasound with frequency in the cases of air and carbon dioxide. An unexpected result with the latter gas was the large absorptive power it seems to have for very high pitch ultrasonic waves.

The high absorption in carbon dioxide has recently been confirmed by Abello [24], who found also that hydrogen is strongly absorptive.

In much the same manner as in Pierce's experiment for gases Hubbard and Loomis [25] recently measured ultrasonic velocities in small samples of liquids. The passage of the movable reflector through a nodal plane was indicated by a reaction on the circuit, the extinction of a neon tube loosely coupled to the circuit registering the effect.

In all reflection experiments in a gas or liquid as above a caution should be observed not to depend too much on the distances between nodal planes when *near the source*. Emission theory shows this to be a region of very variable intensity and phase, and here particularly the nodal figures may be irregular. Also when a gas or liquid carrying the waves is confined in a small space, care must be taken to ensure that the nodal planes actually do correspond to exact half wavelengths. In confined spaces the phase velocity may differ considerably from the velocity when unconfined, and the nodal planes may be irregularly placed. An investigation is being carried out at present by Mr. D. K. Froman, of this laboratory, to determine the variation of the phase-velocity in liquids contained in tubes, and its dependence on the frequency and dimensions of the liquid column. Phase velocity-frequency curves in certain cases show similarities with the "selective dispersion" curves of optics, and probably for the same reason broadly speaking, viz. the absorption of energy at a particular frequency by the column or tube, or both, falling into a resonant condition with the imposed waves.

The velocity of ultrasound in solids can be simply measured by the impact method described in the report of an investigation by Lang [4]. From Kundt's tube measurements of the stationary waves in air, produced at the end of a metal rod by striking the opposite end with a hammer, Lang showed that there was no significant change of velocity with frequency, in steel and brass rods, over a range of frequency from 1,200 to 50,000 cycles per sec.

But other means of measuring the velocity in solids may be devised by utilising the piezo-electric principle. Plates or

bars of the solid may be made to constitute either a single quarter-wave-plate or half-wave-plate oscillator, of which the period of fundamental resonance may be determined by any appropriate device like the torsion pendulum, Rayleigh disc, or listening tube. From the thickness of the plates and the frequency of exact resonance the velocity in the plate may be deduced. An effective and quick method of determining the frequencies of resonance of such an oscillator is to allow it to react on the generating electrical circuit in something of the same manner as in Cady's [26] or Pierce's [23] experiment. The reaction at the exact frequency of fundamental resonance, or of an overtone, may be revealed by the click of a telephone, the throw of a micro- or milliameter, or by the glow of a neon tube, placed in the plate circuit. In an investigation of this kind now being carried out by Mr. D. O. Sproule the velocities of ultrasound in a large number of solids are being determined and other related points investigated. The method adapts itself to the use of small samples of the solid investigated, for very small oscillators in the shape of rods and bars may be employed.

The theory of the longitudinal vibration of viscous rods at high frequencies has been worked out by Cady [27], who applied it in investigating the performances of the small quartz oscillators [27] employed in the service of maintaining a constant emitted wave-length from radio transmitting stations. Quimby [28] also has used the phenomenon of longitudinal vibrations by the piezo-electric method to determine the solid viscosity of metallic rods; and Giebe and Schiebe [28*a*] by exciting small oscillators in a vacuum have been able to investigate resonant frequencies by a phenomenon of luminescence which appears at resonance only.

Reflections of Longitudinal Waves and their Transmission through Materials

When ordinary sound-waves pass through a wall or partition the latter usually is very thin in comparison with a wave-length ; very often it is of non-homogeneous material, and often it is absorptive. The exact laws of reflection and of transmission are difficult to investigate with waves of ordinary sound. But it is possible to regard theoretically the transmission of sound-waves through a homogeneous partition as a problem analogous to the passage of light-waves through a parallel-sided transparent plate. The late Lord Rayleigh [29] treated this problem mathematically, and, following his method, Boyle and Rawlinson [30] have worked out the details of certain results applicable to ultrasonics. It can be deduced that the ratio (r) of reflected

to incident energy for plane waves at normal incidence is given by

$$r = \frac{\left(\frac{\rho v}{\rho_1 v_1} - \frac{\rho_1 v_1}{\rho v}\right)^2}{4 \cot^2 \frac{2\pi d}{\lambda_1} + \left(\frac{\rho v}{\rho_1 v_1} + \frac{\rho_1 v_1}{\rho v}\right)^2}$$

and the ratio (t) of transmitted to incident energy is given by

$$t = \frac{4 \operatorname{cosec}^2 \frac{2\pi d}{\lambda_1}}{4 \cot^2 \frac{2\pi d}{\lambda_1} + \left(\frac{\rho v}{\rho_1 v_1} + \frac{\rho_1 v_1}{\rho v}\right)^2}$$

Here ρ and ρ_1 stand for densities, and v and v_1 for velocities in the propagating medium and partition respectively. Thickness of partition is d, λ_1 is the wave-length in its material.

These relations show that when $\rho v = \rho_1 v_1$, or what amounts to the same thing, if $\rho\lambda = \rho_1\lambda_1$, λ being the wave-length in the incident medium, r is zero ; or in the words of a convenient rule, " when the *mass of a wave-length* is the same in incident and reflecting media the reflection is *nil* and transmission is perfect." Given a solid and large enough plate of any material as reflector, the proportion of energy reflected should depend on the thickness of the reflector. It should be possible for a thin plate to cut off more energy than a thicker one, or a thick plate to let through more energy than a thinner one. For if d above equals $n\frac{\lambda_1}{2}$, where $n = 0, 1, 2, 3$, etc., corresponding thicknesses of plate will be integral numbers of half wave-lengths, and the relations show that the reflection will be minimum and transmission maximum ; if $d = (2n + 1)\frac{\lambda_1}{4}$ corresponding thicknesses will be integral odd numbers of quarter wave-lengths, and reflection will be a maximum and transmission minimum. There is nothing in the theory to prevent its validity at any frequency high or low, but it must be remarked that all considerations of energy dissipation, *i.e.* absorption, within the partition are neglected.

When the vertical vane of a torsion pendulum is placed across the track of an ultrasonic beam the vane itself is a small plate reflector of the waves ; in consequence its readings must depend on the thickness of the vane and the instrument itself may be made to yield information concerning the reflection. In experiments carried out a few years ago by Boyle and Lehmann [31] it was shown, by using ultrasonic waves (frequency 135,000 cycles per second), travelling in water and

striking lead pendula, that the reflection from the lead was a maximum and transmission a minimum at a thickness of vane about a quarter wave-length; and reflection was a minimum and transmission a maximum at a thickness of half a wave-length. Other experiments of this kind, in which different materials and different frequencies were employed, were carried out later by Boyle and Froman [32], with results all similar to the former ones. Recently the writer and Mr. D. O. Sproule [33] performed a series of direct experiments, in which the energy transmitted through a metallic plate, intercepting perpendicularly the path of an ultrasonic beam in water, was measured by a torsion pendulum placed in the "*ultrasonic shadow*" behind the plate. The frequencies employed were 300,000 and 528,000 cycles per second. It was quite clear that, at plate thicknesses of an odd number of quarter wave-lengths, reflection was maximum and transmission minimum, while at thicknesses of a small integral number of half wave-lengths the reverse was the case and nearly all the incident energy got through.

The theory here outlined is straightforward and analogous theories are valid for other and all types of vibratory motion. In the case of acoustic waves the half wave-length is the thickness requisite for a *resonant* longitudinal vibration of the reflecting partition. We can imagine a partition or plate of this thickness picking up the incident energy, setting itself in resonant vibration, and handing on the energy to the medium on the other side. As a quarter wave-length thickness the plate, so far as concerns its own vibration, is most inert, offers the most impedance to the impinging waves, and reflects them back.

Ultrasonic Cavitation

Earliest researchers on ultrasonics in liquids observed the interesting phenomenon of ultrasonic cavitation, or the production of bubbles and expulsion of gas from the liquid by the action of the waves. Some American experimenters made observations on the phenomenon in 1918, but these were not published. Boyle and Lehmann [34] found that in volatile liquids bubbling could be produced at lower energy intensities than in water or in oil, and used the bubbles to indicate the positions of ultrasonic stationary waves formed in benzol, toluol, zylol, and other liquids. Cavitation in the liquid is marked by the continuous formation of bubbles near the ultrasonic generator if driven at sufficient power. The bubbles may be driven to or formed at nodal regions of stationary waves set up by reflecting the beam from a reflector, and curtains of bubbles may be made to rise through the liquid parallel to the face of the reflector at distances half a wave-

length apart. With care the curtains can be produced so well defined and regular that measurements of wave-lengths and velocities may be taken from them.

An interesting question arises at this point. Is or is not the bubbling here described a consequence of disruptions in the liquid by tensions produced in the waves? It is deducible theoretically that there should be a limit to the intensity of energy which a liquid will transmit, at least at low frequencies, and the writer has pointed out that the maximum energy transmissible in water would be, at atmospheric pressure, *i.e.* at or near the surface of water, 0·34 watts per sq. cm., or 4·5 h.p. per sq. metre; at 3 inches depth, 0·56 watts per sq. cm.; at 6 metres depth, 0·85 watts per sq. cm. [35]. It would seem that it is not possible to transmit as much as 1 watt per sq. cm. in water. But in the extremely short period of an ultrasonic vibration it is inconceivable that there would be time for the disruption of the liquid and formation of a bubble from the above cause. Boyle, Taylor and Froman [36] by making energy measurements under the conditions of cavitation, examined this point experimentally, and found that bubbling was produced, at least in one liquid, viz. gasoline, at much lower energies than the theory above would indicate.

That the production of the bubbles depends on the hydrostatic pressure may be shown by an apparatus arranged by Taylor and Sproule [37]. A bell jar, sealed to the face of an ultrasonic generator facing upwards, contained a column of liquid, in this case water, about 12 cms. diameter. The height of column was about 20 cms., and the pressure in the air space above it could be reduced by a connected pump. On exciting the generator stationary waves were produced in the liquid column, the air-liquid surface serving as a reflector. The pressure in the bell jar, or the voltage applied to the generator, or both, could be adjusted to result in the production of either large or small bubbles in the liquid. The greater the pressure the higher was the voltage required. When large bubbles were produced they rose rapidly through the liquid; but the small bubbles, especially at very high frequencies, could be made to remain suspended in the liquid in layers parallel to the reflecting surface and half a wave-length apart. The nodal layers were particularly regular and distinct at frequencies about 170,000 cycles per second.

Here it may be mentioned parenthetically that the whole column of liquid could be thrown into resonance, this condition being indicated by a slight humping of the liquid at the free surface when the frequency was exactly right for the length of column to resound. The height of the hump depended on the intensity of the radiation, and if the liquid were slowly drained

from the bell jar the humping recurred every time the free surface passed through a nodal level. In fact in this way measurements could be taken of half wave-lengths and velocities. Undoubtedly it is this humping of the liquid, greatly magnified by high intensity, which becomes the ultrasonic mound or fountain observed by Wood and Loomis [38] and described later.

To measure the minimum intensity of energy at which cavitation can take place in a liquid an absolute determination becomes necessary, and the most convenient method is by the torsion pendulum. Cavitation was produced in a small tank of commercial lighting naptha, and observations were taken of the frequency and voltage on the transmitter when bubbling could just be discerned. The approximate (not the exact) value of energy emission was determined by transferring the generator to a large tank of water, exciting it at the same frequency and voltage as when the bubbling was observed, and measuring the emitted energy by a pendulum. While the transmitter was working in the naptha, samples of the gas released at its surface by the bubbles produced in the liquid were collected, and later analysed.

It was found that bubbling in naptha could just be discerned at an *average* energy emission of the order of 6×10^{-3} watts per sq. cm. But it is the maximum rather than the average energy intensity which causes the bubbling to begin, and this was of the order of 2×10^{-2} watts per sq. cm. The velocity of sound in the naptha was found to be $1{\cdot}4 \times 10^{5}$ cm. per sec., its density 0·70 gm. per c.c., and its vapour pressure at the temperature of experiment 10 cm. of mercury. From these data it could be estimated on simple theory that the minimum energy to produce disruption in a pure naptha would be of the order 0·3 watt per sq. cm. The measurements showed that in the naptha used, not specially pure, bubbling began at energies about one-fifteenth of this amount.

The samples of gas collected from the bubbling liquid take quite a long time to collect, although bubbling in the liquid may be quite copious. It is clear that much of the contents of the bubbles are absorbed back into the liquid as the bubbles rise through it to the collecting vessel. Analysis of the gas collected showed it to be mostly air.[1] Thus to take one case, the result of the gas analysis was—oxygen, 17·1 per cent. by volume, nitrogen, 71·6 per cent. by volume, organic vapour, 11·3 per cent. by volume. The temperature at which these experiments were carried out was 15° C., at which temperature the vapour pressure of the naptha was sufficient to account for

[1] I am informed that American investigators found the same result some years ago.

the organic vapour. Hence about 89 per cent. of the gas collected from the bubbles was air. Further experiments showed that a liquid could be deprived of its absorbed air by prolonged action, and when so deprived the liquid required greater intensities of energy to restart the phenomenon. Also on aerating the liquid by stirring or shaking cavitation could all the more easily be produced.

All such results point to the fact that we are not here dealing with ruptures of the liquid by tensions in the waves, but with the expulsion of air and other dissolved gases, probably existing mostly as small bubbles around dust nuclei in the liquid. Reduction of resultant pressure by the harmonic pressure in the waves would tend to cause a slight instantaneous evaporation of liquid into bubbles of gas already present ; but the greater part of the action of the waves is to drive smaller bubbles into nodal regions by the radiation pressure and there make them coalesce. In fact a bubble in a nodal region can sometimes be observed to grow, and sometimes two or more small bubbles can be seen to coalesce, until the buoyancy is sufficient to take the enlarged bubble to the surface. If solid dust particles are scattered in the liquid containing stationary waves it will be seen that the bubbles rise and dust falls in the same planes.

The existence of this cavitation phenomenon must cause extraction of energy from the waves, and the discontinuities in the propagating liquid cause further dissipation of energy by scattering and reflection. Consequently the phenomenon tends to restrict the maximum intensity of energy transmissible.

Very High Frequency, High Intensity Effects

If the frequency be advanced to very high pitch and energy emission be further intensified by concentration in small spaces, a host of curious and interesting results may be exposed, as shown in the interesting recent work of Wood and Loomis [38].

These investigators used 2 kw. of power, which energised through electronic valves small quartz blocks oscillators, from 0·7 to 1·4 cm. thick, immersed in a dish of oil. The quartz could be thrown into resonant longitudinal vibration, at frequencies from 200,000 $\sim$/sec. to 500,000 $\sim$/sec., and oscillating voltages up to 50,000 volts could be employed. The intensity of the very high frequency radiation in the oil above the oscillator became so great that the oil was raised in a mound 7 cm. high above the quartz and from the top of the mound oil-drops were spattered in a fountain. On placing a glass disc in the oil above the oscillator conditions may be adjusted to

produce a force of 150 gm. by radiation pressure on the disc. By dipping various devices in the oil the vibrations may be communicated to objects or materials on which the effects of the waves are to be observed.

For example, waves transmitted along thin glass threads (0·2 mm. diameter) held tightly in the fingers produced sufficient heat to sear the flesh. Glass tubes dipped in the oil have stationary waves set up in the walls, as can be evidenced by the production on the inside surface of a tube of regularly disposed rings of heavy oil or heated paraffin with which the inside of the tube is coated. Measurements of velocity, deduced from the frequency and the distances apart of these rings, indicated the waves in the walls to be transverse and not the longitudinal ones which might have been expected. Of course both types of wave should be present; it is a matter of the length of tube, thickness of wall, etc., as to how the available energy will be distributed. In general if one type of elastic vibration exists in a solid body all other types will be present in some degree. Probably it was the evidence of some combination of more than one type which Wood and Loomis recorded, when they observed evidence of the presence of a second system of waves, *obliquely disposed*, in the walls of the vibrating tubes.

When the intense waves generated as described are transmitted along a tapering glass rod drawn to a point the vibrations can make the point drill a small hole in a piece of wood or sheet of glass. Setting up of stationary waves in a conical rod showed that the vibrating segments were longer where the rod was wider, which is the case for transverse vibrations, but not markedly for longitudinal ones. Measurements of the velocity of these waves, in rods of different diameters and at different frequencies, showed that the velocity increased with diameter and depended somewhat on frequency, as would be expected. But at a velocity of about 1,400 metres per sec., in all the cases examined, there was a break in the velocity-diameter curves for any frequency, indicating that beyond this value of velocity the increase of velocity was less rapid with rising diameter, and also less rapid the higher the frequency. Complicated interference patterns were produced by lycopodium powder sprinkled on flat discs when the vibrations were communicated to the discs through solid rods. The form of the pattern of course depends on the shape and dimensions of the disc and the position of its contact with the communication body.

Other interesting phenomena were observed by Wood and Loomis, viz. the production of emulsions and fogs by the "atomisation" or fragmentation caused by the action of these high-frequency waves on mercury and oil in contact with another liquid such as water; the flocculation of suspended particles

in liquid ; suggestions of alteration of type and of rate of crystallisation from solutions in certain cases ; dispersal of colloids from the grains of soil ; and certain biological effects which will be referred to later.

Biological Effects

The earliest experiments were carried out by Langevin and his assistants at Toulon in 1917. With a solid block of quartz as oscillator (about 1 cm. thick), excited at high oscillating voltage from a Poulsen arc, and thrown into a resonant vibration at frequency nearly 200,000 ∽/sec., they found that certain small fish could be incapacitated temporarily or permanently, or even killed, by the intense radiation. The present writer tried the same experiment, with less intense beams and longer wave-lengths (frequency about 45,000 ∽/sec.), on gold fish and silver fish, but with negative results. Negative also were the results on tubes of bacterial culture placed in beams as intense as were available, but with wave-lengths relatively long (frequently up to 130,000 ∽/sec.). Water containing the small unicellular green alga (chlamydomonas sp.) showed nodulation in a tube, the individuals gathering at the nodes ; but probably this was no more than the ordinary stationary wave effect as in the case of dust particles settling at the nodal regions.

Wood and Loomis, on repeating Langevin's experiments with their new apparatus, found that small fishes and frogs could be killed on exposure to the waves, and suggested that the cause may be the high internal heating in the body of the animal by the intense radiation. They found also that cells of spirogyra were ruptured and filaments torn to pieces on treatment with the waves, while unicellular organisms (paramecium) could be made immobile with a short treatment or killed by a longer one. Bacterial cultures survived exposure, probably because of the very small size of bacteria in comparison with a wave-length, in which case no fragmentation is possible. Notable effects were observed on the reduction of the number of corpuscles in blood, both outside and inside the animal body. In a sample of human blood, diluted by a saline solution, the number of corpuscles was reduced one-half by one minute's exposure, further reduced by one-third after another minute, and further by one-quarter after still another minute. After ten minutes no further reduction was indicated no matter how prolonged the excitation. In experiments on a mouse exposed to the waves in water there was no apparent effect at first, but after five minutes' exposure a drop of blood taken from the mouse gave a blood-count showing a marked decrease in the normal number of corpuscles. After fifteen minutes the mouse appeared

weakened and the blood-count had fallen to one half, but in a short time the animal recovered and blood-counts thereafter showed a steady and rapid return to normal.

When one reflects that ultrasonic vibrations are no more than very rapid and *slight* squeezes and tensions in the propagating body, their effects on biological organisms might reasonably be expected to be *nil* in the great majority of cases. Where marked effects are produced it is very likely that very short wave-lengths and *high intensities* are essential. Under these conditions both on account of the high intensity and on account of the rapid dissipation into heat by the viscosity of the medium, there is a marked rise of temperature; consequently temperature effects, if any, must be separated from the effects of the waves themselves if results are to be positively assured. The same considerations apply to the possibility of chemical effects, and this is what has been established by the experiments already carried out.

Chemical Effects

Richards and Loomis [39], proceeding with the work indicated by Wood and Loomis, and using apparatus of the same type, point out the reasonable possibility of very high frequency and intense radiation, (1) influencing the rate of certain chemical reactions, *e.g.* irreversible reactions or those remote from their equilibrium point, and (2) inducing an effect on a chemical system already unstable. In a series of experiments on (1) they found a slight acceleration in the rate of hydrolysis of dimethyl sulphate, but in another experiment the same phenomenon was doubtful. More positive evidence followed by obtaining a slight acceleration in the rate of reduction of potassium iodate by sulphurous acid, over and above the effect caused by the rise of temperature. The frequencies employed in this work were 289,000 ∿/sec., and intensities were high. In experiments on (2), the action of the radiation caused changes in certain sensitive metastable molecules, some examples of which may be quoted. Nitrogen tri-iodide could be exploded when subjected to sufficiently intense radiation, but it had to be set off when moist and used as the coating of a solid filament carrying stationary waves. Less sensitive explosives like ammonium nitrate remained unaffected. The yellow iodide of mercury was converted to the red below 120°, but other allotropic changes (of sulphur, phosphorus) that were examined gave no consistent results. On some superheated liquids there were positive results under the conditions of the experiment, *e.g.* carbon tetrachloride, superheated 5°, evaporated violently, less violently carbon dioxide supersaturated in water. A sirupy

solution of sodium theosulphate was changed by the radiation into a metastable crystal modification. Other solutions gave no positive results.

Cases of fragmentation of some substances at a liquid or gaseous interface were observed, *e.g.* the emulsification of metallic mercury in water; and a slight fragmentation of glass from a glass container of distilled water, which was demonstrated by a faint colouring in the Tyndall effect. On the subject of cavitation these investigators found that the radiation liberated bubbles of vapour in *air-free* liquids at a temperature *somewhat below* their true boiling points; and also that about 2·7 times as much dissolved oxygen and nitrogen were expelled from water as would be accounted for by the rise of temperature alone. Richards and Loomis draw attention to a point, noticed also by other investigators, viz. that on account of the heat produced in friction by the standing waves in the wall of a container there is great difficulty in securing *exact* temperature measurements in the contained material. Standing waves may even be produced in the wall and liquid of a measuring thermometer, and, from the nature of the action of viscosity at high frequencies, temperature gradients are certain to exist when the waves are propagated in very confined spaces.

Marine Applications

The most important practical applications of ultrasonics concern marine operations. Full descriptions have been published of the apparatus of Langevin and his collaborators. There are descriptions of the general purpose apparatus by Langevin and Chilowsky [40], and of the sounding system by Langevin and Florisson [41]. Besides the underlying scientific principles, details and drawings of engineering features of generating devices, construction of oscillators, and ship's mountings are described. In addition descriptions are published of recording devices for signalling, location of submerged objects, and sounding of depths. The operating frequency selected is about 40,000 ∽/sec., which gives satisfactory results, though there are advantages in securing greater range of signalling in operating at a lower pitch. Usual operating voltages rise to 4,000 or 5,000 volts. Reception is accomplished by local electric heterodyning.

(*a*) *Telegraphy.*—The maximum range of under-water telegraphic signalling should extend to about six miles in fair or moderate weather, but of course much depends on the turbulence of the sea. Progressive diminution of intensity is due to the spreading of the beam, viscous resistance of the water, and scattering of the high pitch waves. In rough weather the rolling

and pitching of the ship and breaking of the surface waves can cause a "shock" vibration in the receiving instruments, producing a more or less continuous noise. This may mask received signals and reduce the effective range.

In shallow water and moderate depths reverberations may occur by reflections between the water-surface and the bottom, and some of these are caused by the waste energy from the transmitting instrument in oblique and lateral radiations. Reverberation may be very troublesome in rough weather.

An ultrasonic beam projected horizontally from a ship travels with its line of maximum intensity just below the water surface; for the surface cuts off the possibility of divergence upwards and also oblique reflections take place from it. If a temperature gradient exists, the water growing colder with increasing depth, the beam curves downward by reason of the slightly greater velocity in the top and warmer layers. This may be called "ultrasonic mirage." With a gradient of $0 \cdot 1^{\circ}$ C. per metre the radius of curvature of the beam is calculated to be about 1 kilometre. If the beam runs into patches of water of different temperature, density, or salinity, reflections and refractions may occur; but compared with other extended media water is one of the best carriers of sound.

(*b*) *Locations of Submerged Objects*.—The range for object location (except ice) is about one-quarter to one-half the range for clear signalling, and if necessary the apparatus may be made automatic and recording.

(*c*) *Telephony*.—Ultrasonic telephony is quite possible on the same principle as radio telephony: viz. the modulation of the carrier waves by the fluctuations of the voice through a microphone. The first experiments in telephony were carried out by Langevin and assistants in 1917 with promising results; in recent years work of the same kind has been undertaken in the United States by H. C. Hayes.

(*d*) *Sounding*.—In sounding it is possible to measure depths from a few fathoms to over a mile. In shallow water the time for go and return of the exploring pulse of waves is extremely short; nevertheless by utilising a recording optical device Langevin and Florisson have been able to adapt the system for shallow depths, and as with *sonic* depth-finding there is no necessity to stop the ship to take a sounding. There should be an advantage over other systems in sounding by an ultrasonic beam, in that by the narrowness of the exploring beam the contours of the bottom may be more accurately mapped out.

(*e*) *Ice-berg Detection*.—The writer and Mr. Charles Reid [43] in 1924 took a trip on a Canadian Government steamer through the Gulf of St. Lawrence and Straits of Belle Isle, in the hope of gleaning some information on this important problem. The

apparatus available was of low power, so that long-range detection could not be expected. The object was to find, if possible, (1) what is the range of detection of a berg as compared with the range of a steel ship or rocky shore, exploring instruments being worked under the same conditions and with the same power, and also, (2) what is the effect of the bottom on the possibility of berg detection? In most investigations on object location by either sonic or ultrasonic waves there has been, at least in published reports, insufficient attention devoted to the important factor of the bottom.

In the tests above referred to it was found that the range of detection of a berg *in deep water* was somewhat greater than expected; tentatively it could be put at about one-quarter the range of a steel ship or rocky shore with instruments working under the same conditions. But full warning was given of the preponderant influence of the bottom when trying to detect bergs in shallow water. The bottom and surface echo or reverberation in such cases masks the echo from the ice at no great distance from it. On one occasion when the ship was brought to close quarters with a berg *multiple echoes* were received, due to the multiple reflections between the berg and ship. The multiple echoes were not strong, but they were heard just after the sending signal and were superimposed upon the loud bottom and surface reverberation which was in evidence. On turning the exploring beam away from the berg the multiple echoes ceased, but the bottom and surface reverberation persisted regardless of direction. As the ship drew off the multiple echoes faded, and soon the echo from the berg became masked by bottom echo still as loud as ever. Just here a significant observation was made, constituting, almost, a negative test for ice. At distances of 250 to 400 yards from the berg the bottom echo was not so loud and " milder " when the exploring beam was turned upon the berg than when it was turned away. Doubtless this happened because the reflection from the ice was *much weaker* than from the bottom. When the beam with nothing in its path hit the bottom directly, most of its energy was reflected, a portion of the reflected energy returning to the transmitter to produce the bottom echo. But when the berg was interposed, the ice being less effective as a reflector than the bottom, and also acting as an absorber to screen the bottom from the incident beam, less energy was returned to the transmitter than in the former case and produced a bottom echo not so strong. These experiments on ice should be repeated and extended from a specially equipped ship fitted with apparatus of full power.

(*f*) *Underwater Beacons.*—An interesting marine application is the use of an ultrasonic beam from a shore station, at a

place where a ship has to make towards a dangerous shore or headland, or enter a harbour mouth or channel in the dark or in a fog. A ship suitably equipped with apparatus could pick up the beam and be guided by it past the points of uncertainty or danger. In this respect the beam would serve the same purpose as the alternating current "leader gear" for straits and channels, with the exception that the course of the beam always will be straight. Messages could be telegraphed or telephoned along the beam between the ship and shore. A project of this kind has recently been installed at the port of Calais, France [43], and first reports are that on the whole it is working satisfactorily. An incidental result is that the receivers at the shore station have been able to detect vibrations from a shoal of pebbles situated off the shore not far away. It is thought that these vibrations are due to the rolling and colliding of the pebbles against one another by the motion of the water. The explanation is a reasonable one since the dimensions of the pebbles would be about right to cause in one another elastic vibrations of ultrasonic pitch.

Those researches mentioned in this article which have been carried out in Canada have been mostly provided for by funds of the Canadian Research Council, to whom grateful thanks are here acknowledged.

In the article the writer has endeavoured to present the state to-day of a subject which has its fascinations, combining and correlating as it does important laws of electricity, acoustics, optics, and radiation, besides offering the possibility of a very useful and beneficent practical application.

REFERENCES

1. Rayleigh. "Theory of Sound," vol. ii, 1896, p. 78; *Proc. Roy. Inst.*, Jan. 20, 1888; *Phil. Mag.* ix, 1880, p. 281.
2. Lewis Richardson. *Brit. Pat. Spec. Reports*, 1912.
3. *La Nature*, Jan. 8, 1921; Aug. 20, 1921.
4. Lang. *Trans. Roy. Soc. Can.*, **3**, xvi, 1922, p. 163. Boyle. *Trans. Roy. Soc. Can.*, **3**, xvi, 1922, p. 293.
5. Wente. *Phys. Rev.*, **10**, 1917, p. 39.
6. Hewlitt. *Phys. Rev.* (2) xix, 1922, p. 52.
7. Hartmann. *Phys. Rev.*, **20**, 1922, p. 719.
8. Voigt. "Lehrbuch der Kristallphysik," Teubner, Berlin, 1910.
9. Harrison. *Proc. Inst. Radio Engr.*, Dec. 1927, p. 1040.
10. Rayleigh. "Theory of Sound," vol. ii, 1896, p. 44.
11. Rayleigh. *Phil. Mag.*, iii, 1902, p. 338; x, 1905, p. 364.
12. Larmor. *Encyc. Brit.*, xxii, p. 121.
13. Altberg. *Drude's Ann. der. Phys.*, xi, 2, 1903, p. 405.
14. Boyle and Lehmann. Report, Can. Research Council, May 1922.
15. Boyle, Lehmann, and Reid. *Trans. Royal Soc. Can.*, 3, xix, 1925, p. 167.
16. Boyle, Morgan, and Lehmann. *Trans. Roy. Soc. Can.*, xvii, 1923, p. 141.

17. Verdet. "Leçons d'Optique Physique," Tome 1, p. 301. Crandall. "Theory of Vibrating Systems and Sound," Van Nostrand, 1926, p. 137.
18. Lommel. *Abh. der bayer Akad. der Wiss*, ii, Cl., xv, Bd. ii, Abth.
19. Boyle and Lehmann. *Trans. Roy. Soc. Can.*, xxx, 1925, p. 159.
20. Boyle and Morgan. Report, Can. Research Council, May 1923; *Trans. Roy. Soc. Can.*, Sect. III, May 1928.
21. Boyle and Taylor. *Trans. Roy. Soc. Can.*, xx, 1926, p. 245.
22. Boyle and Taylor. *Trans. Roy. Soc. Can.*, xxi, 1927, p. 79.
23. Pierce. *Proc. Amer. Acad. Sci.*, **60**, No. 5, 1925, p. 271.
24. Abello. *Nat. Acad. Sci. Proc.*, **13**, 1927, p. 699.
25. Hubbard and Loomis. *Nature*, Aug. 6, 1927, p. 189.
26. Cady. *Proc. Inst. Radio Exp.*, **10**, April, 1922, p. 83.
27. Cady. *Phys. Rev.*, N.S., xix, 1922, p. 1.
28. Quimby. *Phys. Rev.*, **25**, 1925, p. 558.
28*a*. Giebe and Scheibe. *Zeits. f. Physik*, **33**, 5–6, 1925, p. 335.
29. Rayleigh. "Theory of Sound," vol. ii, 1896, p. 86.
30. Boyle and Rawlinson. *Trans. Roy. Soc. Can.*, xxii, 1928, p. 55.
31. Boyle and Lehmann. *Trans. Roy. Soc. Can.*, xxi, 1927, p. 115.
32. Boyle and Froman. Report, Can. Research Council, June 1927.
33. *Nature*, Jan. 14, 1928, p. 55.
34. Boyle and Lehmann. Report, Can. Research Council, May 1923.
35. Boyle. *Trans. Roy. Soc. Can.*, xvi, 1922, p. 157.
36. Boyle and Taylor. *Phys. Rev.*, **27**, 1926, p. 518. Boyle and Froman. Report, Can. Research Council, June 1927.
37. *Nature*. Oct. 1, 1927, p. 476.
38. Wood and Loomis. *Phil. Mag.*, Jan. 1927, p. 417.
39. Richards and Loomis. *Journ. Amer. Chem. Soc.*, **49**, 1927, p. 3086.
40. Langevin and Chilowsky System. *Int. Hyd. Bureau*, Special Pub. No. 3, Oct. 1924, p. 11–45.
41. Langevin and Florisson System. *Hydrog. Rev.*, iii, No. 2, 1926, pp. 75–87.
42. Boyle and Reid. *Trans. Roy. Soc. Can.*, xx, 1926, p. 233.
43. "Sound Ranging," *Hydrog. Rev.*, iv, No. 2, 1927, p. 157.

SOME MODERN ASPECTS OF CRYSTALLOGRAPHY

By F. IAN G. RAWLINS, M.Sc., A.Inst.P.

It is common knowledge that crystals have attracted the interest and attention of men from early times. More recently, the nineteenth century provided a period of activity distinguished by its insistence upon crystallography as a nearly ideal natural illustration of solid (Euclidean) geometry. The discovery by Prof. von Laue that a crystal acts as a diffraction grating for radiation of very short wave-length (X-rays), immediately had a profound effect in shifting the centre of interest from external properties to the anatomy of the crystal itself, *i.e.* to the nature of the units of which crystals are built up, and to the manner of their arrangement.

A great deal has been written about crystal structure in the light of X-ray research: in this article this information will be taken for granted. In other words, it is assumed that the elementary nature and arrangement of the units are known for a very large—and rapidly increasing—number of crystals. The purpose now is to see to what further knowledge such labours have led. To this end, brevity may perhaps best be attained by the use of a table showing how present lines of investigation compare with those prevalent in the older (or classical) crystallography.

TABLE I

A *Modern*	B *Classical*
X-ray data.	Microscopical investigations.
Infra-red spectra.	
Deformability of crystal units.	Molecular refractivity.
Co-ordination numbers.	External symmetry.
Thermodynamics of assemblages.	Thermodynamics of continua.

This table does not claim to be complete; obviously there are a number of properties fully as significant to-day as they were when first discussed. For example, workers of last century were acutely aware of the anisotropic nature of crystals, and carefully allowed for this fact by measurements along the

appropriate axes ; but in modern work this need of the recognition of privileged directions is more important than ever, for it corresponds essentially to the density of packing of the ultimate units of the crystal along different planes.

Thus, the application of X-rays to the problem of crystal structure has increased enormously the need of an extension of our empirical knowledge of vectorial properties (*e.g.* hardness, cohesion, compressibility, conductivity, etc.).

X-RAY DATA AND INFRA-RED SPECTRA

(*a*) Crystals are called heteropolar—or ionic—if the units of which they are composed carry charges, and are thus ions as opposed to neutral atoms. A number of simple salts, including the halides of the alkali metals for example, are well known to be of this type. The forces operative in holding the complex together are usually assumed to follow the common (Coulomb) electrostatic law of attraction, and a law of repulsion varying as the inverse nth power of the distance of separation of unlike charges. Many of the simpler properties have been calculated by Born on these assumptions, and show very fair agreement with observation.

A basic characteristic of ionic crystals—on this view—is that the ions retain their individuality, so that each ion consists of its core or nucleus, together with a number of electrons giving the usual chemical valence, *e.g.* Li^+, Mg^{++}, Al^{+++}, Cl^-, O^{--}, etc.

The effective ionic radii have been computed and tabulated for the majority of the elements by Goldschmidt [1] from X-ray data, and by Pauling [2] from theoretical considerations based upon the new wave mechanics. Excellent agreement is found.

The presence of free charges allows the expectation of resonance to an applied electromagnetic field, and this is at the root of much that has been learnt about the forces at work between ion and ion (or ion and charged groups), by means of reflection and absorption spectra of crystals in the infra-red, including the classical " Reststrahlen " of Rubens.

Again, making use of the conception of ions, Joffé has made great progress in investigating electrical conductivity in crystals, and has succeeded in showing conclusively that in heteropolar crystals the process is electrolytic. He considers that thermal diffusion—naturally a function of the temperature—is constantly taking place within the crystal, and that the effect of an applied potential difference across the specimen is simply to exert a directional effect upon the ions as they follow their disorderly thermal movements, driving them towards the appropriate electrode, where they are deposited

in just the same way as obtains for solutions of electrolytes following Faraday's laws.

Some remarkable features, however, are present. In crystals of the NaCl type, for example, practically the whole of the transport of the current is done by the Na^+ ions, the non-metal being almost ineffective [3]. It is interesting to notice that Waller and James [4] have recently shown that the root-mean-square amplitudes of vibration of Na^+ and Cl^- in rocksalt are 0·242Å and 0·217Å respectively at 290° Abs. The sodium ions are thus less tightly bound in the lattice than the chlorine ions.

It is interesting to attempt to visualise the mechanism by means of which the electric current is carried through the crystal from one electrode to the other.

The view is that in the process of thermal diffusion already alluded to, some ions—particularly those of a metallic nature—escape from their original positions, and may even enter spaces in the crystal lattice rendered vacant by other ions of like sign (or charge) performing similar migrations, only the resultant movement towards the poles being the work of the applied electromotive force. Thus the general nature of the lattice tends to be preserved, and a reasonably satisfactory picture is obtained of the origin of the ions which appear after some time as a fairly massive deposit on the electrodes. These ions are indeed those which could no longer find a temporary abode in the manner above indicated.

In the case of crystals containing sodium, these deposits assume a tree-like (or dendritic) form when examined under the microscope, giving rise to much the same kind of appearance as metallographists are accustomed to meet in the study of alloys.

(*b*) Homopolar crystals are those in which the units are electrically neutral (*i.e.* atoms), or built up in such a way that the individuality characteristic of the heteropolar type is lost, or at least much diminished. This latter property is taken to come about as the result of electron sharing between neighbouring centres. But between truly heteropolar and truly homopolar crystals exist—so recent work tends to show—a perfectly gradual transition in that it is often impossible to say that the units are wholly discrete or completely "fused."

A. M. Taylor [5] has lately suggested that high compressibility and high thermal expansion are very fair criteria for the presence of homopolar linkages. Large values of the latter constant clearly point to deviations from a simple law of force, and this would be expected for semi-metallic crystals.

Sommerfeld [6] has recently discussed the nature of the crystal unit from the point of view of the Periodic Law, and

the known physical properties of the element or assemblage. Thus ZnS is not of the form $Zn^{++}S^{--}$, to judge from X-ray work, but much more likely neutral Zn and neutral S.

This compound possesses a tetrahedral structure akin to that of the diamond, leading directly to the belief that a non-polar bond (or linkage) is present, in which two electrons are shared between two atoms, as originally suggested by G. N. Lewis.

However, a more rigorous treatment due to Niessen raises numerous difficulties; he finds, starting from the simplest assumptions of shared orbits, that the units are not electrically neutral, but that the metal actually carries an effective negative charge and the non-metal an effective positive charge.

This investigation was conducted on the basis of classical mechanics: it is not inconceivable that the application of the wave mechanics would lead to a less paradoxical result. As Pauling has lately shown [7] some important differences may be expected on the two theories.

Deformability of Crystal Units

So far only rigid ions have been considered, that is to say, those for which the ionic radii deduced do represent the actual state of affairs existing within the crystal.

The next step is to take account of deformability, or distortion under the influence of the fields of neighbouring ions.

Perhaps the simplest way of looking at this effect is to consider an atom situated between the plates of a condenser, one of which plates is positively charged (P_+) and the other negatively charged (P_-). Then the positively charged nucleus will be displaced towards the negative plate of the condenser, and the negatively charged electronic envelope will be attracted towards the positive plate. Thus if Fig. 1 (*a*) represents the undeformed atom, its condition when in the electric field between the condenser plates is shown in Fig. 1 (*b*), in which a finite distance l separates the electrical centres of positive and negative charges.

If E stands for the electric field, or intensity, between the plates of the condenser, and e is the nuclear charge, then an electric moment $p = el$ is set up, and may be assumed to be proportional to E, so that it is possible to write—

$$p = \alpha E,$$

where α is a constant characteristic of the particular kind of ion.

Also, since this equation can be expressed in the form $\alpha = p/E$, α is the electric moment generated by unit field.

This illustration may serve to show how the ions or atoms of a crystal lattice may be deformed under the influence of the fields of their neighbours. At the same time it will be seen how it comes about that in highly symmetrical crystals (*e.g.* NaCl) this deformability α can have no effect upon the crystal properties, since any one unit (atom or ion) is so symmetrically surrounded by its neighbours, that a one-sided displacement (or polarisation) is impossible.

Nevertheless, in more complicated cases, this term α plays a leading part, and may become important enough to out-

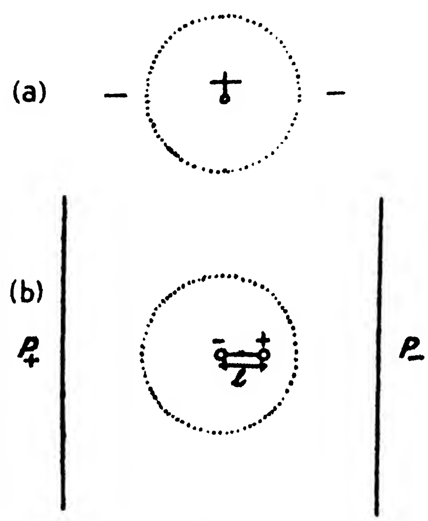


FIG. 1.

weigh other factors such as chemical constitution and ionic radii which usually go far to determine the properties of a crystal. It is the recognition in recent years that ionic deformability may settle almost entirely the equilibrium configuration of certain crystalline compounds that makes it a problem of some urgency to determine α with accuracy for as many elements as possible.

Many years ago, the late Prof. Lorentz showed (on the classical electromagnetic theory) that the deformability is connected with molecular refractivity M according to the relationship $\alpha = 3M/4\pi N$. ($N = 6{\cdot}06 \times 10^{23}$).

Fajans and others have made frequent use of this equation to determine α, whereas more recently it has been established that α is connected—at least for the simpler ions having the structure of the rare gases—with the Rydberg correction term in the series spectra.

Co-ordination Numbers

Few aspects of crystallography are more striking than the change which has come over the method of classification. Previously, external symmetry, and families of planes were the chief means of dividing crystals into groups for purposes of reference. (The spatial relations of Schoenflies and kindred conceptions do not touch the main theme of the present discussion.) From the present point of view, the advent of X-rays has modified all this in that it is the environment of a unit (ion or atom) with respect to its neighbours that now forms the most important basis of classification. Otherwise expressed, crystal geometry is not so much concerned to-day with assemblages of planes as with systems of points in ordered spatial array.

Actually it is quite possible to construct—as Born has done—a formal three-dimensional vector geometry founded essentially upon translations of groups of crystal units according to given conditions, by means of which the whole complex can be defined. In this way one arrives quite naturally at the conception of the " basis group " (the group of units repeating itself in space), and of the unit cell.

Consider a crystal having the chemical composition AX, where A is a cation and X an anion. Then the co-ordination number of such a crystal is defined as the number of atoms or ions of X which surround A symmetrically at equal distances, and which must clearly be the same as the number of atoms or ions of A which surround X similarly.

For example, in the well-known rocksalt structure, any one sodium ion has, as immediate neighbours, six chlorine ions arranged at the corners of a regular octahedron (and *vice versa*), whereas in the cæsium chloride type each cæsium ion is surrounded by eight chlorine ions at the corners of a cube (and *vice versa*). The co-ordination number in the first case is 6, and in the second 8.

For compounds of the type AX_2, obviously each A must be surrounded by twice as many X's as each X is by A's.

Thus for the fluorite (CaF_2) type, the co-ordination number is 8/4, signifying that each calcium is surrounded by 8 fluorines, whereas each fluorine is surrounded by 4 calciums.

Such crystal lattices as these are frequently spoken of as diagonal lattices, for reasons which will now be explained.

Theory shows that it is possible to build up spatial arrays of points such as those just considered from the simple starting point of a cube in which the points are arranged along a diagonal at equal distances apart (see Fig. 2).

This lattice possesses as its smallest unit cell the rhombo-

hedron outlined in the diagram, and by successive translations of this rhombohedron according to the conditions already alluded to (Born), the whole crystal arrangement is formed.

Referring again to Fig. 2, consider the diagonal AB of the cube (which is also a major diagonal of the rhombohedron indicated in the same diagram[1]), and let m_1, m_2, m_3, m_4 be four points in each or any of which an atom may be imagined to be situated, equally spaced along this diagonal: then it is possible to build up the simpler types of cubic lattices by choosing the m's appropriately. An example may make this clearer. Suppose m_1 is chosen to be of the same kind as m_2 (*e.g.* both carbon atoms), and m_3 and m_4 are assumed to be absent (*i.e.* no atoms at these points), then the atomic arrangement depicted in Fig. 3—representing the diamond—is formed.

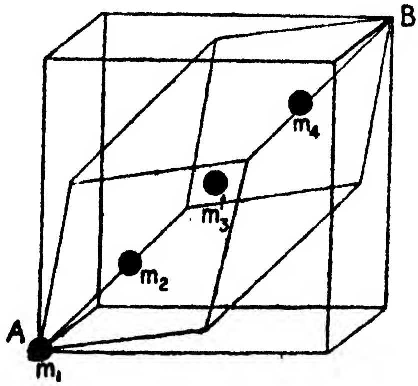


FIG. 2.

Here A′B′ is the diagonal corresponding to AB of the previous figure, and m'_1 and m'_2 atoms corresponding to m_1 and m_2.

It will be observed that in Fig. 3 the points on A′B′ which correspond to m_3 and m_4 in Fig. 2 are vacant.

Successive translation of A′B′ (considering m'_1 and m'_2 to be rigidly fixed to it) in all possible ways in turn will be found to give all the atomic positions indicated by the black circles, and thus the whole lattice is accounted for.

The characteristic tetrahedral arrangement which repeats itself in space is shown by the dotted line.

The following table demonstrates how the m's may be selected to give the various lattices.

[1] The rhombohedron is taken as the unit cell since it contains the smallest number of atoms (two in this example) which repeat in space. This constitutes Born's "basis cell" and "basis group."

TABLE II

m_2 and m_4 absent	Rocksalt type.
m_3 and m_4 absent	ZnS or diamond type.
m_3 absent	Fluorite type.
m_1 absent	Copper type.
All m's present	CsCl type.

The present author has found it very instructive to prepare a model to illustrate these cases.[1] A cube formed of suitable uprights encloses the unit rhombohedron (white thread shows up very well), while three further uprights of proper height have small screw threads at their summits on which balls, to represent the masses m_1 . . . m_4, can be screwed or un-

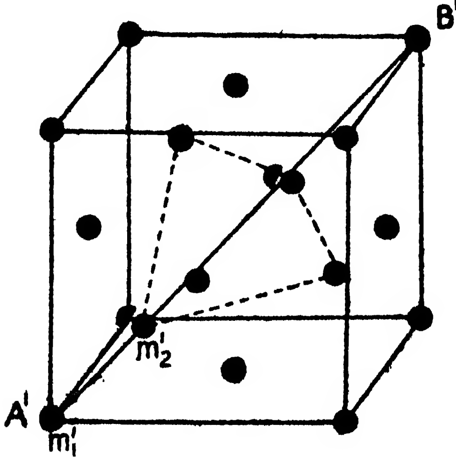


FIG. 3.

screwed at will. It is astonishing how easy it then becomes to visualise the different types.

The following table, from a paper by Goldschmidt [1] gives the possible co-ordination numbers and corresponding crystal types.

TABLE III

	Co-ord. No.	*Type*
AX	1	Single molecules.
	2	Double molecules.
	3	Graphite.
	4	Diamond, and stratified lattices.
	6	Rocksalt, NiAs.
	8	Cæsium Chloride.
AX_2	2/1	Single molecules.
	4/2	α and β Quartz, Cuprite.
	6/3	Anatase, Rutile, and CdI_2.
	8/4	Fluorite.

[1] For the actual construction, the writer is indebted to Mr. Lanham of the University Mineralogical Laboratory, Cambridge.

The structures of high co-ordination number represent a condition of high stability (other factors being equal) for an assemblage the units of which are not subject to a large amount of deformation. In other words, an ion tends to collect as many neighbours about itself as possible, and to keep these at as great a distance from itself as the geometry will permit. If now the term a, the ionic deformability, of one of the constituents is great, the resulting lattices tend to become unstable—as Hund has pointed out—and give place to entirely different types, which have only quite recently been recognised, and which promise to become of the greatest theoretical interest. These are:

(*a*) Molecular lattices.
(*b*) Stratified lattices.

(*a*) correspond to very great deformability, together with a marked discrepancy between the sizes of the constituent ions. Solid CO_2 and solid HCl are of this nature, as well as a host of organic substances. It is possible to maintain that for such bodies the chemical molecule is conserved in the solid state, and itself forms the crystal unit.

(*b*) represent an intermediate condition between true diagonal lattices and type (*a*) above. CdI_2 is a perfect example. It consists of layers in each of which the rigid cadmium ion is bordered on either side in each layer by the easily deformable iodine ion, and any one layer is, in a sense, one enormous molecule in which forces of great magnitude are present.

Macroscopic evidence is afforded by the characteristic flakes in which the crystal occurs.

A number of other compounds are now known to possess this stratified structure, among them most of the common hydroxides. This is natural, since OH^- has a large electric moment, indicative of high deformability.

If now a return is made to the simpler conditions of tangential contact between ions,[1] it becomes fairly evident that the ratio of the radii in (say) a binary compound of the form AX will be subject to limiting values in order that such contact may be maintained. Thus, suppose that it is required to find the ratio of the radius ρ' of a small circle to that of three larger circles each of radius ρ'' to satisfy the condition that the three latter shall surround the former symmetrically (establishing tangential contact) in the form of an equilateral triangle, the problem is readily solved as follows:

The distance AD ($= h$ say) is the height of an equilateral

[1] This means treating the ions as if they were rigid spheres.

triangle of side $2\rho''$ and is therefore equal to $\sqrt{3}\rho''$, and since the centre of the small circle is equidistant from the centres of the others, $\rho'' + \rho' = 2h/3 = 2\sqrt{3}\rho''/3$, or $\rho'/\rho'' = 2/\sqrt{3} - 1 = 0{\cdot}15$.

Proceeding in just the same way for the space lattices of crystals of compounds AX_2, the following limits are obtained for the quotient $Q = \rho'/\rho''$.

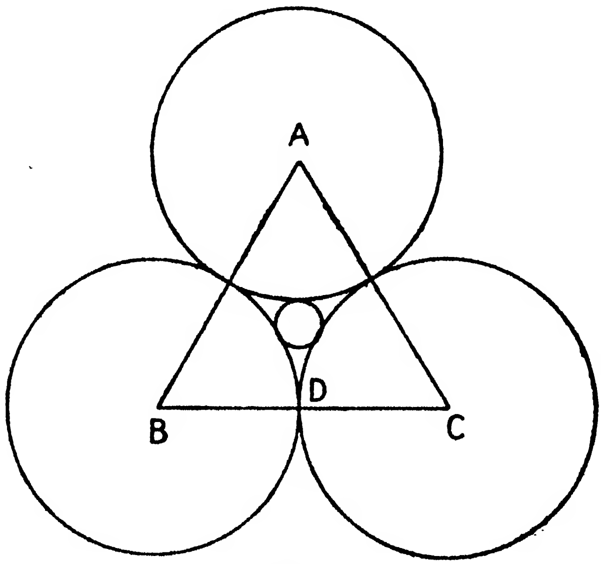


FIG. 4.

TABLE IV

Q between 4·45 and 0·73 . .	Fluorite structure.
Q between 0·73 and 0·41 . .	Rutile structure.
Q between 0·41 and 0·22 . .	Quartz structure.
Q less than 0·22 . . .	Molecular or stratified lattices.

Taking ρ' to refer to the (smaller) cations, and ρ'' to the (larger) anions, the following table gives the values of Q for a few typical stratified lattices.

TABLE V

Compound.	Q
CdI_2	0·47
ZrS_2	0·50
$ZrSe_2$	0·46
$ZrTe_2$	0·41
MoS_2	0·39 [1]

A glance at Table IV will show that if the assumption of rigid ions was justifiable for these bodies, then the first three

[1] MoS_2 differs slightly from the others in Table V; it is the only member of its type yet known.

would have possessed the Rutile structure (6/3), $ZrTe_2$ would be on the threshold between the structures of Rutile and Quartz, whereas MoS_2 would have resembled quartz. Actually, owing to the great ionic deformation, the structures of higher symmetry become unstable, and stratified lattices make their appearance.

Thermodynamics of Assemblages

According to classical mechanics, the specific heat of a solid is independent of the temperature and equal to 6. The quantum theory has offered an acceptable explanation of the form of the specific-heat-temperature curve of the diamond and of numerous other crystals, both elements and compounds. The extension of the earlier conceptions based on continua has yielded in theory a complete solution of the problem in the comprehensive functions deduced by Born and Kármán, whose idea it was to take account of the discrete nature of crystals in deriving the equations. Some important refinements in the deduction of a melting-point formula (akin to the original expression deduced by Lindemann) by means of which the characteristic frequencies of vibration of a crystal can be calculated from thermal data, have been achieved by Braunbeck [8]. Numerical substitution in his equation yields results in very satisfactory agreement with those obtained from infra-red spectra when properly interpreted. What now seems to be required is a frontal attack upon crystal mechanics, including such subjects as true breaking stress, hardness, and kindred matters which have as yet received but scant attention except in the laboratories at Oslo and Leningrad.

This brief, and necessarily incomplete, consideration of the conceptions outlined in Table I may have served to show the outstanding features of the modern crystallography, at least in skeleton form.

Two questions may now be asked:

(1) Given a metallic element A and a non-metallic element X, is it possible to anticipate the type of crystal structure which they will form in the solid compound AX_n? Otherwise expressed: can we prepare a crystal to have a given structure with a certain chemical composition?

(2) How can the occurrence of the same substance in two or more crystalline forms in nature be accounted for (*e.g.* three forms of TiO_2)?

(1) Goldschmidt [1] has discussed the following typical case. It is desired to prepare a di-iodide having the fluorite (8/4) structure.

In the first place, it is easy to see from Table IV that the ratio Q (ionic radius of cation : ionic radius of anion) must lie between the extreme limits 0·73 and 4·45.

The radius of the iodine ion is well established, and amounts to 2·20Å, and thus it will be necessary to find a (divalent) cation having a radius between 1·61Å and 9·79Å. As a matter of fact, the largest divalent cation is that of Barium (1·43Å), thus altogether too small for the purpose.

An escape from the dilemma is offered by selecting nickel, which has a radius of only 0·78Å, but has the valuable property of binding firmly to itself an envelope of ammonia, thus providing an "artificial" unit of radius 2·57Å, which easily satisfies the geometry (Q = 1·12). Hence the compound $[Ni(NH_3)_6]I_2$ should be a di-iodide with the structure of fluorite.

Wyckoff [9] has investigated this crystal, and finds that this is so.

(2) Pauling [2] has worked out the numerical factor in the lattice energy for a number of crystal types. Clearly, if two or more structures have equal, or nearly equal, values of this factor, then one is neither more nor less likely to persist in nature than the other under given conditions, and both forms may be expected.

This factor amounts to 4·80 for rutile and for anatase (both TiO_2) and to 1·640 for Sphalerite, and to 1·641 for Wurtzite (both ZnS). As a matter of fact, Hund had previously reached this conclusion, and had also stressed the result that, apart from deformability, several well-known natural crystals could never have come into being.

The replies indicated to queries (1) and (2) above surely give great confidence in the powers of the new crystallography to add very materially to our knowledge of nature.

LIST OF REFERENCES

1. Goldschmidt, *Ber. d. D. Chem. Ges.*, **60**, 1263, 1927. *Zeit. f. Tech. Phys.* **8**, 251, 1927.
2. Pauling, *J. Amer. Chem. Soc.*, **49**, 765, 1927.
3. Joffé, Soviet Council Reports, 1918–26.
4. Waller and James, *Proc. Roy. Soc. A.*, **117**, 214, 1928.
5. A. M. Taylor, *Trans. Faraday Soc.*, **24**, 157, 1928.
6. Sommerfeld, Three Lectures on Atomic Physics, 1926.
7. Pauling, *loc. cit.*
8. Braunbeck, *Zeit. f. Phys.*, **38**, 549, 1926.
9. Wyckoff, *J. Amer. Chem. Soc.*, **44**, 1239, 1922.

THE DIRECT VERIFICATION OF MAXWELL'S LAW OF MOLECULAR VELOCITIES

By JAS. P. ANDREWS, M.Sc., East London College.

It is a curious fact that a law of such fundamental importance as Maxwell's Law of Distribution of Molecular Velocities should have so little direct experimental verification, and should have had to wait so long for such verification as does exist. So far as I am aware, no frontal attack was made upon such a problem until well into this century, in spite of the fact that Maxwell's first paper on the subject appeared in 1860. Maxwell, in his subsequent work, appears to have been primarily concerned with the development of the molecular model, and the derivation of consequences; Boltzmann was founding the theory on a wider and more secure base, making its philosophical buttresses firmer, and the followers built largely to their specifications. It almost appears that the conspicuous difficulty of the time was the molecule itself; certainly more experiments were designed to prove its existence, and to justify the idea of a theory of molecules, than to sort out their velocities.

It has often been affirmed that the law is amply proved by the verification of its consequences. Undoubtedly a high probability is thus indicated, but the word "proof" is not applicable. Apart from the fact that most "consequences" are derived with the indispensable aid of some other hypothesis or principle, we know it to be quite common for different theories to predict the same course of events, at least within the limits of accuracy attainable. Indeed, established faith may be tested by a calculation on the following lines. The law of perfect gases $PV = RT$ may be derived from the distribution law, together with an additional assumption regarding the temperature. Taking the gas law and the temperature hypothesis, attempt to work back to a law of distribution. It can be shown that little more than symmetrical distribution of molecular velocities with respect to a standard direction is implied.

Such doubts as this may engender might have carried more weight half a century ago than now; for now we have the experiments of Prof. O. W. Richardson and others on the ionic

atmospheres evaporated from heated bodies, resembling in theory the work of G. Jäger on vapour pressure, and affording the soundest demonstration yet produced of the applicability of Maxwell's Law. The existence of Richardson's book, *The Emission of Electricity from Hot Bodies*, obviates the necessity of detail here; but it should be pointed out that in spite of the ionic charges involved, the conditions in many cases closely approached those visualised by Maxwell; and here we have the velocities sorted out, component by component, with considerable precision, finally demonstrating a Maxwell Distribution. After what follows, it should be clear that this remains the only truly direct experimental test of Maxwell's Law.

I find it stated authoritatively (Herzfeld, *Kinetische Theorie der Wärme*) that a method for the direct testing originated with M. Cantor in 1897 (*Wied. Ann.*, **62**, 482, 1897). Actually, Cantor determined the mean velocity of chlorine atoms which entered into combination with a fresh copper surface exposed to a chlorine atmosphere. Respecting the kinetic theory of gases, Cantor merely claims that the large velocities found are a corroboration of the fundamental idea of the theory that a gas at rest consists of many particles moving rapidly. I cannot find a record of any extension of these experiments such as would tally with Herzfeld's account.

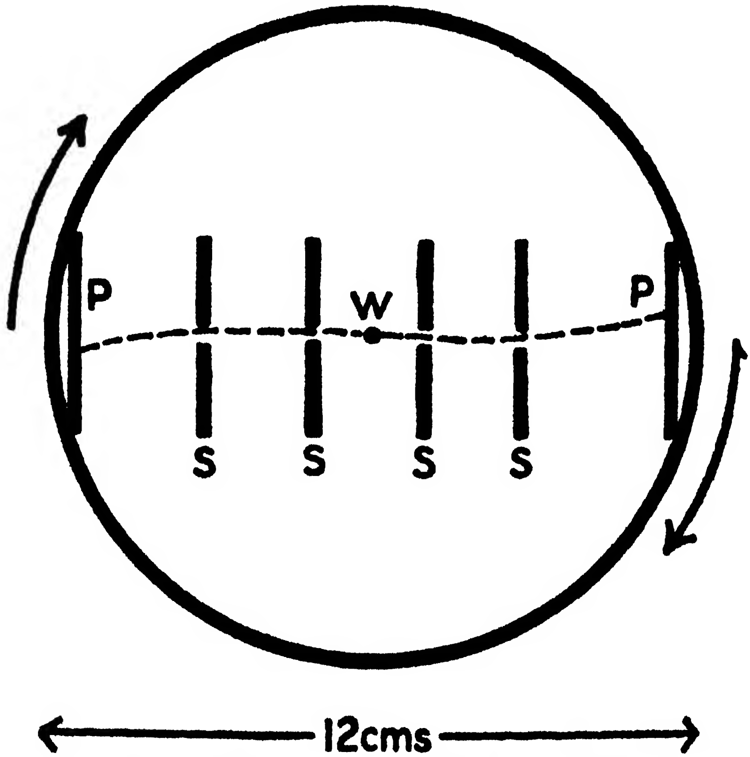
The partial eclipse suffered by the kinetic theory at the beginning of this century has long passed, and one ray of the new light has again fallen upon the problem of direct verification, giving rise to the work of O. Stern (*Z. für Phys.*, **2**, 49, 1920), that of J. L. Costa, H. D. Smyth, and K. T. Compton, on a "Mechanical Maxwell Demon" (*Phys. Rev.*, September 1927), and the very similar experiments of J. A. Eldridge (*Phy. Revs.* December 1927). We may say, at once, that since Maxwell's time the devilkin has apparently grown capable of more complicated work. Instead of being required to sit on his fence putting the second law of thermodynamics out of countenance with both hands, he must now definitely sort out the molecules on his right hand, according to their speeds, while leaving his left hand unoccupied. Adopting this new definition, Stern's apparatus is as impish as either of the others. In Stern's experiments, then, neutral silver atoms are evaporated from a hot wire which forms the axis of a cylindrical chamber. A thin beam of these, selected by a couple of slits whose openings are parallel to the wire, is allowed to condense in a fine line on a target plate, also contained within the highly evacuated chamber. The target plate and set of slits is now given a rapid motion perpendicular to the radius of the cylinder, by whirling round

the axis at high speed. Of those molecules which emerge from the last slit, the faster, reaching the plate before, will imprint themselves in a different line from the slower; and thus we have at once a velocity spectrum in silver, upon the target plate. The density of the mark at different distances from the line made on the still plate, if measured photometrically or otherwise, would determine the relative numbers of molecules travelling with velocities accurately determinable from the dimensions of the apparatus and the speed of rotation. That at least was the intention. In practice, the matter was not so simple. To melt the silver, a high temperature is needed, and the velocities of the atoms are therefore considerably higher than at ordinary temperatures. Thus, in order to obtain a large dispersion on the plate, a very rapid rotation is necessary. In point of fact, as many as 2,700 revolutions per minute, first in one direction and then in the other, produced a distance between the marks made by the atoms moving with the most probable velocities, of only 1·2 mm. Under the circumstances, detailed verification of Maxwell's Law was out of the question. It might appear that the desired dispersion could be obtained by increasing the dimensions of the apparatus. But this would at once enhance the great difficulty of avoiding collisions with other molecules, of air or silver, which would complicate the interpretation of results. As it was, difficulty enough was experienced in maintaining a high vacuum, while employing a rotating axle communicating with outside, a difficulty overcome by interposing a rougher vacuum between the free atmosphere and the chamber. The distance, 1·2 mm., however, is closely compatible with the assumption that the silver atoms travelled with velocities distributed according to Maxwell's Law, supposing their temperature to be that of the hot wire, or a little higher.

Since, both in this experiment and in the next, some doubt may be allowed as to the applicability of Maxwell's Law, we may, following his own method of derivation, bear in mind what kind of gas it is meant to describe, viz. one composed of a very large number of molecules, whose average energy over a sufficient period is finite, and in which the velocities are distributed symmetrically with respect to direction, at random around a favoured value with respect to magnitude. In Stern's experiment, this last is the stumbling-block. His atoms were only those which succeeded in escaping from the matrix, and were therefore probably all above a certain minimum velocity.

In the work of Costa, Smyth, and Compton we are even less sure of the appropriateness of Maxwell's Law. For here the molecules are deliberately drawn into the apparatus by

pumps, and cannot be in the condition just defined. The experiment itself is none other than an ingenious application of Fizeau's method for the velocity of light, modified to measure the speed of molecules. The familiar toothed wheel is paired with a second on the same axle—for reflectors are not permissible—and distances are reduced to suit the speeds. The jet



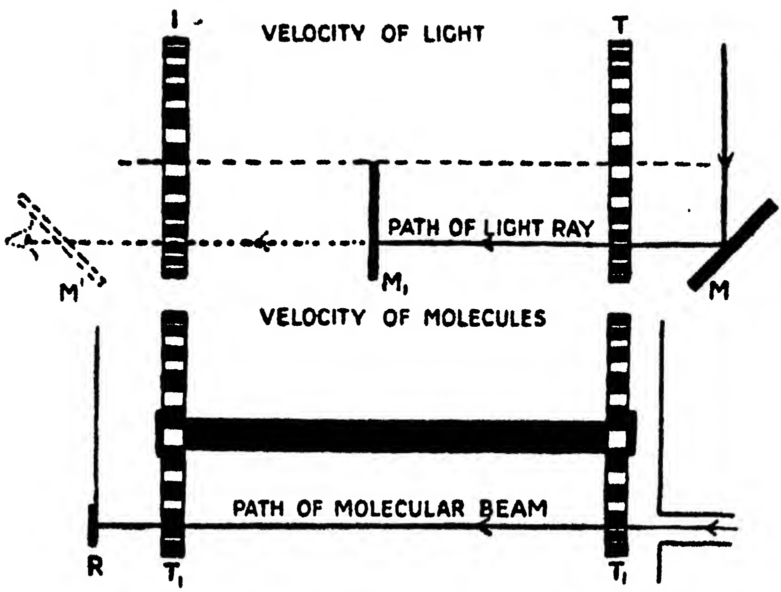
PLAN OF STERN'S APPARATUS (DIAGRAMMATIC ONLY).

W is the section of the glowing wire along the axis, S are the slits which limit the beam. P are the target plates upon which the atoms condense. The dotted line represents the path of the molecules relative to the plate P, when the whole arrangement is rapidly rotated about W in the direction of the arrows.

of gas, drawn into the evacuated apparatus, is directed against the rim of one wheel while the axle revolves at a convenient speed. Molecules whose speeds are within certain ranges calculable from the angular speed and dimensions of the apparatus, will pass between the teeth of the second wheel, deflecting a radiometer vane behind. If a Maxwell distribution is assumed, the pressures on the vane may be calculated and compared with those derived from its deflection. Some agreement is found, but not such as to verify Maxwell's Law.

Since the radiometer is an instrument whose action is not simple, it is not clear what law the experiments do indicate. Although something more appears to be claimed, the result is qualitative as yet. We conclude that these experiments do not demonstrate the Law.

Neither do those of Eldridge, and for similar reasons. Here we have something of a combination of both predecessors, in that a metallic vapour (cadmium) is projected into the



Fizeau's Method and the New Maxwell Demon.

The figure compares the principles only. I and M are the images of the tooth-wheel T and the glass sheet M_1, in the mirror M. T are the rigidly connected toothed-wheels of the molecular apparatus, R the radiometer vane.

apparatus and condensed at the end of its journey to form a mark on a cold target, while the Fizeau rotating wheel system is employed to define the molecular path. The cadmium is finally condensed on the cold moving target and spread out in a fashion exactly analogous to Stern's velocity spectrum; and the measurements are made in the same way. What has been said of the others applies largely to these experiments, with these qualifications: firstly, that much greater care has been taken to eliminate collisions between molecules; and secondly, that the temperature is much lower than in Stern's case.

In conclusion, we may ask why this matter has revived at this time. With regard to the first American experiments, no reason except that of general scientific interest appears.

Although capable of doing the work of Maxwell's " Individual " (as his creator named him), these mechanisms are not used to that end. Why, then, should they be employed to demonstrate a law which in many ways is already proved ? The answer to these questions may probably be found in the advent of new theories of gases. Thus we have quantum theories of gases, of which that of Einstein might be mentioned ; while the new wave-mechanics, in so far as it may be regarded as the statistical mechanics of systems which are mutually dependent, must cast reflections on older theories which regard gases as assemblies of independent systems. It is to be anticipated that molecules exert a more potent attraction upon one another when they are closer together, or are moving slower, that is, at high pressures and low temperatures. Calculations for these regions generally become very difficult, but the indications of the quantum theory are, according to Stern, that a difference of 1 per cent. in the calculated velocities may be expected for hydrogen at the boiling-point, and one atmosphere pressure. The need for some experimental test is clear ; Stern's method was indeed tried with that object in view, and more may yet be heard of it. The high pressures required are, however, a very grave, if not insuperable, difficulty. It will be observed that in none of the above cases was any attempt made to attain it. That experiments of the thermionic type are entirely out of the question is abundantly evident.

We are bound to conclude that while the new experiments have not illuminated the old ground, they may represent the pioneer attempts at one of the most difficult experimental problems in physics.

DINOFLAGELLATES

By MARIE V. LEBOUR, D.Sc., F.Z.S.
Naturalist at the Plymouth Marine Laboratory

Dinoflagellates, or Peridinians, constitute a group of unicellular organisms which are of peculiar interest in many ways, being on the borderland between plants and animals, and second only to the diatoms in importance as food for those animals which mainly subsist on these minute creatures both in the sea and in fresh waters.

The earliest known dinoflagellate, and perhaps still the best known of the group, is *Ceratium* (Fig. 1), so called from the horns which protrude from the three angles. In size it is a good deal less than a millimetre, its body is composed of clear protoplasm and it is provided with a covering, the so-called theca, transparent and composed of cellulose or some closely related substance. Round the body is a groove, the girdle, in which is lodged a long vibrating thread which is more or less whip-like and therefore called a flagellum. This thread vibrating in the groove gave rise to the name dinoflagellate, or peridinian, both of which are derived from a word meaning a whirlpool. Running posteriorly is a second groove which lodges another flagellum projecting straight out behind. These transverse and longitudinal grooves thus respectively lodge the transverse and longitudinal flagella which are the organs of locomotion. The transverse flagellum encircling the cell is continually vibrating; the longitudinal flagellum trails or lashes backwards and forwards and by means of the combined movements a typical twirling forward progression is effected.

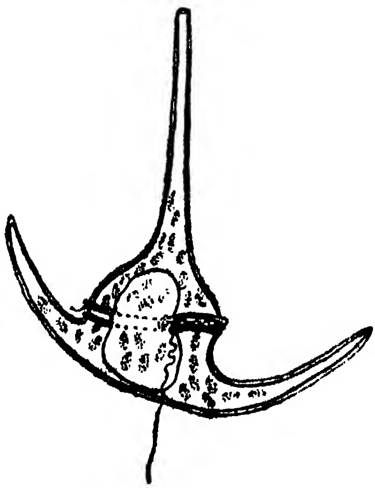


Fig. 1.—*Ceratium.*

Inside the protoplasm are small bright yellow bodies, the colour-bearing bodies, or chromatophores, and by their aid the organism feeds, for they contain a colouring matter closely related to chlorophyll, the green colouring matter of plants, and by means of this in the presence of sunlight carbon dioxide can be split up and the carbon utilised for building up complex foodstuffs which make up the protoplasm of the cell. Ceratium thus feeds like a plant: it is holophytic. Other forms may contain green or brown chromatophores.

Ceratium is mainly marine, although there is one very common fresh-water species. In the sea the long horns help in enabling it to adjust itself so that it can keep in those layers where the light can penetrate and where it may occur in enormous numbers.

There are very many different kinds of dinoflagellates, ranging from extremely simple cells to species so highly advanced

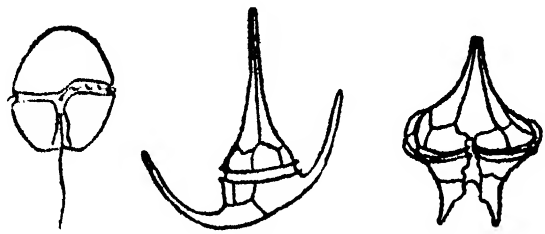


FIG. 2.—(*a*) Typical naked dinoflagellate. (*b*) *Ceratium* showing theca composed of plates. (*c*) *Peridinium* showing plates.

that they seem to herald the approach of the Metazoa, or multicellular animals. Their interest lies in this variety, and we shall now proceed to review the group as a whole and show how many interesting problems are suggested by their study.

We have naked forms with a covering so thin that it can collapse upon the slightest provocation, and we have thecate forms, such as *Ceratium* and *Peridinium*, which possess an armature persisting after the death of the cell. This theca may be composed of many plates arranged in regular patterns (Fig. 2).

The naked species form a special division and amongst them are probably the most primitive; but also in it we have the most highly developed of the whole group.

A simple naked dinoflagellate consists of a mass of protoplasm with a nucleus, the outside being bounded by the thinnest pellicle. A groove lodging the transverse flagellum encircles the body and a longitudinal flagellum lodged in a ventral groove protrudes behind. In the protoplasm may be yellow,

brown, or green chromatophores, in which case the organism is holophytic, or the cell may be colourless or very pale pinkish or yellowish without chromatophores, or with brightly coloured plasma. In these latter cases we may often find food remains inside the cell. Sometimes these can be clearly recognised as

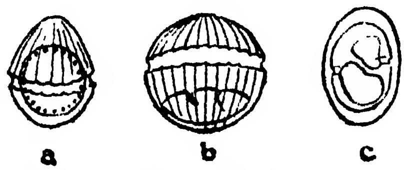


FIG. 3.—Dinoflagellates with food inside. (*a*) With diatom. (*b*) With dinoflagellate. (*c*) Encysted to digest its food.

other dinoflagellates, diatoms of flagellates (Fig. 3), or in the case of large species even multicellular organisms. The cell has engulfed its food; it is feeding like an animal. It is holozoic. Sometimes a species with chromatophores can eat holozoically too. It can eat in two ways, both as a plant and as an animal.

The botanist claims the dinoflagellates as plants. The zoologist claims them as animals. Both have good grounds, for there is no distinction between plants and animals except in their feeding, and this often breaks down when we reach the unicellular forms. Dinoflagellates form a group on the borderland, perhaps more animal than vegetable. They may thus feed like plants or animals. Most of the naked forms living in the open sea are holozoic and are often found with enormous food masses inside them. The mouth region is the longitudinal groove, which is mobile and capable of much distension. The whole cell may be greatly distorted when a large meal is being taken. Sometimes it is even necessary for the creature to encyst and form a large covering round itself whilst digestion is taking place, so that it may be at peace and undisturbed. Perhaps the most striking carnivorous form is one known as *Polykrikos* (Fig. 4). This is common round our coasts, and is peculiar in that it is compound, being composed of a chain of individuals, eight in number, perpetually connected together. It has eight transverse grooves marking the individuals, but it is a curious fact that the nuclei lag behind and are fewer than the grooves, usually four in a chain of eight. Instead of regarding this as a compound unicellular organism some regard it as one that is on its way to be a metazoon, or multicellular organism. *Polykrikos* has progressed so far that it actually contains stinging capsules,

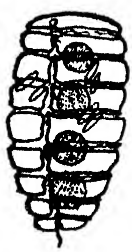


FIG. 4. *Polykrikos.*

or nematocysts, similar to those belonging to hydroids and jelly-fishes (Fig. 5). The structure is essentially the same. Probably everyone knows the stinging cells of Hydra, the Fresh-water Polyp, consisting of a cell containing the nematocyst with its coiled thread and barbs. On exciting

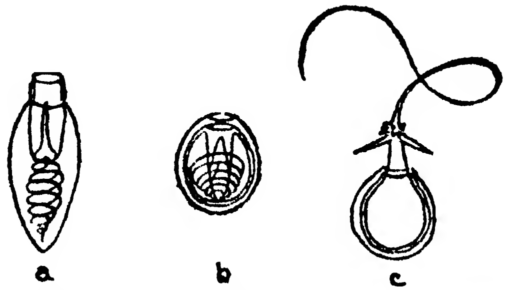


FIG. 5.—(*a*) Nematocyst of *Polykrikos*. (*b*, *c*) Nematocyst of *Hydra*, at rest and everted.

this the barbs and thread are thrown out, the barbs first; then the hollow thread, like the inverted finger of a glove, is gradually everted. In *Polykrikos* we have a capsule containing one barb and a thread which probably acts in a similar way, the thread bearing a poisonous fluid paralysing the food before it is engulfed.

One other genus in this group contains stinging capsules, and on this account is called *Nematodinium* (Fig. 6). This is a very

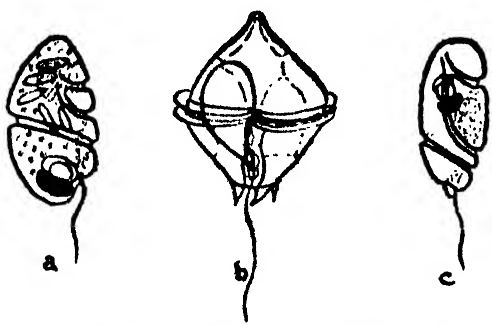


FIG. 6.—(*a*) Nematodinium. (*b*) *Peridinium* showing pusule. (*c*) *Pouchetia*.

tiny creature, about forty thousandths of a millimetre. Besides having stinging capsules it also possesses a large eye with a beautifully formed lens. It has also chromatophores suggesting holophytic as well as holozoic nutrition. *Pouchetia* is colourless, with a well-developed eye, and may be so twisted that both

girdle and longitudinal groove curve more than once round the body. This brings us to the highest forms of all, which have eyes with complex lenses and also a large prod which projects from the region of the longitudinal groove (Fig. 10). Nobody has ever seen the use of this prod, but one may presume that it is a sort of tentacle for catching food as the forms possessing it are holozoic. Other kinds have been known to send out pseudopodia-like processes, similar to Amœba, for catching food.

Amongst the naked dinoflagellates there are also saprophytic species living on decaying organic matter and parasitic species living on or in other animals or plants.

The thecate forms are a still larger division. These may be holophytic or saprophytic, but so far none are known to be holozoic. A very large number are colourless, pale yellow or pink, and of their method of feeding we know very little. Probably they are saprophytic. To be certain of their method of nourishment is one of the most important dinoflagellate problems. In all of these there are large vacuole-like organelles known as pusules (Fig. 6). These are not contractile vacuoles such as are present in Amœba, where the fluid collected from inside the cell is ejected into the water; the fluid in this case is taken into the pusule from outside. In the longitudinal groove of the theca is a pore through which both flagella pass; the pusule opens into this. It strongly suggests feeding on fluid substances, especially as the pusule is very large in these forms but smaller or even absent in those with chromatophores.

The theca is usually pierced with minute pores putting the protoplasm in connection with the surrounding water, and there may be larger openings besides the flagellar pore, notably one at the apex known as the apical pore. Many thecate forms have chromatophores, and some of the presumably saprophytic forms are capable of making starch. These have colourless plastids, the leucoplasts. Fat is very often contained in the plasma.

We do not know much about reproduction in this group. Division into two, usually oblique, is frequent. Thus *Ceratium* divides into two across the theca, half having to be newly made in each daughter individual. Nearly all the naked forms divide in this way, sometimes encysting before doing so. Sometimes there is a complicated life-history entailing two or three stages, as in *Gymnodinium lunula* (Fig. 7), the Pyrocystis of the older writers, which is a very small naked dinoflagellate, with girdle and two flagella. In some way as yet unknown it encysts, and a relatively enormous round cyst results. In this the cell divides up into two, four, and eight individuals, first the nucleus, then the cell, and each of the eight resulting daughter cells is surrounded by a half-moon-shaped cyst. These are at first

packed into the round cyst, are liberated, and the contents of each one divides again into two, four, six, or eight individuals which become like the original free swimming cell and escape from the cyst ; and so the cycle goes on.

Prof. Kofoid of California, who has worked much on this group, is of the opinion that these complicated life-histories are common. So far we know only a very few.

There are many parasitic dinoflagellates. Some of these are external and live on small free-swimming marine animals or on the bristles of diatoms. Others live inside small crustacea or even in their eggs. In many copepods, which are very small relatives of the shrimp, they encyst, forming an enormous bag where the first cell divides into many spores, to be finally freed into the sea as small dinoflagellates with grooves and flagella.

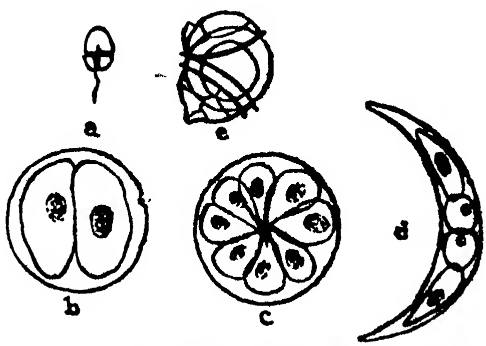


FIG. 7.—(*a*, *b*, *c*, *d*) *Gymnodinium lunula*, showing spore formation. (*e*) *Peridinium*, with spore.

Spore formation is common in the thecate forms, the theca dividing either at the girdle or between the plates to allow the exit of one, two or more spores, each of which develops into an individual similar to the parent (Fig. 7). *Ceratium* may form a single spore. In certain laterally flattened marine forms with the theca divided laterally into two halves there is a special arrangement for enlarging the cell before division, a thick piece forming between the halves so as to distend the theca greatly. These are known as megacytic forms and are usually full of fat. When the cell divides the enlarging piece is lost and a new half is added to each individual. The jug-like species, *Dinophysis* and *Phalacroma*, all have megacytic stages (Fig. 8), and may be colourless or pink, in which case they are probably saprophytic, or they may have greenish or yellowish chromatophores and are holophytic. These have the girdle far in front and guarded by winged membranes. Their relatives in the ocean have the membranes enormously enlarged to facilitate flotation.

Noctiluca, one of the chief sources of phosphorescence in

the sea, is the largest dinoflagellate known. Its nearest relatives are the tentacle-bearing forms. Interpreted as a dinoflagellate, it has a much-reduced girdle and transverse flagellum, owing to the enormous distension of the body due to vacuoles forming inside to enable it to float easily in the sea. Free-swimming spores are formed here with a more conspicuous girdle.

Gough, when working at the Plymouth Marine Laboratory, made some important and most interesting investigations into the division and rate of multiplication in *Ceratium*. Working at night by means of successive tow-nettings taken every few hours, he found that *Ceratium fusus*, a very elongated species, divided rapidly at night from about midnight to 3.30 a.m. By 9.30 the formation of the new cells was complete, and from his observations he came to the conclusion that the rate of division for each cell would be once every two days under favourable conditions. In this way we could get enormous masses of one species, and *Ceratium fusus* is very luminous and often the cause of large expanses of luminescence in the sea. Rapid division in other forms gives rise to the phenomenon of "red water" in tropical seas. This is usually caused by a thecate form, *Goniaulax polyedra*, but may sometimes be due to naked forms. Recently a new species was found to be the cause of large expanses of "red water" in Japan. The sea suddenly becomes a deep red or chocolate colour, the cause being the enormous masses of these organisms with brown or orange-yellow chromatophores (Fig. 9). The sea is coloured sometimes for miles and for a depth of some metres. The death of the cells and subsequent sinking into lower regions whilst decaying is the cause of the death of many marine animals, even fishes, which live at the bottom. Kofoid has given pictures from California of large skate and other fishes, sea-cucumbers, crabs, etc., flung dead on the beach after one of these invasions of "red water." Apparently the floating life is not affected, many creatures feeding on the cells. The Pearl Oyster may feed on it and is in no way disturbed.

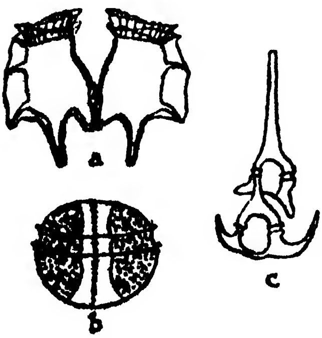


FIG. 8.—(*a*) "Twin Form" of *Dinophysis*. (*b*) *Phalacroma*, megacytic form. (*c*) Heteromorphic chain of *Ceratium tripos* (after Lohmann).

Many other dinoflagellates occur in large numbers and are valuable food for various animals. Pilchards on the French coast are often found full of them, and the Egyptian Pilchard constantly takes a *Ceratium* as part of its usual food.

An interesting feature in some dinoflagellates is their habit of chain formation. In its simplest form two individuals after division remain in connection (twin forms) (Fig. 8), or in other cases several may remain attached. In *Ceratium* chain formation is frequent. On division the cell often does not come away from its sister cell, but remains attached by its apical or front horn to the ventral groove of the individual in front. In this way a long chain may be formed.

Another very interesting phenomenon in certain *Ceratium* species is the formation of what is known as heteromorphic chains. One individual may divide and redivide, and a chain is formed. As the chain lengthens the latest formed individual at the front end gradually changes its shape so that at one end of the chain the form is quite different from that at the other end. In *Ceratium tripos* one of the commonest and widely distributed of the marine Ceratia, the front individual is elongated and small, with short hind horns, whilst the hind one is the ordinary *tripos* form with long curved hind horns (Fig. 8). These heteromorphic chains are usually formed where the conditions are abnormal. At Kiel, where the salinity is low, they are frequently present. Dr. Jörgensen of Christiania, who has studied these chains deeply, is of the opinion that the short-horned forms are degenerate. If one examined the individuals at either end one would certainly regard them as different species. It is curious that the short-horned forms approach the very widespread freshwater species *Ceratium hirundinella*, which possibly bears out the theory that salinity has something to do with it, which may very well be, as the long curved horns of the marine forms fulfil a certain purpose in keeping the cell in the upper layers, which probably is not so necessary in the stiller, fresher waters.

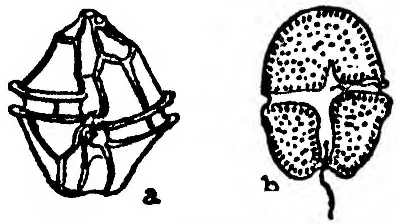


FIG. 9.—Dinoflagellates causing "red water." (*a*) *Gonyaulax polyedra*, the chief cause in America. (*b*) *Gymnodinium sanguineum*, the chief cause in Japan (after Hirasaka).

Environment has a profound influence on dinoflagellates. In fresh water we get chiefly the simpler forms usually holophytic and not with any specially adapted appendages. Small ponds and lakes may be full of them; generally swift-flowing streams are without them. In brackish water there may be many, some of which form an important part of the food of such sedentary animals as the Cockle, Mussel, and Oyster. On sandy beaches at low tide there are often to be found large greenish-brown patches which on investigation are seen to be

enormous masses of dinoflagellates. The sand-loving species have adapted themselves to a special mode of life in a most successful manner. Several genera may be represented, but all have the same characteristics. They are all flattened, either dorso-ventrally or laterally, they all have a mobile longitudinal groove, often produced into a flap by which they cling to the sand grains (in this way lying flat they are safe when the waves come over them), they nearly all have deep yellowish-brown chromatophores and are holophytic. They are all fairly small and have rather short flagella, so that although they may move rapidly they are not long-distance swimmers. Here we have predominantly the genus *Amphidinium*, with its girdle far forward and usually flattened dorso-ventrally; but it is interesting to find that there are certain laterally flattened forms with a much thicker covering which lead up to the jug-like genera *Phalacroma* and *Dinophysis*, which strongly suggests the origin of these from *Amphidinium*—possibly an origin from very shallow water. Perhaps the most interesting of all is a *Polykrikos* discovered by Miss C. Herdman, with a very much flattened cell and yellow chromatophores, without any trace of nematocysts, and, moreover, only two nuclei to its eight individuals. Again suggesting a possible coastal origin for this peculiar genus.

Miss Herdmann has devoted much time to the study of these sand-loving forms at Port Erin, Isle of Man, where her father, Sir William Herdman, originally discovered the brown patches in the sands caused by *Amphidinium* (Fig. 10). Port Erin is not, however, by any means the only known locality for these. There are many on the Northumberland coast and in Wales. On the Californian coast at La Jolla, where much work has been done on dinoflagellates, many of these sand-loving species have been found which are of the same type, but usually different species from those found in Britain. No doubt a careful survey of our sandy beaches would bring many more to light.

Miss Herdman finds a certain daily rhythm in the appearance and disappearance of these organisms in the sand, depending on light and tide; a dim light such as occurs in the morning and evening when the sky is overcast producing the largest masses, together with uncovering at low tide, when most of the species come to the surface. Besides the daily rhythm she finds a lunar rhythm, where the largest number occur at spring-tides, the lowest at neaps.

Next to the beach come the coastal waters and the open sea. Near the coast are a number of dinoflagellates, generally of a simple form, belonging to many genera, holozoic, holophytic and saprophytic, but as we approach the open sea their character is more distinct. The naked forms are often more highly

coloured, beautiful yellows, pinks, purples, and even blue occurring. These highly coloured species are nearly all holozoic, the plasma itself being coloured or containing pigment or coloured oily masses, but rarely containing chromatophores. These particularly beautiful creatures are extremely voracious, and are often found quite distorted with the amount of food they have succeeded in taking into their small bodies. In the open sea many of the naked forms tend to be twisted on the main axis, so that the girdle is twisted from 1½ to 4 times round the body, the longitudinal groove also being twisted (Fig. 6). A characteristic of the dinoflagellate cell is its asymmetry, and this is often accentuated in the open-sea forms, an adaptation which enables them to adjust themselves easily in the sea water. These are often provided with broad membranes attached to

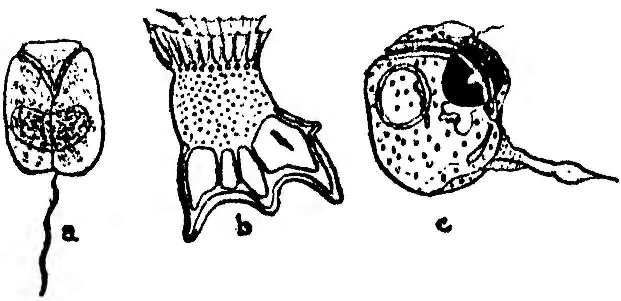


FIG. 10.—(*a*) *Amphidinium Herdmani*, causing discoloration of the sands. (*b*) *Ornithocercus*, showing expanded wings (after Schütt). (*c*) *Erythropsis*, showing eye and " prod " (after Kofoid and Swezy).

the girdle, or longitudinal groove, which may be enormously developed and prevent sinking as a relatively enormous surface is exposed. We see this in the horns of *Ceratium*, which are often flattened or have broad membranes attached, but we see it best of all in the truly oceanic genus *Ornithocercus* (Fig. 10), which has greatly developed wings. Dr. Johann Schmidt informed me that, in his collections from the open Atlantic on his " Dana " Expedition to the Sargasso Sea to investigate the life-history of the eel, various species of *Ornithocercus* were much in evidence. This is very likely the headquarters of the genus. Nearly all those forms with large eyes and a prod live in the open sea, and most of the largest naked species (Fig. 10).

Dinoflagellates are lovers of warmth, although many occur in northern waters. In cold climates, however, they are scarce in winter and are at a maximum in summer. On our own coasts about February, with the coming of more sunlight, those minute plants, the diatoms, which constitute the floating flora

of the sea, occur in quantities; soon after these the dinoflagellates appear, staying all the summer and well into the autumn, but dwindling almost to nothing in the winter. They do not like the absence of sun.

The survey of the group is now finished, and several points of interest have arisen.

We have in the dinoflagellates a group on the borderland between plants and animals. There are various problems relating to nutrition. Nearly related is the luminescence so common in these organisms. Any disturbance of the surrounding water causes this to appear, and there are descriptions of fishes and mammals coming into the area of these luminescent organisms and their path being illuminated by the light given forth by these tiny creatures. Then there are problems of distribution and adaptation, the latter shown very well in the sand-loving and many oceanic forms. Their response to differences in environment and hydrographical conditions is marked. There is the problem of their origin. They are usually believed to be descended from a flagellate-like ancestor. Did they originate in fresh water, brackish, or sea water? All the simplest forms of all the groups occur in fresh water. Is this their original home? *Ceratium hirundinella*, the common fresh-water species, is enormously widely distributed. As far as we know, the farther we go out to sea the more highly organised are the dinoflagellates.

Perhaps, however, the most interesting problem of all is their relation to the multicellular animals. We see them clinging together after reproduction, first two, then several together. We see them as two, four, or eight individuals in one which cannot be separated. We see forms with nematocysts, characteristic of Cœlenterates, which are highly organised animals, and with eyes with beautifully formed lenses. Surely these facts are highly suggestive in studying the evolution of all animals.

NOTES

Obituary: E. H. Ross and H. C. Ross (R. R.)

My two brothers, E. H. and H. C. Ross, were born in England in 1875, and were therefore eighteen years younger than myself. They were *identical twins*, and were almost exactly alike. Educated at the Isle of Wight College, they entered the medical profession through St. Thomas's Hospital. They qualified in 1898 and joined the Royal Navy as Medical Officers about 1902.

My work on malaria had been completed in 1899 as regards its scientific bases. My efforts to get the public to reduce mosquitoes were opposed or ridiculed for many years, and, in fact, produced a great hostility against myself. But at the end of 1905, Sir Horace Pinching, Director-General of the Public Health Department of Egypt, wisely decided to commence anti-malaria work in that country and persuaded my two brothers to resign their permanent posts in the Navy in order to enter his department in Egypt, promising them permanence of tenure; simply in order that they might follow my teaching as regards mosquito-control. Everything went well for a time (see the account given by H. C. Ross in my *Prevention of Malaria*, Murray, 1911, p. 498); and Sir Horace Pinching was very pleased with my brothers' work. E. H. Ross had almost changed Port Said, where he was appointed Health Officer, had cleared out most of the cesspools under the houses, and had reduced very largely the mosquitoes (as well as the scoundrels) who made the town, as H. C. Ross says, "One of the most disgusting 'holes' on the face of the earth." Unfortunately Sir Horace Pinching, who had given every help, was forced by the rules of his service to retire in the autumn of 1907. I do not know exactly what happened, but apparently his successor (whose name I am glad I have forgotten) was not so favourably disposed towards mosquito-control and began to adopt tactics which are well known in certain services towards juniors of whom any senior happens to disapprove (or is jealous of). I naturally tried to fight for my two brothers, and wrote several letters to the Foreign Office about the matter, but could obtain no redress; and

was finally told to stop writing about them at all. My brothers had thus lost permanent appointments, apparently without any faults of their own. They came to Liverpool, while I was a Professor there. Fortunately, Mr. J. McFadden, a rich American gentleman, began to employ H. C. Ross on studying cancer, which he did with the result that he formed a new theory regarding the pathogenesis of the disease, which was that certain chemical substances generally or often produced at the site of any lesion, such as a tumour or an injury, had the effect of producing local over-healing by making the cells there reproduce very greatly. He called such chemical substances "auxetics," and he demonstrated this auxetic property in the chemical substances referred to by showing under the microscope that they could force human leucocytes to divide under the observer's eye (*Induced Cell Reproduction and Cancer*, H. C. Ross; Murray, 1910).

He was, however, as unlucky as his twin brother; scarcely any one of the British Cancer Specialists could be persuaded to accept his hypothesis. I, myself, verified the auxetic effects of one of these chemical substances and carefully examined a stained specimen which H. C. Ross had prepared, and wrote an article on the subject (*Proc. Roy. Soc. Med.*, 1911, vol. v, pp. 103–8). This note was read at the Laboratory Meeting on November 7, 1911. In this particular specimen many of the leucocytes were actually fixed during the process of division—a thing which, of course, is never seen in an ordinary specimen of finger blood. Unfortunately, that kind of preparation was very difficult to make. Still more unfortunately, Mr. McFadden died during the war, and his researches were concluded about 1919, and both E. H. and H. C. Ross were thrown out of work.

H. C. Ross died on board ship coming home from Ceylon on December 14, 1926, and E. H. Ross died in London on March 20, 1928. They had both been very unfortunate.

Disillusion. By an Economic Entomologist (F. W. Dry, D.Sc.)

The occupation of a general practitioner in Economic Entomology is a fascinating one. An assortment of problems exercise his wits. His profession usually affords pleasant surroundings, change of scene, and a generous allowance of fresh air. In such laboratory investigations as he can carry out, interspersed between spraying demonstrations and nursery inspection, he is not subjected to the strain of prolonged microscopical study. True, he at times may be taken at a disadvantage. An outbreak will occur of an insect pest which he does not know how to control. Probably he can find in the literature some remedy

to suggest, and invites reports on its success. But when these arrive in such shape as a telegram, " Absolute failure," his position is a little embarrassing. In his answers to inquiries sympathy is now apt to take the place of advice, and he may permit himself to point out that he does not know sufficient—and lacks the opportunity to discover it—about the ways of life of the creature, the factors which determine its abundance, and its relation with its environment. But give him the chance to do this fundamental research, what then ?

Some few years ago in Kenya it fell to me to devote most of my time to work on the Coffee Bug and its two chief egg parasites. About three-quarters of the eggs laid by the bug are parasitised. A female bug is able to produce many more offspring than a female of either parasite species, but the life-cycle of the bug is much longer, and on balance both species of parasites can multiply at a considerably greater rate than their host.

It was found in the life-history work that unmated females of both species of parasites produced just as many offspring as those that were fertilised, but with parthenogenetic reproduction, as is so often the way with hymenopterous parasites, all the offspring were male. Mated females, however, produced about three females to one male, and as a similar ratio was obtained in parasites reared from eggs from the field one could be sure that sexual reproduction was the rule there. Under laboratory conditions, however, unless careful attention were given to the comfort of the insects, one of the two species did not yield the normal quota of the more important sex. The numbers of the sexes would be more nearly equal, which was wasteful, for a male was perfectly willing to accept even more than his ordained allowance of three wives. This matter, had breeding parasites for distribution become desirable, would have been of distinct importance.

The varying fortunes of the two parasites were followed in the field for several years. Now one was in the ascendancy, now the other, the percentage of eggs parasitised by the two species combined varied, and so did the abundance of the Coffee Bug itself. In the laboratory it was found that the two parasites responded differently to increased temperature, one having its life-cycle speeded up more. A consistent difference in fertility was also found. These facts, unfortunately, did not help very much in explaining the field records. One species, it is true, did show on the whole greater powers of multiplication, and this species was found to be more abundant in the field when Coffee Bug eggs were plentiful. When these eggs were scarce the advantage appeared to lie with the other species, and the obvious suggestion was made that that species was the more

successful at hunting out eggs when their discovery entailed prolonged search.

The object of all this work was to seek an understanding of the interactions between the three species, the host, and the two parasites. As stated the problem is by no means simple, and there were other complications. Had the aim been achieved it might have been possible to forestall outbreaks by liberating additional parasites of the appropriate species, or of the two species in correct proportions. At the same time, subsidiary to this undertaking, another project demanded attention, although here no progress at all was made. This was the rearing and feeding of coffee bugs on some inexpensive medium so that parasites might be reared on a large scale without the waste of coffee berries otherwise entailed in the support of a large colony of the bugs.

Here came a rude awakening. No longer is there any call, from the coffee planter's standpoint, to press further this imaginative enterprise. Some practical-minded exponent of Applied Entomology, instead of discovering how to keep coffee bugs alive without coffee, has found out how to kill them in their haunts. He employed a poison bait spray. Some excuse may indeed be pleaded for not having made use of that expedient. Spraying of any kind was thought to be useless. The attempts that were made were unsuccessful. The insect's chitin is pretty tough, and as it pierces portions of the plant with its proboscis and sucks the juices from below the surface stomach poisons were unlikely to be effective. One was familiar with the use of a poison bait spray against the fruit-fly, but the idea of attempting such a defence against the Coffee Bug simply did not present itself. But someone tried it —just a little arsenic, a little coarse sugar, dissolved in water, flicked lightly over the bushes—and in Kenya, Uganda, and Tanganyika it has been found to work.

Some years ago the writer of this note was discovered, uninvited, in a field of swedes, and their irate owner was the reverse of pacified by the explanation that he was pursuing a certain insect in order to amuse himself. That assuredly was the measure of his achievement in studying those egg parasites in Kenya. Prof. P. F. Kendall, lecturing once to a class of students, explained a certain reluctance to put into print the results of the research about which he had been talking. "I did not carry out this work," he said, "so that I might publish a paper. I did it to satisfy my curiosity."

Modern Alchemy (J. G. F. Druce, M.Sc. (Lond.), R.Nat.Dr. (Prague), F.I.C.)

The strong current of experimental science during the nineteenth century is generally held to have proved the im-

possibility of realising the dream of the alchemists, namely the transmutation of base metals into gold. Attempts with this object in view are supposed to have ceased at the close of the eighteenth century, when it was established that each metal was a fixed elementary body possessing characteristic properties of its own which rendered it distinct from the rest. Whilst some alloys and mixtures could be made to simulate other metals, it was realised that true transformation had not occurred.

The nineteenth century was not without a few dreamers who claimed to have made gold from other metals. One " adept " indeed has even survived to this day. Like his mediæval predecessors, he projects some transmuting powders, arsenic and antimony sulphides, upon molten metal to obtain a golden product which it is asserted actually contains that metal. Such is the claim of M. F. Jollivet Castelot,[1] President of the Société alchimique de France.

During the nineteenth century one or two unscientific men laid claim to have obtained gold from other metals. Mention may be made of a certain Frenchman Tiffereau, who in 1840 obtained gold from Mexican silver by treatment with nitric acid in the heat of the sun's rays. Failure to repeat his results in France was attributed to the feebleness of the sunshine, but it subsequently transpired that the Mexican silver contained appreciable amounts of gold—enough to account for that isolated by Tiffereau.

The idea of transmutation has appealed to the imagination of many unacquainted with science. Thus the Scandinavian writer, Strindberg, mistook some films of iron hydroxide for gold. For some reason he soaked paper in solutions of green vitriol and exposed them to his tobacco-smoke. This caused the appearance of a thin golden, metal-like film on the paper. It arose from the action of the alkaline smoke on the salt. According to Strindberg, the film did not show the chemical tests for iron, but this is very doubtful.

Alchemy in the sense of the search for the Philosophers' Stone to convert base metal into gold is a thing of the past, but attempts at the transmutation of one element into another are still attracting attention.

These attempts are inspired by the phenomena associated with radioactivity. The individual elements uranium, radium and thorium are continuously undergoing disintegration. In every case the final metallic product is lead. These disintegrations are natural processes which, so far, have not been subjected to experimental control or alteration. Efforts to imitate

[1] *Études d'Hyperchimie : Chimie et Alchimie*, par F. Jollivet Castelot. Pp. 285. Paris : Librairie critique, Émile Noury, 1928.

such transformations have occasionally been partially successful. Thus in 1919 Sir Ernest Rutherford [1] disintegrated atoms of nitrogen into hydrogen by bombardment with the alpha-particles given off by Radium-C.

Sir William Ramsay's conversion of hydrogen into helium [2], and copper into lithium have not been confirmed, and it cannot be asserted that the numerous recent attempts to obtain gold from mercury by electrical methods have been substantiated, although they are not without interest.

According to modern views on the atom it should be possible to obtain gold by the electrical bombardment of the mercury atom, whereby this on losing an alpha particle should leave gold. This deduction has stimulated a number of physicists to try various methods of dislodging an alpha particle from the mercury atom. The Japanese Professor, Nagaoka [3], and his co-workers (1923) tried the effect of a large spark discharge on the surface of mercury emulsified with oil. The carbonaceous residue was stated to contain traces of gold which were detected with a spectroscope.

Two Germans, Drs. Miethe and Stammreich [4] found gold in the black deposits on the walls of old mercury vapour lamps.

It is well known that the ordinary mercury of commerce often contains traces of gold which are very difficult to remove, and the gold claimed to have been made by Nagaoka and by Miethe and Stammreich is usually believed to have been present originally in the mercury they used. Many attempts have been made by Prof. Smits [5] to convert lead into gold, but nothing conclusive has been proved.

The conversion of (inflammable) hydrogen into (inactive) helium has also engaged attention. Indeed in 1926 Drs. Paneth and Peters [6] of Berlin published some experimental evidence which seemed to prove that they had actually effected this transmutation of one gas into another without the intervention of radioactivity. Later they announced that the helium which was shown to be present had leaked into their apparatus from the air. It had not been produced from the hydrogen they used.

REFERENCES

1. Sir E. Rutherford. *Phil. Mag.*, 1919, xxxvii, 537, 562, 571, and 581, and later papers.
2. Sir W. Ramsay. *J. Chem. Soc.*, 1913, ciii, 264; Ramsay and Cameron, *ibid.*, 1908, xciii, 966.
3. Nagaoka, Sugiura, and Mishima. *Jap. Journ. Phys.*, 1923, ii, 121.
4. A. Miethe and H. Stammreich. *Naturwissenschaften*, 1924, xxv, 597; and *Physik. Zeitschr.*, 1925, xxvi, 842.
5. A. Smits. *Nature*, 1926, cxvii, 13 and 613.
6. F. Paneth and K. Peters. *Ber.*, 1926, lix, 2039.

Yellow Fever in West Africa (R. R.)

The Thirteenth Annual Report of the International Health Board (Rockefeller Foundation) has some good remarks on Yellow Fever in West Africa. On page 153 it decides that "the results of the studies are as yet inconclusive." So we have thought. There are many reasons for doubting whether the severe non-malarious fever found in West Africa is really the same as the yellow fever of the American regions. I argued the point in a paper (*Trans. Soc. Trop. Med. and Hyg.*, vol. iv, No. 8, July 1911); but many writers seem to ignore all these considerations. In the meantime the International Health Board is quite right in acting with great reserve toward the question. However, from the recent literature, the consensus of opinion seems to be that the yellow fever in West Africa is clinically and pathologically similar to yellow fever in South America, notwithstanding the fact that in none of the African cases has the *Leptospira icterodies* been found. (See "Experimental Transmission of Yellow Fever to Laboratory Animals," *American Journal of Tropical Medicine*, 1926, viii, 2 (March), by Adrian Stokes, Johannes H. Bauer, and N. Paul Hudson. Also "The Relation between Weil's Disease and Yellow Fever," by Dr. Andrew Watson Sellards, in the *Annals of Tropical Medicine and Parasitology*, vol. xxi, No. 2 July 22nd, 1927.)

The Development of Loch Ness (Lieut.-Col. W. H. Lane)

There has been considerable controversy during the past few months in connection with the Inverness-Fort William road. In regard to this sector of the Glasgow-Inverness highway the section which will entail the heaviest expenditure for construction will probably be from Inverness to Fort Augustus. The very fact that at each end of a stretch of this section there stands a notice warning motorists to drive with caution over the intervening space of ten miles is sufficient evidence in itself to justify the assumption that this length of ten miles will necessitate a new alignment of the roadway from one extremity to the other. Now, once it is admitted that a new alignment of the roadway will have to be carried through a major portion of this section, then it becomes a matter of common sense to aim at obtaining the best alignment possible. The question then arises—does such an alignment exist? It certainly does exist, but that part of the route bordering Loch Ness is at present submerged. Briefly speaking—the best alignment for this reach would lie along the present north shore of Loch Ness. It will be obvious to anyone, who is at all acquainted with the district, that if the surface-level of Loch Ness could be lowered

sufficiently, the foundations of a roadway, which would stretch from one end of the loch to the other at a dead level, and without any dangerous corners, would be laid bare. The bottoming of the road would be *in situ*; the material for the surfacing would be at hand. Could any engineer hope for a better legacy from nature? The problem then resolves itself into the following proposition. Would it be possible profitably to lower the surface-level of Loch Ness so as to admit of an 18 ft. highway being constructed along its northern shore? That query might be answered in the affirmative provided such a project would prove advantageous in other ways as well.

Initially—the fundamental basis of a scheme for the development of Loch Ness would rest on the construction of a canal *at sea level* connecting Loch Ness with the Moray Firth.

There would not appear to be any insuperable engineering difficulties in the way of cutting such a canal, and the cost would not be excessive. What advantages, then, would accrue if such a scheme were put into effect? Ships of any size could steam or oil direct into Loch Ness. They could thus berth at railhead—Fort Augustus. Fort Augustus would therefore become the port for the north on the east coast with Fort William the port on the west coast, a distance of a little over thirty miles separating the two ports. Surely such an undertaking would prove profitable from a trading standpoint?

How would it affect the railway company concerned in relation to its traffic returns? With the advent of shipping of any and every denomination to Fort Augustus this branch line would, *ipso facto*, be converted into a main line connecting the east coast with the west coast, and the scale of profits should be increased in proportion.

The next point at issue would be the benefits derivable hydro-electrically. First of all, by the construction of this canal an additional 52 feet or so of fall could be secured for any hydro-electric scheme. Two rivers of considerable volume empty their waters into Loch Ness, the Oich at Fort Augustus, and the Moriston at Invermoriston. It would be possible, at comparatively small expense, to harness these two rivers for the purpose of generating electricity of such power as would prove efficient and remunerative commercially without interfering with existing interests. Then the cutting of this canal would, as has already been stated, uncover an almost perfect alignment for the Glasgow-Inverness highway. Furthermore, it would render possible the construction of an electric railway along the south shore of Loch Ness, thus linking up Inverness with Fort Augustus.

Finally, the adoption of the scheme outlined for the development of Loch Ness would open up a wide vista of possibilities

navally. During the Great War the Loch Ness basin was constituted a "military" area under the Admiralty. With the addition of a canal capable of accommodating the largest battleship connecting the Moray Firth with Loch Ness; of a supply of electricity of enormous power; of railway and road facilities, not to speak of natural defences which could be so utilised as to render the "area" almost impregnable against attack from the air, could not the scope of the "area" be extended to meet the exigencies of the times?

A Sanitary Tragedy

E. A. Seagar reports a very unhappy circumstance in the *Journal of Tropical Medicine and Hygiene*, March 1, 1928, namely the entry of malaria into Barbados. Hitherto the disease seems to have been entirely absent there, and the case is similar to that of the entry of malaria into Mauritius in 1866. One would have thought that the local authorities would have taken more care regarding the entry of dangerous mosquitoes, but we suppose that the same thing that happened there has happened in most British possessions, where mosquitoes are apparently allowed their freedom without let or hindrance. Thirty years have now elapsed since the mosquito theory of malaria was fully established, and yet we hear that the insects abound in most British colonies as much as they do in many other places. The authorities always seem to think that the disease malaria does not cost them anything. It generally doubles the death-rate or more, as well as being an enormous tax on agriculture. Really, it is time to ask whether the British nation would not do well to hand over all its tropical colonies to the United States, who certainly show some intelligent interest in this matter. Now, in future, the Barbados Government will be forced to pay considerable sums in consequence of this new plague which has been allowed to enter the colony, and apparently it is scarcely possible to eliminate the disease altogether in future after entry. I have always maintained that antimalaria sanitation is the principal test of efficiency in any local government.

Easter Massacre of Lambs (R. R.)

The Times of April 27 publishes a paragraph from its correspondent at Corfu on the massacre of lambs there at Easter time. I was in the Lake Copais district on Easter Sunday in 1913, and on that day was driven up the hill to the Valley of the Muses on Helicon, where we were given a very good lunch of roast lamb by a Greek farmer. Since then I have always found great difficulty in eating lamb, but have

generally succeeded in doing so in spite of my repugnance. What struck me on that day, which was a beautiful still morning, was the layer of smoke all down the valley, which, my host said, was due to the fires employed in cooking the lambs. But what also struck me extremely unpleasantly was that the numerous flocks of sheep which we met on the way seemed all to be huddled together with fear. The lambs had just been removed from the ewes, which appeared to be conscious of their loss. It was like the Massacre of the Innocents described in the Bible, except that ewes were substituted for the human mothers. Unfortunately the massacre does not go on only in Greece. One often wonders whether the human race will ever abandon its horrible carnivorous habits, or whether it can ever do so, even if it wishes. The daily slaughter of animals for food must amount to many millions. Suppose that we all try ovolactarianism combined with a little vegetarianism. There is something really horrible in the idea of killing and devouring young lambs and kids.

The Captivity of Orang-utans

Sir Hesketh Bell published a very interesting though pitiful article in *The Times* in May regarding some sixty orang-utans which had been sent from Sumatra to Europe. These animals are, as he says, " magnificent specimens, giants of colossal strength," who feed " on the fruits and succulent leaves of their choice " in the great jungles of that island. They are now exiled from their home and imprisoned for life, presumably in various zoological gardens, where they will probably ultimately die—for what reason ? Simply to indulge the curiosity of *homo insipiens* and a very poor class of *homo insipiens* at best. There were a number of mothers with their young ones amongst this party, and their sufferings during the long voyage to Europe, penned in small cages in which they could not stand upright, must have been extreme. Men like to preach that we may all go to heaven some day, but I wonder what the orang-utans will say to this theory. It is a question whether the real apes do not always stand outside the cages.

We quite agree with Sir Hesketh Bell that something ought to be done to limit this trade, which appears to be almost as immoral as the ancient slave trade.

Notes and News

The names of those selected for election as Fellows of the Royal Society this year are as follows : G. Andrep, M.D., D.Sc., physiologist ; H. Bateman, M.A., Professor of Mathematical Physics in the Institute of Technology, Pasadena, California ;

C. H. Browning, M.D., Professor of Bacteriology, University of Glasgow; S. S. Cook, engineer with the Parsons' Marine Steam Turbine Co.; W. D. Dye, D.Sc., Head of the Electrical Standards Section of the National Physical Laboratory; C. C. Farr, D.Sc., Professor of Physics, Canterbury College, New Zealand; Major Greenwood, F.R.C.P., Chairman of the Statistical Committee of the Medical Research Council; J. W. H. Harrison, D.Sc., Lecturer in Zoology at University College, Newcastle-on-Tyne; W. N. Haworth, D.Sc., Professor of Chemistry in the University of Birmingham; D. Keilin, M.A., University Lecturer in Parasitology, Cambridge; F. L. Kitchin, Sc.D., Palæontologist to the Geological Survey of Great Britain; F. S. Macaulay, mathematician; S. B. Schryver, D.Sc., Professor of Biochemistry, Imperial College of Science and Technology; W. Stiles, Sc.D., Professor of Botany in the University of Reading; R. Whytlaw-Gray, Professor of Chemistry, University of Leeds.

The following awards have been made by the scientific societies concerned:

The Huxley Memorial Medal of the Royal Anthropological Institute to Baron Erland Nordenskiöld for his work in South America; the Messel Medal of the Society of Chemical Industry to Dr. R. A. Millikan; the Gold Medal of the Linnean Society to Dr. Edmund Beecher, Professor of Zoology in Columbia University, New York; the Perkin Medal of the Society of Dyers and Colourists to Dr. E. R. Schmidt of Elberfeld for his work on anthraquinone; the Gold Medal of the Institute of Mining and Metallurgy to Sir Alfred Mond.

Prof. T. F. Thorpe has been elected President of the Chemical Society and Dr. J. A. Bather President of the Palæontological Society.

Dr. A. Muller has been appointed Assistant Director of the Davy Faraday Research Laboratory.

The deaths of the following well-known scientific men have been announced during the past quarter: Prof. A. Abetti, Italian astronomer; Sir D. Ferrier, neurologist; Prof. W. W. H. Gee, physicist; Mr. E. W. Maunder, astronomer; Prof. R. Pribram, chemist; Dr. F. Raschig, chemist; Prof. T. W. Richards, For.Mem.R.S., chemist; Mr. F. W. Shurlock, formerly Principal of the Derby Technical College; Mr. A. Siemens, electrical engineer; Prof. E. Wiedemann, physicist.

We would remind our readers that the annual meeting of the British Association will be held at Glasgow during the period September 5–12. Reduced railway fares will be available and the provisional programme holds promise of a most interesting week. Sir William Bragg's presidential address will deal with *Modern Developments of the Physical Sciences*

and their Relation to National Problems, and two evening discourses, entitled *The Study of Popular Sayings* and *The Mystery of Life*, will be delivered by Prof. E. A. Westermarck and Prof. F. G. Donnan. The lochs, golf-courses, and engineering works in the district provide unusually good opportunities for excursions.

The very interesting addresses delivered on the occasion of the centenary of University College, London, last year have now been published by the University of London Press (separately, price 1*s*. each; bound together in cloth, 12*s*. 6*d*.). The scientific articles relate the progress which has been made in science and engineering during the last hundred years, and form most valuable summaries of the outstanding achievements of science described by men who themselves have played no small part in the advance. Sir Oliver Lodge dealt with Physics; the late Prof. Starling with Physiology; Prof. Fleming with Electrical Engineering; and Prof. Collie with Chemistry.

The *Year-book of the Universities of the British Empire* for 1928 (London, G. Bell & Sons; price 7*s*. 6*d*. net), published by the Universities Bureau of the British Empire, contains all the features which, in the past, have made it an indispensable handbook to those concerned with university affairs. These include statements of the courses and facilities for research available at all the universities, statements of pre-entry qualifications, of fees, scholarships and degrees. There are, in addition, lists of the members of the staff at each college (with an alphabetical index) and a great deal of general information which should prove most helpful to both graduate and undergraduate students.

The Department of Scientific and Industrial Research has decided to publish a monthly periodical entitled *Building Science Abstracts*, modelled on the lines of *Biological Abstracts*, or *Chemical Abstracts*. The cost will be met in part by contributions from the Institute of Builders and the abstracting will be done by the staff of the Building Research Station at Watford. (Annual subscription, 10*s*.; single copies, 9*d*.; or 10*d*., post free, from H.M. Stationery Office.) The need for such a periodical will be realised when it is stated that abstracts will be gathered from over one hundred scientific and technical journals; but it is, nevertheless, rather surprising that the wealthy building trade should need any subsidy from the D.S.I.R. to obtain it; the incomparably poorer scientific professions manage somehow to support their own abstracts.

Gauthier-Villars, the well-known French publishers (55, Quai des Grands-Augustins, Paris), have just commenced the publication of a series of monographs under the general

title *Mémorial des Sciences Physique.* The series will be conducted along the lines of the existing monographs devoted to mathematics and is edited by MM. W. Villat and J. Villey. Each volume will contain about sixty pages, and will give as clear and concise an account as is possible of some definite problem of current interest. The first two parts have already been published. Fascicule I, entitled *La Mécanique ondulatoire*, by M. Louis de Broglie, is entirely mathematical, and contains a good account of the development of this new branch of physics up to the end of 1926. Fascicule II, *La Télémetrie monostatique*, by M. Armand de Gramont, is, on the other hand, entirely descriptive. It contains an account of the principles employed in various types of optical range-finders and discusses the accuracy which can be obtained with a few selected examples. The volumes in preparation—some sixty in number—cover the whole range of modern physics. The price is 15 francs, plus postage, etc.

The Committee appointed by the Safety in Mines Research Board to advise on the question of further research regarding the use of wire ropes in mines, has embodied in a Paper a summary of the present state of knowledge on this subject; and this Paper is published under the title, "Wire Ropes in Mines, some Notes Regarding their Manufacture and Use" (Safety in Mines Research Board Paper, No. 41, H.M. Stationery Office; price, 1*s.* net).

The first part of the Paper gives an account of the manufacture and properties of the wire used and of the construction of the rope itself. For the information of those who have not specially studied metallurgy, these notes are introduced by a section on the manufacture of steel.

The rest of the Paper relates to the selection and use of wire ropes under mining conditions. There are chapters on the factor of safety, the various types of capping, the renewing of the capping, lubrication, the drum, the handling and care of a rope, the examination of ropes in use, and the deterioration of ropes during service. Guide ropes, balance ropes and haulage ropes are considered as well as winding ropes.

A list is given of the more urgent outstanding problems in regard to which research is in progress. The Chairman of the Committee states in an introductory note that some interesting results have already been obtained and will be published shortly.

The Report of the Proceedings of the Imperial Agricultural Research Conference, held last autumn, is now available (H.M. Stationery Office, Adastral House, W.C.2, price 1*s.* + 5*d.* postage). The work of the conference was reviewed in the April number of SCIENCE PROGRESS by its Secretary, Mr.

W. R. Black. The next conference is to be held in Australia in 1932.

The Eighth Annual Report of the British Non-ferrous Metals Association shows that that body is doing its utmost to assist the engineering and metallurgical industries in this country, but that the manufacturers themselves are, as a whole, indifferent to the work which is being accomplished. The number of subscribing members remained stationary during the year dealt with in the report; difficulty was experienced in bringing the trade contributions up to the minimum required to obtain the D.S.I.R. grant, and the Council of the Association was compelled to appeal to the Department for a modified agreement covering the period ending December 1929. The increased grant sought for has been provided, and it is hoped that a very considerable increase in membership will have been secured before the post-grant period begins in 1930. The research staff of the Association was engaged during the year on twenty-one problems of great industrial importance. Several patents were sealed for the benefit of the members of the Association, notably one dealing with new ternary alloys applicable to cable sheathing and lead-pipe manufacture. The Association is doing most excellent pioneer work in starting a new Development Section to bridge the gap which must always exist between the research laboratory and the factory. The purpose of this section is to demonstrate to the manufacturer that the new methods developed in the laboratory can be used, even in *his* factory, and that they are worth while. Thus to quote from the Report (p. 23):

"It would probably have been of little or no avail to send to a pipe maker copies of the results of the work done at Woolwich. Instead, a full-scale experimental extrusion was carried out at the works of a pipe maker and the whole process from the alloying to the final testing of the pipe done by the Development Section. The result was that the pipe maker, instead of being called upon to read a report probably unintelligible to him, witnessed palpable pipe coming from his own press and exhibiting properties of manufacture and subsequent performance markedly better than his standard product. Thus his interest was vividly aroused, and if at this stage there is any obstacle to the manufacture of ternary alloy pipe on a large scale it is certainly not lack of appreciation of the superiority of the material on the part of this particular maker; its benefit and economics have been clearly and positively demonstrated."

The Non-ferrous Metals Association is to be congratulated on tackling this obvious if difficult practical problem, and

we can only hope that its enterprise will meet with due appreciation from the trade before it is too late.

The Report of the National Institute of Industrial Psychology for 1927 shows that the fees paid for the services of the Institute during the year amounted to £11,774 as compared with £8,600 in 1926, while the membership increased from 984 to 1,138. The number of persons applying for vocational guidance more than doubled, and a large and varied series of investigations of works methods were carried out, apparently to the satisfaction of all concerned. One investigator, for example, obtained practical experience as an omnibus conductor, and was able to make many recommendations concerning the collection of passengers, the comfort of omnibuses, and the simplification of the work of the conductors. Previously the methods used when giving the vehicles their nightly wash had been simplified and perhaps, later on, the Institute will be commissioned to discover a means whereby *all* the metal polish on the brass work may be removed without the assistance of the passengers. The appeal fund reached a total of £21,211 towards the total of £100,000 required to put the Institute on a permanent footing and further contributions are urgently needed and as thoroughly deserved.

Preliminary arrangements are being made for the Fifth International Botanical Congress, which will be held at Cambridge from August 16 to August 23, 1930. Prof. A. C. Seward has been appointed Chairman of the Executive Committee, and the Secretaries are F. T. Brooks, 31, Tenison Avenue, Cambridge, and T. F. Chipp, Royal Botanic Gardens, Kew. Early notice of intention to attend the Congress is requested by the Treasurer, Dr. A. B. Rendle, Natural History Museum, London, S.W.7, to whom the membership subscription of £1 must be paid before April 1, 1930. The Congress will be divided into the usual sections, at which papers will be read at the invitation of the sectional sub-committees. The discussions will be conducted in English, French, or German.

We have received a copy of the first number of the *I.C.I. Magazine* (January 1928)—the staff magazine of Imperial Chemical Industries, Ltd. It contains a number of articles, together with reports of a social character from the various factories of the combine. Mr. Henry Mond contributes an outline of the labour policy of the company, which has a capital of £56,000,000 and employs from top to bottom some 50,000 men. The key-notes of the policy are Security, Co-partnership, and Contact: by these it is hoped to maintain the present contentment of the staff and to produce in each man the feeling that he is not lost in the complexity of the machine. Security is afforded by a new grade—the staff grade—to which

all workers may be promoted after five years' service. The privileges of this grade include payment at weekly instead of hourly rates, payment for bank holidays and for all certified sickness up to six months, and, finally, guarantee of a month's notice at the termination of employment. A good attempt to minimise the outstanding evil of our artificial civilisation—uncertainty of tenure.

Messrs. Sidgwick & Jackson have sent us the three parts of a work entitled *Classified Problems in Physics*, by D. B. Briggs, M.A., of Bradfield College. Part I contains exercises on Mechanics and Hydrostatics (pp. viii + 128, price 3*s.* net); Part II is devoted to Electricity and Magnetism (pp. viii + 128, price 3*s.* net); and Part III deals with Heat, Light, and Sound (pp. viii + 184, price 3*s.* 6*d.* net). The standard is that of the First School Certificate examination, and a small proportion of the questions is drawn from papers set by various examining bodies at that examination. They are therefore of the direct make-the-candidate-think-as-little-as-possible type so much in favour at the present time, and a good drilling in them would undoubtedly form a firm foundation for success at any first school examination. Each section of the various subjects is preceded by a short outline of the principles involved in the solution of the exercises and by a selection of worked examples. Taking the volumes as a whole, Mr. Briggs has performed his task quite admirably and only a few minor blemishes are to be found. Among these are the absence of any proper units for momentum ("f.p.s. units" coming from a candidate is not acceptable), the use of the term ohm/cm. cube instead of ohm-cm. as the unit of resistivity and the adoption of a familiar sign convention in geometrical optics which most experienced examiners know to be quite useless at this stage.

The papers sent to us this quarter by the Bureau of Standards, Washington, are of a very technical character and hardly suitable for abstract here. Attention may, however, be drawn to a detailed discussion of the theory and practice of Maxwell's method for the absolute measurement of capacitance written by H. L. Curtis and Charles Moon (Scientific Paper, No. 564), and to a description of a method for the determination of nitrogen in the gases from metals fused in a vacuum by L. Jordan and J. R. Eckmann (Scientific Paper, No. 563).

Dr. O. Leyton has produced an excellent work on the *Treatment of Diabetes Mellitus* (Adlard & Son, 21 Hart Street, W.C.1). It discusses nearly all the points which arise in connection with this progressive subject, in which so many patients are interested.

The Seventh Congress of the Far Eastern Association of Tropical Medicine, which was held in Calcutta last December, and of which a full account has appeared in the *Indian Medical Gazette* of March and April, has also published a souvenir of the Indian Empire, which is full of matters of interest (Thacker's Directorate, Ltd., 6 Mangoe Lane, Calcutta).

Dr. C. Strickland, M.A., B.C., Professor of Medical Entomology, School of Tropical Medicine and Hygiene, Calcutta, assisted by K. L. Chowdhury, M.B., D.P.H., in collaboration with a number of well-known workers, has published an abridged Report on Malaria in the Assam Tea Gardens, with pictures, tables, and charts. The Report ought to be studied by all planters and doctors in Assam. It is published by the Indian Tea Association, Royal Exchange Buildings, Calcutta.

CORRESPONDENCE

To the Editor of SCIENCE PROGRESS

MATHEMATICS FOR STUDENTS OF TECHNOLOGY

I. *From* L. B. BENNY, M.A., F.R.A.S.

DEAR SIR,—In a notice of my book, *Mathematics for Students of Technology*, which appeared in a recent issue of SCIENCE PROGRESS, the reviewer stated that " the only serious error of principle occurs in the treatment of indices."

While I do not object to any comment, favourable or otherwise, and have never previously wished to criticise reviews of this book, or any other of my writings, it is, I think, permissible to reply to a direct charge of this nature. Failure to do so would naturally be interpreted as acknowledgment of its accuracy.

The charge made by your reviewer is quite unfounded. Possibly he was misled by the preliminary paragraph, in which a graphical introduction is given, designed to show that the logical method of dealing with indices other than positive integers, is to give them such meanings as conform to the index laws which apply to positive integral indices. The algebraic treatment that follows is then entirely conventional and logical.

If your reviewer will examine the contents of pages 140–2 again I believe he will agree with me, and withdraw his charge. Should he persist in maintaining it, I shall at least be sinning in good company, including the writer of every textbook on algebra that I have ever examined, from Chrystal downwards (or is it upwards ?).

I would add that the book has been carefully examined by other competent mathematicians, and has been reviewed widely ; apparently no other reader has been misled by the graphical introduction to which I have referred.

Yours faithfully,

L. B. BENNY.

MUNICIPAL COLLEGE,
BOURNEMOUTH,
May 10, 1928.

II. *From* R. C. J. Howland

Dear Sir,—Mr. Benny appears to believe me so incredibly careless as to have criticised his book without having first troubled to read it. I assure him that this is not so. Nor does a second reading of the pages he mentions incline me to change my opinion.

The primary error made by Mr. Benny, in common with many other writers, is in believing that, when indices are redefined in terms of surds, it is unnecessary to reprove the index laws. The truth is that each definition is framed to conform at first with only one index law. The assumption without proof that the other laws will also remain true belongs to the type of mathematical mysticism that believes our symbols to be under some inner necessity of behaving well, however shabbily we may treat them.

Contrary to Mr. Benny's statement, Chrystal is not his fellow sinner in this respect. On page 182 of his first volume, after having defined $x^{p/q}$ in the usual way, he writes, "We have now to show that the meaning just suggested for $x^{p/q}$ is consistent with all the Laws of Indices . . .", and this he proceeds to do. His proofs are, I think, not wholly satisfactory, but he is in no doubt as to the necessity of giving them.

Mr. Benny's other sin (the word is his own) consists in making his treatment of surds dependent on that of indices. Since the fractional indices are defined in terms of surds, it is evident that something, at least, must be known about surds before the laws of indices can be proved. Mr. Benny's confusion of mind on this point is sufficiently shown by the following, taken from p. 145 of his book:

"*To prove that* $\sqrt{ab} = \sqrt{a} \times \sqrt{b}$.

We have $\sqrt{ab} = (ab)^{\frac{1}{2}} = a^{\frac{1}{2}} \times b^{\frac{1}{2}} = \sqrt{a} \times \sqrt{b}$."

Since $x^{\frac{1}{2}}$ has been *defined* as equivalent to $\sqrt{x}$, this "proof" may be read,

$$\sqrt{ab} = \sqrt{ab} = \sqrt{a} \times \sqrt{b} = \sqrt{a} \times \sqrt{b},$$

after which a Q.E.D. would be an impertinence.

It is true that Mr. Benny sins in a numerous company, but there are textbooks, notably the *Algebra* of Barnard and Child, which avoid this particular error. To make the treatment quite rigorous and also quite easy is not possible, but the argument can at least be arranged in such an order that, though there are gaps, there are no vicious circles.

In conclusion, I hope that my review made it quite clear

that I think Mr. Benny's book a good one. If it were not so, it would not be worthy of criticism on a point of logic. It is because he has made an unusually successful attempt to be at once practical and sound that I would like to see this particular blemish removed from his pages.

Yours faithfully,

R. C. J. Howland.

University College,
London,
May 23, 1928.

ESSAY-REVIEW

TOWARDS A QUANTITATIVE EMBRYOLOGY. By JOSEPH NEEDHAM, M.A., Ph.D. Being a review of **Les Bases Physiologiques de la Fécondation et de la Parthénogenèse,** by ALBERT DALCQ, Dr-és-Sci. [Pp. 274.] (Paris: Les Presses Universitaires de France. Price 35 francs.)

THE hope of most biologists is that their science shall in the due course of time become what is called " exact." This can mean only that although at present the expulsion of qualitativity from biology and the ordering of its material in quantitative schemes is far from complete, biologists hope that eventually this process will be accomplished. The accomplishing of it evidently involves the answers to many great and very difficult questions, among which the problem of what a species is and how it can be dealt with quantitatively is one of the worst. Nevertheless, this quantification of biological facts, this constant seizing upon those items in the external world of living things which can be expressed mathematically and about which the questions, How much? and How many? can be asked, has been proceeding at a steady and inevitable rate from a time already far back in the history of biology.

The more quantitative biology becomes, the more close are its relationships with physics and with chemistry. In linking up with them the marks of boundaries, plainly apparent to common sense, between the living dog and the dead stone tend to disappear, not in the sense that no differences exist between the two, but in the sense that the stone and the dog are products of the same forces operating in one case in a rather simple and in the other case in a rather complicated medium. That diversity in the external world, that pluralistic picture of the universe which is so dear to common sense, is thus given up for a kind of unification of our experience, for science has its monisms as well as metaphysics. Horrible things happen only when the two are confused, but then in full measure. We do not, indeed, often stop to think of the time when biology shall be an exact science, and it might be worth while pointing out to some that our object in life will not then lose its savour for being achieved, for new vistas of co-operation with the physicists will open out. By that time most of the jungles of " natural history "

will have been explored and biology and physics will go forward together as the investigations of the resultant aspects of the larger and smaller organisms respectively.

All that has been said about biology as a whole applies to embryology. Probably the first appearance of the quantitative in embryology took place in the fifteenth century in Italy. No one can read through Leonardo da Vinci's *Quaderni d'Anatomia* (anatomical notebooks) without being struck by the exceedingly quantitative way in which he deals with the facts of embryology. Aristotle and many other workers had made observations on the intestines of the embryo, but Leonardo's laconic "20 braccias of bowels" tells its own tale. It was Leonardo who noted a relationship between the length of the fœtus and the length of the umbilical cord, it was Leonardo who speculated on the specific gravity of the amniotic liquid, it was Leonardo who wrote of the increase in weight of the developing embryo with such penetration that one almost expects to see a growth curve as one turns over the page, only to realise with a shock that he lived two centuries before Descartes and four centuries before Minot. Nor is it without significance that Leonardo's embryological sketches, exquisitely beautiful in their drawing, are interspersed with little diagrams of wheels, cogs, chains, and mechanical devices. Modern embryologists are not sufficiently aware of the honour they ought to pay to the greatest of Renaissance Italians. Then in the eighteenth century there were a few continuations of this attitude of mind, notably in the work of G. Hamburger and J. B. Mazin, while the great Haller wrote on the growth-rate of the embryo of the chick, a part of his work which has not received sufficient attention. But evidently quantitative embryology could not proceed far while as yet the simple morphological groundwork had not been gone over, and all the researches of von Baer, Kölliker, the Hertwigs, and a great number of others in "entwicklungsgeschichte" were necessary preliminaries to mensuration and numeration rather than ends in themselves. Some workers, indeed, have recently maintained that on the contrary morphology was and is an end in itself, and one of them has suggested that the fundamental object of the morphologists is to come down to solid geometry instead of to causation as is obviously the physiologists' aim. No doubt there is an element of truth in this view, but in so far as any morphologist holds such an opinion of his goal he must admit himself to be an artist searching for the æsthetic experience of significant form rather than that understanding of how the thing works which seems to be the essence of scientific explanation.

Such a standpoint was not that of His, who, in his famous letter published in the *Proceedings of the Royal Society of Edin-*

burgh, vividly described the opposition he encountered among morphological embryologists in his attempts at " entwicklungs-mechanik." The opposition, however, was not long-continued, and since the time of His and W. Roux the mechanical quantitative conception of embryology has gone steadily on its way, followed more slowly, perhaps, than might have been expected, by the application of physico-chemical methods to embryology. It is with regard to the problem of fertilisation, standing, as it does, half within and half without the field of embryology, that the most far-reaching physico-chemical conclusions have been drawn. Such studies have brought important facts to light and have carried us a very long way indeed in our understanding of the fertilisation process.

In 1919 F. R. Lillie of the University of Chicago published his valuable little monograph, *Problems of Fertilisation*, but since that time an immense amount of work has been done on the subject and the time was overripe for a further and larger monograph. It was this work that Dalcq undertook, and now that we have the result to read in and to refer to, we can only say that it would have been very difficult to have done it better. His arrangement of chapters is curious in that they follow a reverse time-sequence to that followed by the egg-cell itself. After an introduction, there follows a chapter on the Physiology of Segmentation. Here the literature is very complete and special attention is paid to the theories of cell-division due to Gray, Spek, and others. It is possibly to be regretted that Dalcq seems to give undue credence to the measurements of intracellular p_H by the " micro-écrasement " method, and the changes which it is said to undergo during cleavage. The extremely important paper by Buytendijk and Woerdemann in the *Archiv f. Entwicklungsmechanik* on this subject is not mentioned, doubtless because it was published too late for inclusion. Dalcq does, however, emphasise Gray's criticisms of Vlès as regards rhythms of CO_2 production by segmenting echinoderm eggs. Other relevant papers not mentioned include the work of Rapkine on respiration of echinoderm eggs after fertilisation. But the chapter as a whole is excellent.

The second chapter deals with the period elapsing between fertilisation and the first cleavage, and includes discussion of spermatozoal movement, aggregation, agglutination, antagonism, egg-secretions, the receptive stage, the penetration of the sperm-cell and the effects accompanying it, the cortical reaction, monospermy, polyspermy, and the general cytology of fertilisation. This latter subject is continued in the third chapter, which deals with the cytology of the formation of the first mitotic figures, but speaks also of certain aspects of parthenogenesis. The fourth chapter is called " The agents of fertilisa-

tion," and is mainly concerned with the specificity of spermatozoa and eggs and with the means by which parthenogenesis can be brought about. In this connection a most valuable table is given which shows all the agents which have been successfully employed in producing parthenogenesis and the eggs in question. The means are divided into (*a*) mechanical processes such as brushing, shaking, and especially pricking, (*b*) physical processes, such as heating, subjection to various forms of wave-motion (light, X-rays) and hypertonic or hypotonic solutions, (*c*) chemical processes such as subjection to various acids and salts, and finally (*d*) agents taken directly from living bodies, such as blood-serum, blood, and cœlomic fluid. It would perhaps be going too far to say that a perfectly consistent theory of parthenogenesis is as yet available, but we are very near it as Dalcq's discussion demonstrates.

Chapter V takes up the question of the fertilisation membrane and deals with it thoroughly. Mention is made of Fauré-Fremiet's work with Ascaris eggs, but only to his demonstration of the part played by ascaristerol in one of the three membranes, and not to his much more important finding (which has, however, not been repeated) that there is a diminution in carbohydrate inside the egg-cell after fertilisation supplying material for the chitinous fertilisation membrane. In Chapter VI the physiological results of fertilisation are considered in detail, such as permeability of the cell-membrane, oxidation-rate, heat-production, and disappearance of the capacity to be fertilised. Here the bibliography is rather confused, for a paper by Mendel and Mitchell in the *Journal of Biological Chemistry* for 1911 is mentioned without any reference being given to it, and Loeb and Wasteneys' work on the starfish in which they could not confirm the rise in respiratory rate occurring at fertilisation found by Warburg on the sea-urchin also has no reference. Bialascewicz' name is also consistently spelt wrongly. A real criticism is that no explanation is given for Meyerhof's low calorific quotients, and indeed they are hardly mentioned at all, although they have considerable importance. Still more serious is the criticism that Dalcq still adheres to the van' t'Hoff temperature coefficient system (Q_{10}) instead of following the advances made by Crozier and his associates using the Arrhenius equation. Shearer's work on glutathione in sea-urchin eggs, again, is accepted without criticism.

The seventh chapter deals with the period from maturation to fertilisation, *i.e.* the dispersion of the nuclear material, the formation and disappearance of the germinative vesicle. Cytodieresis is also discussed. Finally in Chapter VIII Dalcq concludes his monograph by inquiring what is the essence of fertilisation. Membrane-formation, he says, certainly is not a

form of cytolysis, but their relations are coincidental. " Écartons donc toute idée d'une mise en marche de développement qui débuterait par une tentative de suicide." He discusses the double process which seemed to be revealed by the two agents which were necessary in Loeb's parthenogenetic methods. He devotes much space to Lillie's fertilising theories, and on the whole rejects them. He considers that as work proceeds in this subject there seems to be less and less reason for supposing that what goes on during artificial parthenogenesis tells us much about what goes on during fertilisation by spermatozoa. " The spermatozoon gives to the rhythms of cellular life a rapidity which inanimate agents do not succeed in imitating." But it would be impossible to abstract the conclusions of Dalcq and the book itself must be referred to for them. It is curious that in accepting the main basis of Child's physiological gradient theory to account for polarity in the unfertilised egg, Dalcq seems to regard it as at present incompatible with physico-chemical description, while Child himself never wearied of emphasising the contrary view.

But, as will have been observed during the course of this review, the criticisms have been for the most part of a detailed character and not concerned with fundamental defects. Indeed from these Dalcq's book is singularly free. It is a notable contribution towards a quantitative embryology, and will be of much help to those who walk in that path. If to it were added a definitive monograph on the physico-chemical aspects of embryology itself, leaving out the fertilisation process, the foundations would be laid for a fresh attack on these problems. Investigators would be able to ascertain easily what has been done and what should next be done. As for the promise of these lines of work, it reveals itself by its own light and needs no demonstratory pointings on our part.

REVIEWS

MATHEMATICS

Partial Differential Equations of Mathematical Physics. By A. G. WEBSTER, A.B., Ph.D., edited by S. J. PLIMPTON, Ph.D. [Pp. viii + 440, with 97 figures.] (Leipzig: B. G. Teubner, 1927. Price M.23 stitched, M. 25 bound.)

THE partial differential equations of mathematical physics are usually, one might almost add fortunately, linear, an important exception being the equations of motion of a viscous fluid. The investigator of a particular physical problem is usually aided by the special conditions appropriate to the materials to which his equations relate. It would, however, seem desirable to treat these equations from a purely theoretical standpoint, but such a treatment presents features which make the subject unusually difficult and tedious.

The author of the present work has reached a successful compromise by diluting the pure theory with copious applications to initial or boundary conditions of a general character. The book is extremely well written, an important and useful feature being the derivation of the most important general equations in a preliminary chapter in which vector methods are fully explained and freely used. There are a fair number of misprints, mostly of an obvious nature, but it may be noted that the contents of the bracket in equation 106′ on p. 38, for a viscous fluid, should read

$$\operatorname{grad} p - \mu \triangle v - \frac{\mu}{3} \operatorname{grad} \operatorname{div} v$$

the symbol $\triangle$ being used throughout for the Laplacian operator ∇^2. The methods of Cauchy, Green, and Volterra are fully discussed and are followed by a perspicuous exposition of spherical, cylindrical, and ellipsoidal harmonics. A special chapter is devoted to applications of these, but no mention is made of Whittaker's general solution of Laplace's equation in the form $\int_{-\pi}^{\pi} f(z + ix \cos u + iy \sin u, u)\, du$. The book concludes with a chapter on Integral Equations. The style is clear throughout, and the author has succeeded remarkably well in his endeavour to present a continuous and co-ordinated account of the linear equations of mathematical physics. The book is worthy of being regarded as a standard work on the subject with which it deals.

L. M. M-T.

ASTRONOMY

An Outline of Stellar Astronomy. By PETER DOIG, F.R.A.S. [Pp. 183, with 21 figures.] (London: The Draughtsman Publishing Co., 1927. Price 7s. 6d.)

IN this little book Mr. Doig has attempted a survey of many of the problems of recent astronomical research and, it should be said, has succeeded very well. The book is intended for readers who are familiar with the simple concepts

such as "magnitude," "trigonometrical parallax," and the like, and with the elementary principles of spectroscopy. What mathematics there is, is of a simple nature, and is mainly relegated to the several appendices. The author has taken considerable pains to collect and present in an attractive form a mass of statistical material which will be found useful by astronomers in general. The book is illustrated.

The first part of the book contains the observational data relating to the stars. A rather sketchy account of the spectral characteristics of the stars has been hidden away in an appendix—it would have been better to have incorporated this section in the text. On p. 19 there is a very good figure to show the preponderance of negative radial velocities of stars in the neighbourhood of the solar apex and of positive velocities in the neighbourhood of the ant-apex.

Part II deals with the "Nature of a Star." As an introduction, there is a brief summary, on the usual lines, of atomic structure and spectra. Then follows an account of Eddington's researches on the "Internal Constitution of the Stars," followed by a chapter on stellar evolution.

Part III deals with the Galactic System and the external systems—the island-universes—with a description of Hubble's work at Mount Wilson.

The book is in no sense a treatise. It can be warmly recommended to those who desire to keep abreast of the rapid advances in latter-day astronomical inquiry.

W. M. S.

The Constellations and their History. By the Rev. CHARLES WHYTE, LL.D., F.R.A.S., F.R.S.E. [Pp. xxii + 273.] (London: Charles Griffin & Co., Ltd., 1928. Price 10s. 6d.)

THIS work will be of value to beginners in astronomy, and not without interest to those who have passed the elementary stage. Dr. Whyte gives an account of the constellations and stars from primitive times and divides his work into seven chapters, occupying altogether about 270 pages. These deal with the Constellation Figures and their Origin, Diurnal Motion, the Distances of the Stars, the Nature of the Stars, the Signs of the Zodiac, and the Constellations North and South of the Equator. There are star maps for each month, and plate charts of the Northern and Southern Heavens. The other two plates, which represent stellar spectra, have been reproduced quite well. This book might be placed with advantage upon the shelves of municipal or school libraries.

H. S. T.

PHYSICS

Wien-Harms Handbuch der Experimentalphysik. Band XXIII, Teil 1. Phosphoreszenz und Fluorezsenz. By P. LENARD, F. SCHMIDT and R. TOMASCHEK. [Pp. xxiii + 741, with 162 illustrations.] (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1928, Price: M. 69 brosch. M. 71, geb.)

PROF. LENARD of Heidelberg is responsible for the successful accomplishment of a very large number of investigations on phosphorescent compounds, and it is therefore very fitting that he and his collaborators should contribute a volume on phosphorescence and fluorescence to such an important undertaking as the *Wien-Harms Handbuch der Experimentalphysik*. The study of these compounds has been somewhat neglected in England, at any rate in purely academic circles, and the huge accumulation of scientific knowledge which we owe to the German workers comes as a surprise to the general reader.

However, if we trouble to think of the great number of these compounds and the complexity of their behaviour, it is not surprising that it requires a large volume to set forth the accumulated knowledge in detail. This is the explanation of the fact that vol. xxiii of the *Handbuch* is published in two parts. Part I is now before us, and a very brief examination is sufficient to convince us that it exhibits the same excellent printing, binding, and illustration as its precursors. It deals with the general behaviour of pure phosphorescent compounds of the alkaline earths, with the intensity and total quantity of the light emitted by phosphorescent compounds, with photoelectric effects observed with such compounds, the measurement of their dielectric constants and their interpretation, the disturbing effects of light and pressure on their luminosity, the exact measurement of the energy stored by them after exposure to light and their absorption spectra. Some three hundred pages of the book are devoted to descriptions of the methods of preparation and the individual characteristics of pure phosphorescent compounds. The authors set forth the subject-matter in a very efficient manner, and whenever possible they fully describe the experimental arrangements and procedure. They take considerable pains to supply convenient sectional references which enable the reader to check for himself the correctness of every important statement or generalisation which they make, and to supply valuable summaries of the main features of interest throughout the book. Clearly, it represents an extremely valuable addition to our available sources of information on these important subjects.

In the second part of this volume, the authors will deal with the decay of phosphorescent compounds, the molecular properties of the molecular aggregates or centres which exhibit phosphorescence and fluorescence, the luminous effects observed when crystals are broken, scintillations, and the luminous effects produced by chemical reactions at room temperature and by living organisms. It is very unfortunate, however, that in this volume the authors think it necessary to employ the term Weber instead of the term ampere for the practical unit of electric current. Whatever may be the historical justification it is somewhat late in the day to initiate such a change.

L. F. BATES.

Neuere Methoden und Ergebnisse in der Hydrodynamik. By PROF. C. W. OSEEN. [Pp. xxiv + 337, with 7 figures.] (Leipzig: Akademische Verlagsgesellschaft, 1927. Price M.22, stitched; M.24, bound.)

EULER's Paradox, that the "ideal" fluid of classical Hydrodynamic Theory offers no resistance to a body moving with uniform velocity, is a defect which robs the mathematical theory of fluid motion of much of its reality. The historical development of mechanics has always proceeded from the ideal frictionless condition to the actual state of natural motions, in which friction plays an important part. Hydrodynamic Theory has, however, been unable to pass from the ideal to the actual, and what is possibly much more serious, from the actual to the ideal. Agreement with experience is the only test by which the success or failure of a mathematical theory applied to nature may be judged, and in this respect hydrodynamics, with some exceptions, notably the treatment of wave motion, has signally failed. It is with the object of elucidating the reasons for this failure that Prof. Oseen has written a book which may be regarded as epoch-making in Hydrodynamic literature.

It is clear that the properties which must be postulated for the "ideal" fluid in order that the resulting entity may have any real meaning for the physicist must be such that the solution of a problem of motion for a viscous fluid should agree, when the coefficient of viscosity becomes zero, with the solution of the corresponding problem made on the assumption that the fluid

is non-viscous. The passage to the limit of vanishing viscosity is the very kernel of the solution of the paradox of the classical theory, and is the central idea which dominates the author's work. Although this passage to the limit still proves elusive, Oseen has succeeded in carrying it out completely in the case of the linear equations obtained by his now well-known method first published eighteen years ago. The conclusions are in direct contradiction to the classical theory and in qualitative agreement with observation. The characteristic result obtained is the partition of the body into a hydrodynamical " front " and " rear." On the front the fluid slips, in the rear it moves with the body, energy disappears at the interface, and the body experiences resistance in a fluid of vanishing viscosity. Further researches by Zeilon extend these results so as to give good quantitative agreement with experiment.

The work under notice has, however, another claim to attention as giving a connected survey of the present state of knowledge of the motion of a viscous liquid and the methods available for attacking the many problems awaiting solution. Such an account is not readily available elsewhere, most of the results being published in scattered memoirs. The author lays stress on the integral equations by which fluid motion may be expressed as opposed to the differential equations by which the problem is usually approached. The only general method at present known of attacking the problem of viscous motion is that of successive approximations, and the use of integral equations obviates the necessity of proving the existence of derivatives of the second order with respect to the time, necessary conditions for the existence of these being of a very complex nature.

The book is divided into three parts. The first part deals with the equations of motion and methods for their solution. The second part is concerned with the solution of special problems with given boundary conditions, treated first by the method due to Stokes, and secondly by that of the author. The third part, which is in many respects the most interesting, discusses the problem of the limiting form of the motion under vanishing viscosity. A bibliography is attached which shows the extent of the author's own researches.

Prof. Oseen is to be congratulated on producing such a clear and readable account of an intricate subject. His book is the first volume to appear of a series of Monographs on Mathematical Subjects, under the direction of Prof. E. Hilb, and it is to be hoped that the succeeding volumes will maintain the high standard set by the first.

L. M. M-T.

Statics and Dynamics of a Particle. By WILLIAM DUNCAN MACMILLAN, M.A., Ph.D. [Pp. xviii + 430, with 184 figures.] (London: McGraw-Hill Publishing Co., Ltd., 1927. Price 25*s*. net.)

THE author states in his preface that this book is designed as a textbook in mechanics for colleges and universities, particularly for those interested in astronomy, physics, or mathematics. An able exposition is given of the behaviour of a particle under Newton's Laws of Motion, but probably the modern physicist would require a rather different treatment. The author has developed the subject from the beginning and in great detail. It is therefore a pity that the First Law of Motion on p. 36 was not more carefully stated. Free use is made of vector methods, which are fully explained, and it is refreshing to find that the poundal has been restored to its proper place as the unit of force in the foot-pound-second system of units. The book is divided into three parts, of which the first deals with the fundamental concepts of mechanics. In Chapter VI the centre of gravity is defined to be the same as the centre of mass. The existence of a centre of gravity implies the assumption that the weights of the particles form a system of parallel

forces. This assumption may be unjustifiable in the case of a very extended body. Part II is concerned with statics and gives a comprehensive account of equilibrium and stability. There is also a valuable chapter on the statics of deformable bodies, the treatment being clear and simple. Part III treats of the dynamics of a particle, and is possibly the best part of the book. A comprehensive survey is given of the various problems that arise, together with an account of Lagrange's method and Hamilton's Principle. The book is well supplied with examples and can be recommended.

L. M. M-T.

The Great Physicists. By I. B. Hart, Ph.D., B.Sc. [Pp. vi + 137.] (London: Methuen & Co. Price 3*s.* 6*d.* net.)

This little book forms one of a series on the great scientists, written in a popular style, and intended to appeal to those who have little or no knowledge of the subject. The book in hand is ably written from this point of view, and should stimulate interest in a branch of history which has inspired a number of new books in the last few years. The author's method is to take some twenty-five famous men, and write short biographies of them, and through them to illustrate the physics of their times. This scheme has the advantage of popular appeal through anecdote and quotation, but it leads to historical errors, in that discoveries are assigned to some of these men in cases where research has shown that lesser-known men had priority. This is noteworthy in connection with Galileo, and the history of the thermometer. However, we must take the book for what it sets out to be—the author disclaims any intention of making it a history of physics—and we must cordially admit that, as an introduction to the serious study of scientific history, the author has succeeded in writing a stimulating book.

E. G. R.

CHEMISTRY

The Higher Coal-tar Hydrocarbons. By Arthur Ernest Everest, D.Sc., Ph.D., F.I.C. [Pp. xiv + 334.] (London: Longmans, Green & Co., 1927. Price 18*s.* net.)

The importance of coal-tar hydrocarbons in the manufacture of dye intermediates is generally realised, and it is perhaps for this reason that so much popular interest is taken in coal-tar products. The number of compounds which have been isolated from coal-tar is so large that it is convenient to treat the different classes separately. Thus the present book deals only with hydrocarbons, excluding naphthalene and anthracene, about which so much information is readily available; the reader thus finds that here the acenaphthene, fluorene and phenanthrene groups are very fully treated. The book opens up a large field of work which has as yet been but partly explored, and in itself is an incentive to further research.

Much detail is given of the isolation and purification of the compounds, together with the determination of their structure and the methods of synthesis employed. The compounds themselves, together with their derivatives, are described very fully with their physical properties. A large number of references is given, and the work should prove of great value both as a textbook and reference book for this branch of the subject.

Incidentally the question of the numbering of these ring systems calls for comment; it seems a pity that one system cannot be agreed upon universally, in order to prevent confusion.

J. N. E. D.

Recent Advances in Organic Chemistry. By ALFRED W. STEWART, D.Sc. Fifth Edition. [Vol. I, pp. xiv + 387. Vol. II, pp. xiv + 382.] (London: Longmans, Green & Co., 1927. Price 21*s*. net each vol.)

THE success of the previous editions of this book has been such that the writer feels that this, the fifth edition, needs but little introduction.

In this edition the book has been published in two volumes, thus more space is available for accounts of new work, and also for the reinsertion, after revision, of parts of the earlier editions which had been crowded out owing to lack of space. Other chapters have also been rewritten and brought up to date. Each volume has its own subject and author index, and may therefore be used separately. The one objection to this procedure is that the cost of the two volumes together is now somewhat high.

The various sections are not merely summaries of recent work, inasmuch as that earlier work is included, thereby making the survey complete, so that the reader has collected for him, and arranged in a very readable way, a large amount of information on various chemical themes. The book is clearly printed, well illustrated, and contains numerous references. It may be recommended with every confidence to the student preparing for his final examination in chemistry.

J. N. E. D.

The Electronic Theory of Valency. By NEVIL VINCENT SIDGWICK, M.A., Sc.D., D.Sc., F.R.S. [Pp. x + 310.] (Oxford: Clarendon Press, 1927. Price 15*s*.)

DR. SIDGWICK's book is one that we can welcome wholeheartedly. The ease of style and the skilful arrangement of the subject-matter makes the book delightful to read. Its value lies not so much in the number of entirely new ideas that it contains, for these are few, but in the way it gives definite shape to ideas that have been vaguely entertained for some years. The author succeeds in being definite without being dogmatic. This is especially valuable in dealing with a subject about which there has been so much vague talk. Care is taken throughout the book not to assign to the electron in its orbit properties which the physicist has found it not to possess, and thus to avoid the reproach that chemists sometimes bring upon themselves.

The book avowedly aims at giving a general account of the principles of valency and molecular constitution founded on the Rutherford-Bohr atom. In effect, however, it shows the impotence of this model in its present stage of development in tackling the problem of molecular stability. The Bohr atom gave for the first time a satisfactory position to the transitional elements of the long periods and to the rare earth metals, and his method of interpreting spectra enables the number of loosely bound electrons (the electrovalency) of these elements to be independently determined, but it does not give us a means of predicting from general principles the number of orbits that form a stable group. The nature of the orbit of an electron which is shared by two nuclei is still a mystery, and so the prospect of predicting the number of shared orbits which make for stability (the co-ordination number) is even more remote.

In this state of affairs it would seem that an advance is only likely to be effected if chemists and physicists co-operate closely. Dr. Sidgwick and other chemists are doing their part by endeavouring to discover from chemical and physico-chemical data what orbital arrangements are in fact most stable. This information should prove invaluable to the physicist in his task of unravelling the tangle of the electronic orbits if only he can be persuaded to regard it sympathetically. The gulf between physics and chemistry has been narrowing rapidly during the last decade. This book should accelerate the process.

R. K. SCHOFIELD.

The Phase Rule and its Applications. By ALEXANDER FINDLAY, M.A., D.Sc., F.I.C. Sixth Edition. [Pp. xv + 326, 165 figures.] (London: Longmans, Green & Co., 1927. Price 10s. 6d. net.)

THE regularity with which new editions of Findlay's *Phase Rule* have been called for must be a source of justifiable satisfaction to the author. It is now twenty-five years since the first edition appeared. The earlier editions, which did not differ greatly one from another, were widely used, and were largely instrumental in familiarising teachers and students in this country with the principles and applications of the phase rule. The situation has altered somewhat, however, in the last few years. There are a number of general textbooks in which space is devoted to a consideration of the phase rule, and there are also works which deal concisely and lucidly with more specialised aspects of the subject. Thus Findlay's *Phase Rule*, which formerly held the field almost unrivalled, has now to meet formidable competition. This, reflected as it is in the increased interval of time between this edition and the last, has doubtless been the reason for the extensive revision which the author has undertaken in preparing the sixth edition.

On the paper wrapper we are told that the sections dealing with systems of one and two components have been entirely rewritten. The preface is more truthful in saying that these chapters have been recast, rearranged, and to a considerable extent rewritten. Less than one-third of the composition is new, but there has been considerable reshuffling. Thus, for instance, a section that formerly concluded Chapter IV and acted as a general summary of the properties of one-component systems, has been transferred bodily to Chapter III to serve as an introduction to a study of these systems. The change is not an unqualified success, and these alterations do not lessen the diffuseness which has always been a weakness of the book.

On the other hand the treatment of a number of equilibria has been rendered more quantitative, and so more precise, by the use of the Clapeyron-Claussius equation. This is a step in the right direction, and considerably enhances the value of the book. At the same time, the use of kilograms per square centimetre as a unit of pressure, rather than the bar or the atmosphere, is unfortunate, and calculated to provoke the ire of physicists. A number of examples of recent applications have been added throughout the book, thus bringing the text more up-to-date. R. K. SCHOFIELD.

The Theory of Emulsions and their Technical Treatment. By W. CLAYTON, D.Sc., F.I.C. Second Edition. [Pp. xii + 283, illustrated.] (London: J. & A. Churchill, 1928. Price 15s. net.)

THIS work on the applications of emulsions is already sufficiently well known to need no fresh introduction to chemists. The first edition was published in 1923 under the title of "The Theory of Emulsions and Emulsification," and it is a sufficient indication of the general appreciation of the work that a new edition is now called for.

The range of treatment is somewhat wider than in the original edition, and covers recent quantitative work on various aspects of emulsions, in particular the measurement of surface and interfacial tension and the determination of the size frequency analysis of emulsion globules.

In addition, special attention has been paid to the technical applications of the subject, such as the manufacture of milk, butter, margarine, ointments, soaps, detergents, etc., the importance of which needs no emphasising, and, as Prof. Donnan points out in his Foreword, Dr. Clayton is exceptionally well qualified to write such an authoritative work on this subject.

A number of excellent illustrations have been included, in particular some admirable micro-photographs of emulsions by Prof. W. Seifriz of Philadelphia.

There are three short appendices, (I) " Theory of Emulsion Stabilization by Solid Particles," by Prof. W. Ramsden; (II) a list of " Emulsifying Agents for preparing technical oil/water Emulsions"; (III) a list of references to the " Separation of Technical Emulsions, particularly crude Oil Fuel Emulsions." In addition there is a bibliography of 24 pages, and an excellent Index, so that the book will undoubtedly continue to occupy the very high place already won by the first edition.

F. A. MASON.

GEOLOGY

A Textbook of Geology. 4th Edition. By PHILIP LAKE, M.A., F.G.S., and R. H. RASTALL, Sc.D., F.G.S. [Pp. xiv + 520, 134 figures, 33 plates.] (London: Ed. Arnold & Co., 1927. Price 21s. net.)

THE well-deserved popularity of " Lake and Rastall " is illustrated by the appearance of a fourth edition, after an interval of five years. The size of the volume has been increased by only 12 pages, but a considerable amount of revision has nevertheless been accomplished. New sections deal with the chemical composition and structure of the earth as a whole, the decomposition of common minerals in weathering, ironstones, salt-domes, and the causes of magmatic differentiation. The chapter on earth movements has been revised in view of modern work on orogenesis and isostasy, and a brief note on theories of continental drift has been introduced. In the stratigraphical part, by Mr. P. Lake, the most extensive changes have been made in the account of the Devonian System. In this edition, however, as in the last, no mention is made of the Moinian and Dalradian formations in the account of the Pre-Cambrian rocks of Scotland, although these formations together cover by far the greater part of Pre-Cambrian Scotland. It is also still stated (p. 389) that most of the lava-flows of the Scottish Lower Carboniferous are andesitic, although on p. 504 the basaltic nature of these eruptions is recognised. They are not, however, connected with the Caledonian folding, as is here implied. We must deprecate the misuse of the term "teschenite" (p. 254) for the group of dolerites containing felspathoids, etc. The term "alkali-dolerite" might have been more appropriately used in its place, especially as the terms "alkali-gabbro" and "alkali-basalt" find places in the nomenclature adopted.

Notwithstanding these small points of criticism and correction, " Lake and Rastall " thoroughly maintains its reputation as the best textbook of geology, of intermediate size, to put into the hands of British students of the science.

G. W. T.

The Structure of the Alps. By L. W. COLLET, D.Sc. [Pp. xii + 289, 63 figures, 12 plates.] (London: Ed. Arnold & Co., 1927. Price, 16s. net.)

WHILE Prof. Collet's book on the structure of the Alps is offered to the student and general reader as less detailed and technical than many other accounts, yet the subject is so complex, and its details so abundant, that we fear his aim is still unrealised. However, Prof. Collet has made an heroic attempt to perform what many geologists will consider as a practical impossibility. Alpine structural geology is of such colossal complexity, and its scale is so imposing, that almost every investigator has had to put forward his own interpretation, based on detailed knowledge of only a part of the ground. In the multitude of counsellors how shall the student find safety? Despite the small size of the book (*vide* the preface) we think that it should have been considered necessary to include at least one general topographical map of the Alps, without which the student is baffled by inability to follow the rapidly-changing scene, and its multitudinous place-names.

Prof. Collet is an exponent of the more extreme views of Alpine structure propounded by Argand and Staub, which represent the Alps as due to the gripping of a great geosynclinal region between Europe and Africa, as between the jaws of a gigantic vice. Africa is supposed to have advanced upon Europe, the folds being piled up in front of the advancing continent wave upon wave, which have one after the other curled in, broken, and the fragments overridden by further waves. The Eastern Alps are believed to be parts of " Africa " which have come to rest upon Europe. The Western Alps, on this theory, represent the borders of the Foreland, and the great overthrust folds which were developed in the geosynclinal region.

The exposition and understanding of these views require the employment of a whole arsenal of recondite technical terms, which are concisely explained in an early chapter of the work. Prof. Collet has been wise in devoting detailed attention to the geology of certain popular Alpine resorts, such as Zermatt, Grindelwald, etc., which are much visited by geologists and geographers ; and his book will be of great value in the hands of workers actually upon the ground described. We are afraid, however, that the ordinary student and general reader will find the work rather difficult reading ; but if they will master the essential structural terms and ideas expounded in the earlier chapters, the volume will repay perusal. The book is dedicated to Mr. E. B. Bailey, of the Geological Survey of Scotland, and Prof. O. T. Jones has contributed an appreciative foreword. G. W. T.

Physico-Chemical Geology. By R. H. Rastall, Sc.D., F.G.S. [Pp. vii + 248, 62 figures.] (London : Ed. Arnold & Co., 1927. Price 15s. net.)

Dr. Rastall's book is an account, admittedly incomplete, of the application of modern physical chemistry to certain geological problems. It supplies a real need of students and other workers in geology, who are handicapped by their lack of early training in those fundamental physico-chemical principles knowledge of which is now so necessary in order to understand modern developments in the science.

The first four chapters deal with the states of matter and the principle of equilibrium ; fusion, solidification, and solution ; isomorphism and solid solution ; polymorphism and inversion. The principles thus elucidated are then applied in turn to igneous rocks in general, mineral formation in igneous rocks, metamorphism, rock-weathering, salt-deposits, ore-deposits, refractories and abrasives, and finally colloids in so far as they appear in geological processes. This scheme is ably carried out, and is of much value in bringing together those geological principles, theories, and facts which are united and explained by physical chemistry.

A statement on p. 90 reads as if the crater of Halemaumau in Hawaii was identical with the giant volcano Mauna Loa. Dr. Rastall is clearly a believer in the alkaline–calc-alkaline division of the igneous rocks, and deprecates even the intrusion of a spilitic series upon this simple scheme of classification. He does not mention that many other transgressions of this sort have been made within recent years. On what appears to be inadequate evidence cancrinite is regarded as a mechanical mixture of soda-mica, calcite, and perhaps other molecules, and not as a true mineral species ; other observers, however (*e.g.* Brögger and Barth), regard it as a secondary or synantectic mineral developed by reaction between nepheline or plagioclase, and calcite.

Norwithstanding a certain compression of statement, Dr. Rastall's book is clear and easy to read. It should be of great use to advanced students, especially as it extends the physico-chemical viewpoint to weathering, metamorphism, ore and salt deposition, refractories, and abrasives, and presents the whole subject of the application of physical chemistry to geological problems in a simple and not too detailed and technical manner.

G. W. T.

BOTANY

Orcharding. By V. R. Gardner, F. C. Bradford, and H. D. Hooker. [Pp. xi + 311, with 136 figures.] (London: McGraw Hill Publishing Co., 1927. Price 15*s*. net.)

Considering the vast importance of fruit-growing to the community throughout the world, it is surprising how difficult it is to find in the literature of the subject information which combines practical considerations with the fundamental principles upon which they are based. From that point of view this book on "orcharding" is particularly enlightening in that it makes a definite attempt to put a scientific explanation to problems of fruit-growing that are matters of common knowledge. For instance, it is universally accepted that apple-trees tend to bear well one year and badly the next, but the reason for this is seldom elucidated, and the illustrated explanation here given is welcome. A preliminary account is given of the growth and nutrition of fruit-trees, special attention being devoted to the formation of fruiting habits and the question of the tree's carbohydrate supply. Later chapters deal with the factors which make for productive orchards, and bear on the questions of pollination, winter injury, orchard sites, propagation, and grafting. The practical man will find much that is helpful on the subjects of training and pruning, aided by the photographs illustrating the points dealt with. Under modern systems of orcharding it is becoming increasingly realised that quality pays, and far greater attention is now given to keeping the trees clean and free from disease by the use of appropriate sprays, both against insect and fungus pests. Furthermore, the best quality fruit loses half its value unless it is attractively packed, and the importance of the small details in finishing the fruit for market is therefore emphasised, together with the best means of transportation and refrigeration where necessary.

As a business, orcharding depends for its success upon the appropriate association of details. The choice of a site, its location and aspect, the selection of varieties suitable for growing and marketing, operating expenses such as spraying, cultivation and manuring, must all receive consideration in relation to the particular local circumstances, and when the fruit is produced the methods of packing, grading, and marketing may make all the difference between success and failure.

W. E. B.

Mycorrhiza. By M. C. Rayner. New Phytologist Reprints No. 15. [Pp. iv + 246, with 64 figures.] (London: Wheldon & Wesley. Price 21*s*. net.)

The work before us, which is a reprint of a series of articles that appeared in the *New Phytologist*, vols. xxv and xxvi, is a welcome addition to the literature on the biology of plants, the more so that the present account includes an extensive bibliography of a rather widely scattered literature.

The subject-matter is divided into three sections, dealing respectively with the early work on the subject (1840–80), the observational period from 1880–1900 (pp. 7–50), and the modern experimental and observational work which, with the bibliography of nearly twenty-one pages, occupies the remainder of the text.

There are few problems connected with the biology of organisms of greater interest than their mutual relations, and the exact status of the partners in the mycorrhizal association has attracted numerous investigators. The authoress has shown that in *Calluna* the entire plant is infected and each seed bears with it the mycelium that ensures infection of the seedling, an obligate condition which probably enables the Heather to obtain soluble nitrogenous substances from the acid soils in which it most commonly grows. In

contrast to this condition some orchids exhibit intermittent infection, whilst some of our native plants, such as the wild arum, exist both in the infected and in the non-infected condition. Such marked differences in the association suggest that neither the view that we are dealing with controlled parasitism or the view that there is mutual benefit is entirely true or false, but that there is probably every degree of dependence, benefit, and toleration between the respective partners.

Even where an advantage from the association with a fungal partner seems clear the nature of the benefit would seem to be very varied. In the Ericaceæ the benefit appears to be nutritional, but in *Gastrodia*, *Liparis*, and perhaps also in some Bryophyta, the parasite serves to stimulate the production of sexual organs which may be analogous to the effect of root-pruning on fruit-trees or adverse conditions on many algæ. That is, the advantage may be the indirect outcome of a check to the plant's activity. Such instances sufficiently illustrate how fine-drawn may be the distinction between mutualism and parasitism.

The method of presentation is both simple and interesting, though in parts a trifle diffuse in style.

E. J. S.

Kostychev's Plant Respiration. Authorised Edition in English with editorial notes. By PROF. S. KOSTYCHEV. Translated and edited by DR. C. J. LYON. [Pp. xii + 163, with 11 text figures.] (Philadelphia: P. Blakiston's Son & Co. Price 2.50 dollars.)

THIS very readable account of the respiratory processes in plants will be found very useful by students unfamiliar with the extensive and scattered literature dealing with modern views as to the chemical changes involved in the respiration of carbohydrates.

Not unnaturally the greater part of the text is occupied with that aspect of the subject with which the author's researches have been especially associated, namely, the relation between anaerobic and aerobic respiration. The old view that anaerobic or intramolecular respiration represents an adaptation that enables plants to survive brief periods of anaerobic conditions, as also the view that it is a purely pathological phenomenon, has given place to the recognition of intramolecular respiration as a process that is normal in character and the precursor of aerobic respiration. The latter is regarded as utilising the intermediate products of alcoholic fermentation, but in the case of yeast these intermediate products are in excess of the requirement for aerobic respiration and in large part become converted into ethyl-alcohol and CO_2. In the normal higher plant the intermediate products are held to be utilised as soon as formed, so that only under anaerobic conditions is the alcoholic by-product produced.

So far the picture presented in these pages is clear, but from the insufficiency of our present knowledge the actual chain of the chemical processes involved is only very incompletely visualised. The views of Palladin, in which hydrogen acceptors are responsible for the removal of hydrogen both from the respiratory material and the accompanying water with the evolution of CO_2, may prove the basis of a clearer explanation. Palladin's hypothesis is not without important experimental support, and if true implies that the rôle of the atmospheric oxygen is the reactivation of the hydrogen acceptors by removal of hydrogen combined with them.

The numerous bibliographical references are a useful feature, but one would have welcomed a fuller treatment of the work of other investigators.

E. J. S.

ZOOLOGY

Sagitta. By S. T. Burfield, B.A., M.Sc. (Liverpool Marine Biological Committee.) L.M.B.C. Memoirs, xxviii. [Pp. viii + 104, with 12 plates.] (Liverpool: at the University Press; London: Hodder & Stoughton. October 1927. Price 6*s*. 6*d*.)

In publishing his monograph on Sagitta, Mr. Burfield has supplied a long-felt want. The L.M.B.C. Memoirs are well known, the present one being the 28th of the series, all of which deal with common animals easily obtained, each describing in detail one individual species. The object of this one, *Sagitta bipunctata*, is the commonest species of Sagitta occurring off our coast and a constant member of the plankton.

After discussing the historical aspect, classification, occurrence and habits of Sagitta, the affinities of the Chætognatha and the methods used in his work, the author proceeds to a description of the animal. This is done in a very clear manner, and should be a great help to all students, for it entails much arduous and intricate work. In this small worm, little more than a centimetre in length, there are thirty-three recognisable muscles, eighteen of which belong to the head, and a function is known or suggested for each of these. Special attention is paid to the eyes and elaborate diagrams of their structure are given, the general conclusion being that there is "no image perception . . . but that the organs no doubt serve to distinguish light intensity, and there will be a limited sense of directional perception."

Development takes place in the sea, the egg pushing its way out of the oviduct to the exterior as soon as the spermatozoon has entered. Only about two days elapse before the young is hatched, as a very simple larva without any food canal, the latter being functional in about eight days. It is not stated how long it takes for the worm to become mature.

A short chapter is reserved for parasites, for which Sagitta is a favourite host, cestodes, trematodes, and nematodes all occurring in it at times, sometimes very frequently. A particularly full and useful bibliography is given, and the memoir is illustrated by some clear diagrams and twelve plates.

M. V. Lebour.

By Reginald Crundall Punnett, F.R.S. Seventh Edition. [Pp. xiv + 236, with 63 figures.] (London: Macmillan & Co. Price 8*s*. 6*d*. net.)

This is a very useful work, and gives very clearly an account of the Mendelian theory of inheritance. The simplicity of the Mendelism of twenty years ago has vanished with the increase in genetic experimental work, and a book of this type must be constantly brought up to date. Prof. Punnett explains the Presence and Absence theory, and accepts the existence of inhibitor factors to explain incomplete dominance. There is an interesting chapter on Sex-Linkage, and in the chapter on Sex and Intersex some account is given of the experiments of Goldschmidt on the Gipsy moth, and of the work of Crew and Witschi. In Chapter XIII the Chromosome Theory is explained, and the relation between chromosomes and factors is illustrated by the case of Drosophila; acknowledgment is made of the ingenious hypotheses of American observers to explain the difference in the offspring of grey normal brother and sister when crossed with the double recessive black vestigial male or female, but it is clear that Prof. Punnett considers more experimental work is required in this branch of genetics, though it has received support on the cytological side from the brilliant work of Prof. Morgan and others.

Opinions on Variation and Evolution differ widely, but this book is not concerned with those views except indirectly; it is concerned to state the theories that arise from a Mendelian interpretation of the phenomenon, but

at the same time it seems a pity to condemn entirely to the background other views on Variation and Evolution.

The final chapter deals with Man, and the difficulty of applying Mendelism to many characters in man is pointed out; it has been applied to some deformities and diseases, and Prof. Punnett thinks that the clue to the inheritance of diseases, restricted, not absolutely but largely to one sex, such as colour blindness, is supplied by a study of Drosophila; but we must confess that we have found exceptions to the rules stated, since in one pedigree three colour-blind women are the daughters of a father who had been thoroughly tested, and was not colour blind, and in other pedigrees a colour-blind woman had sons with normal colour vision.

Biologists will differ as to the value of this or that theory of inheritance, of this or that experiment and the deductions from it, but an understanding of the basis of each theory is essential, and this book admirably supplies that need for the Mendelian theory of inheritance.

β₈.

MEDICINE

Mosquito Reduction and Malaria Prevention. By J. A. Crawford and B. S. Chalam. [Pp. 1–107, with 17 illustrations.] (Wesleyan Mission Press, Mysore City, 1927.) Second Edition. (Oxford: University Press. Price 4*s.* 6*d.*)

The authors of this little book have recognised the fact that success in anti-malaria and anti-mosquito work largely depends on enlightened co-operation between all members of the control staffs and an intelligent interest on the part of the local community. Their book presents the reader with a concise, non-technical account of the various matters connected with malaria prevention and mosquito control.

A short summary of our knowledge of malaria and its causative organisms introduces the reader to sections devoted to descriptions of the "Life History of Mosquitoes," "The Habits of Mosquitoes," and "Preventive Work," and eight Appendices, the most notable of which is Appendix VII, where a "Table for Identifying the Females of the *Anophelini* of India" is included.

The book is admittedly of an elementary character and for those for whom it has been designed to appeal it will, no doubt, serve the purpose of stimulating interest in anti-malaria and anti-mosquito measures. Here and there in the text, however, we have the feeling that attempts at simplification have gone too far and that the enthusiastic neophyte may tend towards that little knowledge which at times is dangerous. For instance, in discussing the classification of mosquitoes the following statement is made: "The Anophelini have only one genus *Anopheles*: while the *Culicini* have at least two—*Culex* and *Aëdes*. The latter is usually spoken of as the genus *Stegomyia*." Mosquito work would indeed be vastly simpler than it is were the latter part of this sentence true, for actually there are over thirty different genera of the *Culicini*, and *Stegomyia* is but one of the several subgenera of the single genus *Aëdes*. On page 20 short descriptions of the characters of the *Anophelini* and the genera *Culex* and *Aëdes* are given, but to attempt to differentiate the larvæ of *Culex* from those of *Aëdes* with any certainty on the characters assigned would be quite impossible. With the exception of the *Anophelini* no attempt is made to indicate how the worker is to identify the species important from the standpoint of public health. On page ii *Culex fatigans* and *Aëdes argenteus* are cited as important species, yet the reader is left in the dark as far as the recognition of these species is concerned. On the other hand, the authors give much more information concerning the Indian species of *Anopheles*, and include a useful table giving the distribution and types of breeding-places.

Under the sections "The Habits of Mosquitoes" and "Preventive

Measures" there are many observations which are interesting and of practical value. The suggestion is made that no building in India should have rain-gutters, but instead that the water should run directly off the roof to fall on a cement ledge on the ground-level, and that the outside edge of this ledge should have a channel to lead the water away. There is much to be said for this suggestion, and we have seen this system in operation in certain parts of Africa. Nevertheless, it is open to abuse, and if the drainage channels are not periodically cleared mosquitoes will as readily breed there as in the gutters themselves.

To laymen and untrained workers the book admirably presents an essential outline of the scope, interests, and methods of anti-malaria and anti-mosquito measures.

MALCOLM E. MACGREGOR.

MISCELLANEOUS

Conditioned Reflexes. An Investigation of the Physiological Activity of the Cerebral Cortex. By I. P. PAVLOV. For. mem. Royal Society. Translated and Edited by G. V. ANREP, M.D., D.Sc., Lecturer in Physiology in the University of Cambridge. [Pp. xv + 430, with 18 illustrations.] (Oxford, at the University Press, 1927.) Price 28*s*. net.

THE appearance of this book is to be regarded as a happy event and one which will make history. Happy because of the good fortune which enabled the Royal Society to secure the help, as translator, of Dr. G. V. Anrep, himself a pupil of Professor Pavlov, and therefore as familiar with the subject-matter itself as with both the languages concerned in the original and in the present translation which so speedily ensued. Epoch-making because it opens up a new method of experimental inquiry, the consequences of which it is at present impossible to foretell.

The method of investigation involves the study of reflexes acquired under experimentally controlled conditions. The particular reflex chosen for the series of problems under investigation was the secretion of saliva by dogs. This normally occurs when food is given or exhibited to the animal, but if just prior to or accompanying the feeding some previously neutral stimulus be given, *e.g.* the sounding of a particular tone of a tuning-fork, then it is found that after the repetition of such a "reinforced" stimulus an appropriate number of times, the formerly neutral stimulus has itself acquired (even when not reinforced) the property of eliciting a flow of saliva—in other words it has become a *conditioned* stimulus. With monumental patience and consummate skill, Pavlov and his pupils have thoroughly analysed this type of reflex and shown what possibilities it possesses as an instrument of research. For instance, to revert to the tuning-fork note, it was found that other tuning-fork notes could be discriminated from the positive conditioned one, and this provides us with an objective method of studying the animal's powers of discrimination; it was shown that dogs can discriminate to one-eighth of a tone. Other stimuli could be investigated by analogous methods.

Most interesting chapters are those dealing with the phenomena of inhibition and sleep, which are shown to be related phenomena. Strong inhibitory states develop under certain conditions of experiment and it was often found that at these times the animal fell into a profound sleep.

The material dealt with in this book has been accumulated during many years of research work and is too intricate and important to be reviewed adequately. All physiologists and psychologists will read the book with great interest and feel very grateful for the appearance in English of such a complete account of the subject. The difficulties of translation were no doubt considerably increased by the necessity for rendering into English for the first time the whole terminology of a new subject.

C. LOVATT EVANS.

Measurement of Air Flow. By E. Ower. [Pp. vii + 199, with 73 illustrations.] (London: Chapman & Hall. Price 15s. net.)

There is an insistent demand for a book, written in English, which shall deal adequately with apparatus and methods available for determining the velocities of streams of air or other gases. Such determinations are of extreme value in industrial practice, but the methods generally employed in the routine of such measurements make it certain that the results achieved are practically valueless in the majority of cases. There is nothing inherently difficult about the determination of the velocity of a gas stream. The gas industry determines the volume of its huge supply through mains, services, etc., with an error certainly not exceeding 2 per cent. Whence the difficulty then? We think it arises from the fact that the gas most usually metered industrially is air, and air has no monetary value. Consequently it is generally considered hardly worth while attempting an accurate determination. The consequence has been that more than one industrial concern, employing only an impossible form of Pitot tube or a form of vane anemometer that might have been labelled "found in the trenches" has achieved a rocketing reputation for being advanced and scientific. Apparatus adequate for the work would be regarded as savouring of the transcendental and metempiric.

The author offers the book as a first step to meet the needs of students and practical engineers. It may be of use to students; it will be of little use to engineers in their practical work. The book is not of a sufficiently practical character for that purpose. We find forty-five pages devoted to the design of Pitot tubes and the theory of the vane anemometer. Space is found (p. 52) for proving that the mean velocity of flow through a pipe is equal to the mean of the mean velocities through its parts. Such niceties of discussion make no appeal to the practical engineer. He wants to know merely what apparatus he should use, how he should use it, and would like tables to be supplied so that he is not troubled with calculations. This book doesn't meet his needs.

It is less practical in its character than Pannell's *Fluid Velocity and Pressure*; it is far behind Litinsky's *Messung Grosser Gasmengen*. The final table given in this last work is the sort of thing that appeals to the practical engineer.

In addition to matter to which reference has already been made, chapters in the present work are devoted to: the flow of air in pipes, plate orifice, venturi, and shaped nozzle anemometers, manometers, and electrical anemometers. The book is well printed on good paper; the illustrations are good; there are an adequate index and inadequate bibliography. The price is not unreasonable. We recommend the book to students interested principally in theoretical aspects of the subject. The practitioner will learn German and get Litinsky's book until an English version is available.

J. S. G. Thomas.

The Interaction of Pure Scientific Research and Electrical Engineering Practice. By J. A. Fleming, M.A., D.Sc., F.R.S. [Pp. x + 235, with 64 illustrations.] (London: Constable & Co., Ltd., 1927. Price 15s. net.)

Prof. Fleming enjoys a high reputation as an accomplished lecturer, and we naturally welcome the appearance of this book, in which is reproduced in a very convenient form a course of eight advanced lectures delivered before the University of London in October and November 1926, at the Institution of Electrical Engineers. As its title indicates, the book is designed to introduce to the pure scientist the advances which his more practical brethren have

made possible by the discovery of new materials with important physical properties and of new appliances, and to place before the electrical engineer an outline of the theoretical considerations which have guided his more academic brethren in their labours. This does not mean that the book will not appeal to a wider circle of readers, for it can certainly be read and appreciated by all who are interested in scientific thought and scientific developments, even if every reader will not agree that Government technical work is generally behind the times and that the men in control are usually not sufficiently progressive or eager to break new ground.

In his first lecture Prof. Fleming deals with electrical insulation and conduction, describing researches on dielectric coefficients, dielectric strengths and breakdown voltages, insulation resistances and improvements in the mechanical qualities of insulators. He also gives a brief outline of the electron theory of conduction in metals and discusses the electrical properties of pure metals and alloys. In the next lecture, on ferromagnetism, he includes a description of the applications of hysteretic repulsion which was discovered by Mordey. A very interesting summary of the history of thermionics is given in the third chapter, together with details of the manufacture and applications of thermionic valves. Glow and arc discharges and their applications are described in the fourth lecture. The fifth lecture, on telephony, is perhaps the one which emphasises in the most convincing manner the close connection between pure and applied scientific research, for here we have an outline of the history of the telephone and we are given an idea of the immense amount of pure scientific research on the dynamics of speech sounds and the analysis of them and the effect of all parts of the telephone appliances on the wave form of the transmitted currents and the air vibrations reproduced. We have, too, a description of the revolutionising effect of the discovery of permalloy on transatlantic telegraphy, the use of the thermionic amplifier as a telephone relay which is so perfect that all other forms are obsolete, the use of wave filters, and wireless telephony. Of great practical interest is the sixth lecture on surges and pressure rises in electric transmission lines, produced by atmospheric disturbances, by failure of insulation or by sudden breaks in the circuit, and the construction of apparatus, such as the Ferranti Grant surge absorber, to render them harmless. The remaining lectures on electrochemistry and electrical measurement are equally interesting.

The book serves a very useful purpose, and if the remarks on Bohr's theory of spectra do not cause the faces of the pure scientists to be brightened with as much joy and interest as the faces of the engineers in Prof. Fleming's audience (particularly when he wrote the equations for the hydrogen atom on the blackboard), yet they will obtain much help and inspiration from its perusal.

L. F. B.

Excavations in Malta. (Percy Sladen Memorial Fund Expedition.) Parts I and II. By M. A. Murray. With Chapters by G. Caton Thompson. [Pp. 49, with 21 plates, and pp. 43, with 33 plates.] (London: Bernard Quaritch, 1923, 1925. Price 7*s*. 6*d*. per vol.)

Outside a limited number of specialists, the interest of Malta as a field of prehistoric research is unrealised. This is perhaps no great misfortune. If the antiquities of the island had attracted attention more widely, the agricultural methods of the islanders, which are, at least, in part preservative of their monumental areas, might have been superseded by the more subversive ways of the hunter of antiques for the tourist trade.

It is in the natural course of things that Miss Murray should have dedicated

her book to Dr. Zammit. No one can possibly speak of Maltese archæology without reference to his great work. But although he has done much himself, and although other distinguished archæologists have worked at intervals in Malta, the more the facts are brought to light and sifted, the less susceptible of anything like an intelligible explanation do they appear.

The expeditions of which Miss Murray was the leader in the successive seasons from 1921 to 1924, which are described in these two volumes, had the elucidation of certain well-defined problems as their objective. The excavation of the cave of Ghar Dalam was a hunt for Palæolithic, or, more specifically, Mousterian Man, while Miss Murray's work, of which the exploration of the megalithic site of Borg en Nadur became the main object, it was hoped would throw light upon the great problem of Maltese archæology, the character and relation, if any, of the Neolithic and Bronze Age civilisations of the island. In both cases the results were negative, though none the less valuable for that.

In 1917 Mr. Despott discovered in the cave of Ghar Dalam two teeth which exhibited the characteristic form of the teeth of Neanderthal Man. Apart from the intrinsic interest of any discovery relating to Neanderthal Man, that Neanderthal teeth should be found in Malta is remarkable in view of the absence of anything like Mousterian implements and other traces of palæolithic man in the island. Miss Caton Thompson's excavations during two seasons failed to produce any further evidence on the point. Her work, however, was by no means unproductive, for, apart from its value for the guidance of future explorers, it brought to light valuable data bearing upon the prehistoric fauna, including some new species, and has gone some considerable way towards the elucidation of the remarkable problem of the apparent association of remains of the hippopotamus with relics of neolithic man.

At Borg en Nadur, Miss Murray was engaged on the excavation of an apsidal temple, and of a megalithic wall. The area of the site is considerable; but it was disappointing to find that the whole had been subjected to very great disturbance. Careful records of stratification were kept, but proved on this account to be valueless. Remains of the Bronze and Neolithic Ages were found, comparable to those from other sites, and a number of cult objects. Beyond attributing a ritual significance to them, it was not found possible to indicate their use with any certainty, except, of course, in the case of the bethel-stones, with their characteristic pointed tops. Although the pottery was in a very fragmentary condition, characteristic types have been reconstructed. In her account of these finds, Miss Murray has wisely added descriptions of pottery from other sites. In the event, the pottery will probably prove the most important contribution to our knowledge of Maltese archæology which has been the outcome of Miss Murray's labours.

In their general results, Miss Murray's expeditions restated the problem of prehistoric Malta rather than elucidated it to any degree. Here is a neolithic civilisation which shows no sign of contact with a Bronze Age and a Bronze Age which shows no sign of development, notwithstanding localised differences, and lasts for a very short time only. The suggestion that the island was merely a port of call for traders hardly seems entirely adequate, though the modern position of Malta as a naval base and garrison may not be without significance as a pointer for early history.

It is impossible to conclude without a tribute to the meticulous care with which Miss Murray has recorded and figured her results and to the fortitude and endurance of her colleagues and herself in carrying on this arduous work in the climatic conditions of a Maltese summer. And a tribute must also be paid to the enlightened generosity of the Maltese Government in helping on the work.

E. N. FALLAIZE.

Meteorology. By DAVID BRUNT, M.A., B.Sc. [Pp. iii + 19, with illustrations.] (London: at the Oxford University Press, 1928. Price 2*s.* 6*d.* net.)

THIS small work gives an account of Meteorology as a branch of Physics. It can hardly be called a popular manual, for, although the treatment is non-mathematical, anyone with no knowledge whatever of physics could obtain only a rough general idea of the argument in some of the harder chapters. The manner of tackling the problem of presenting, in a very small space, the essential features of a big subject like meteorology, appears in every way commendable, and the same thoughtful care that must have been devoted to the general structure of the work is evident in the smallest details. The absorption of the sun's heat by the earth's atmosphere, to which Chapter V is devoted, is a subject usually shirked, or at least scamped, in works of much greater size than this. Perhaps the reason is that the authors have seldom, like Mr. Brunt, served an apprenticeship in astronomy before turning to meteorology.

This work should find its greatest sphere of utility in the advanced classes of the best schools, where there should be a science master competent to help his classes through most of the harder parts, but this is not meant to imply that any intelligent, albeit unscientific, reader could not learn at least as much meteorology from these pages as from any other handbook of the same size. It is the most thoughtful, and easily the best, small manual that the reviewer has so far encountered, and he has found it a pleasure to read.

E. V. NEWNHAM.

The Lindley Library Catalogue of Books, Pamphlets, Manuscripts, and Drawings. [Pp. viii + 488.] (London: Royal Horticultural Society, 1927. Price 21*s.* net.)

THIS convenient 8vo volume contains the carefully prepared and well-printed catalogue of the Royal Horticultural Society's Library, of which the original nucleus was the private library of Prof. John Lindley, acquired after his death some sixty years ago. Building on this foundation the Society is now the possessor of one of the finest horticultural libraries in the world. The number of entries is approximately 16,000, and the issue of this catalogue an event of some importance. Gaps no doubt there are, but it is not easy to detect them. Two omissions may be mentioned. There does not appear to be an historical collection of examples of growers' and seedsmen's lists dating as far back as such things go; those included belong to comparatively recent times. Guide-books to foreign botanic gardens and similar establishments are not easy to find, and it would have been a great advantage if some collective indication could have been given. As with most libraries, a preliminary weeding out of a certain number of rather irrelevant pamphlets and books would have emphasised the high excellence of the collection as a whole.

F. W. O.

"Kingship." By A. M. HOGART. [Pp. x + 250.] (London: Oxford University Press, 1927. Price 12*s.* 6*d.* net.)

IN this work the author attempts to apply to customs and beliefs the methods that have been so successful in the study of language. By a careful analysis of the main features of those forms of religion in which kingship and divinity have appeared to be merged, he tries to trace these to a common origin, and to show how widespread has been the influence of such beliefs, and how they continue to permeate the thoughts and customs even of modern civilisation.

The author has been led to this study by a long association with the south Sea Islanders, from whom he draws a good deal of his material, and this, together with much drawn from India and elsewhere, is subjected to a process of dissection after the manner of the anatomist.

He first makes out his case that the earliest known religion is a belief in the divinity of kings, or at least that in the earliest records known "man appears to us worshipping gods and their earthly representatives, namely kings." This statement is substantiated by a variety of evidence. The tendency to identify the enemies of the king with the enemies of the deity, and to associate kingship with victory, is shown to have survived to the present day, as for example in our own National Anthem, and the ascription of miraculous powers to kings has also persisted till comparatively recent times. The idea that immortality could be attained by partaking of some magic potion can be traced in the kava of the Fijian, the ambrosia of the Greeks, and the soma of Vedic vitual, to the much more spiritualised conception of the sacred elements in the Christian Communion.

The analytical method is applied in detail to the coronation ceremony, the component parts of which are sought for in the customs of a number of races. The close similarity of many of these components to rites associated with marriage ceremonies leads the author to the conclusion that the ordinary marriage ritual is but the matrimonial part of the coronation ceremony detached and simplified for the use of the common people, so that "after all the revolutions in thought that have shaken Europe, the marriage ceremony still retains the impress of its royal origin, even to the original theory on which the whole ritual is based." A similar analytical method is applied to the ordination of priests and to ceremonies of initiation, and a theory of their evolution as collaterals of the coronation ceremony is propounded.

Finally, we are led to a somewhat novel explanation of the processes by which man "discovered" that he possessed an immortal soul. The Fijians believe this to be acquired at some period after birth, and the author suggests that it was through the spreading of ceremonies of initiation or other forms of consecration from kings downwards to all classes of society that the possession of a soul gradually came to be regarded as not merely the divine attribute of the monarch, chief, or priest, but an attribute common to all mankind. Thus we end with the conception that the institution of a divine kingship, which emerges with the dawn of human history, became gradually spiritualised and diffused and thus played an important part in the evolution of religions.

This book breaks new ground and contains much that is suggestive, and the conclusions which are arrived at are presented with praiseworthy caution.

β_{s}.

The Antiquity of Man in East Anglia. By J. Reid Moir. [Pp. xiii + 172, with 25 plates and 74 text figures.] (Cambridge University Press, 1927. Price 15s. net.)

It is suggested in the Preface to this book that "There is, perhaps, no other part of the world richer in the remains of our remote ancestors than that of Suffolk and Norfolk. . . ." But such a contention can only be supposed true if a particular meaning is attributed to the phrase "our remote ancestors." There is, of course, not a shred of evidence to prove that the earliest human beings who inhabited East Anglia stood in the direct line of descent of modern Englishmen or any other race of modern man. Our forerunners they certainly were, but it is unlikely that they were our true ancestors. The word "remote," too, must be taken to connote the very earliest periods which have left evidences of human activities recognised as such by modern archæologists. But if it is meant to apply to the whole of the prehistoric

era, then it may well be questioned whether East Anglia has such abundant and diversified remains as the Dordogne region of France, or as some other parts of Europe. There are still some authorities who refuse to admit that the alleged relics of Tertiary man are authentic, though their number has been greatly reduced within the past few years. But, if their view be the correct one, then Norfolk and Suffolk can only claim to have furnished minor, though interesting, traces of early man.

Supposing that pre-Pleistocene man lived in England, and that he left somewhere enduring proof of his existence, then that proof is most likely to be found in the late Tertiary deposits of East Anglia. Mr. Reid Moir is better fitted to speak of the earliest accredited remains in that field than anyone else. All interested in these questions will welcome this summary of the evidence he and others have collected, and of the conclusions he draws from them. The chapters devoted to pre-palæolithic man make up only a third of the whole, and they are all too short. The remaining ones deal with the archæology of later periods down to Anglo-Saxon times. The style in which they are written is calculated to attract the reader having no previous acquaintance with these matters, and those who have will appreciate its freedom from dogmatism and textbook formality.

The first daring suggestion that a certain kind of flints found in England had been intentionally chipped by man in the Pliocene, or in an earlier, period was made by Benjamin Harrison. The Kentish gravels in which his Eoliths were found are of an uncertain date, but certainly of a high antiquity. The correlation of a similar, but not identical, type of implement with strata of a definite geological age was made in East Anglia by the writer of this book. That marked a great advance, and the theories which had previously seemed hazardous to almost all became widely accepted. The fact that his own type is distinct from that found earlier is stressed by Mr. Reid Moir, and he speaks of " Pre-Palæolithic " and " Pre-Chellean " implements to distinguish them from the true Eoliths. The latter are also said to be found in Suffolk. The most complete vindication of these theories would be furnished if " Pre-Palæoliths " were to be found associated with a fragment of human bone. Not only has no such find been made yet, but few of these flints seem to have been found with bones of any animal. But the workers in this field nurse the ever-present hope of making a discovery which will rival that of the Piltdown remains. The rarity of bone is also a characteristic of the Palæolithic strata of East Anglia, and, owing to that fact, our knowledge of some aspects of the life of prehistoric man in that area is always likely to be meagre compared with that derived from localities where caves and rock-shelters have protected many more or less perishable remains. A quartzite pebble picked up on the surface of the ground at Nayland in Suffolk bears the engraved outline of a goat, and it is presumed to be of Upper Palæolithic date. Engraved flints from Grime's Graves, and a shaped piece of chalk from another locality—all of uncertain date—are the only other possible examples of Old Stone Age art in East Anglia. Like the earlier periods, the later ones must be known almost entirely from flints alone. With due reserve, however, Mr. Reid Moir advances the suggestion that two other kinds of relics have been found in Palæolithic deposits in his terrain. In a brickfield at Ipswich he discovered fragments of " a very rough and hitherto unknown type of pottery " which is believed to be contemporary with remains of the mammoth found at the same place. The other unusual, and almost unique, find had only been described previously in *Nature* (January 9, 1926). It consisted of two oak planks supported by birch stakes and flints disclosed at 6 feet below the present land surface. The wood was blackened with age and it had evidently been preserved owing to the fact that the ground round had become water-logged. These interesting fragments are thought to be the remains of a wind-screen, and their dis-

coverer does not assign them to any definite period, though he is evidently convinced of their great antiquity. He was led to experiment in the shaping of wood with flint implements, and found that planks and stakes could be fashioned, and holes could be bored, with considerable success. Many different forms of tools were found to be well adapted for that purpose. A piece of wood from the Cromer Forest Bed at Maundesley in Norfolk is also said to have been humanly shaped. The short survey of the Neolithic period deals almost entirely with the excavations at Grime's Graves. Long burrows are unknown in East Anglia, in spite of the fact that implements of that age are numerous. Bronze Age tumuli and artefacts have been found in considerable numbers.

This book is excellently illustrated with plates and text-figures. The frontispiece is a photograph of a large hand-axe from the Cromer Forest Bed, and it carries more conviction than a drawing would, however good. Fig. 5 shows the familiar section of the North and South Downs, with the original surface of the chalk dome indicated by a broken line. On the very summit of that dome a point is marked as "Former position of Eolithic land surface," but, surely, that is too much "in the air." The skull which is figured in Plate XXII should be described as Neolithic or Early Bronze Age, as in the text opposite. The suggestion made on p. 44, that the "Cromerians" had larger hands than ours because of the large size of their implements, seems to be rather hazardous. Many of the references are given without dates.

The wider aspects of these researches are dealt with in the last chapter. The moral is drawn that the cradle of early man may be sought for in our own fields with greater hope of success than in distant and unexplored Asia. Digging is expensive, and the ground as yet unexplored will doubtless yield more important evidences of man's origin and ascent than any yet found. It is to be hoped that Mr. Reid Moir will himself be able to continue and extend the work which has hitherto depended so largely on his own personal efforts.

β_2.

Spectroscopy. By E. C. C. Baly, F.R.S. Third Edition, Vol. III. [Pp. viii + 532, with 60 illustrations and 6 plates.] (London: Longmans, Green & Co., 1927. Price 22*s*. 6*d*.)

This is the penultimate volume of the four which will comprise the new "Baly." The first two were expanded versions of sections of the original book, but this is almost entirely new matter, since it is concerned with spectroscopic applications of the quantum theory which were only in their infancy when the last edition was published (1918). The first chapter, which runs to over 300 pages, deals with line spectra, and the remaining three with the Zeeman and Stark effects and emission band spectra.

The subject is approached from the historical standpoint, a method which, as the author realises, may not meet with general approval; but criticism is disarmed by the ability and enthusiasm he displays. As he remarks, there does not appear to have been any previous attempt to tell the whole story of the development of our knowledge of line spectra. Nevertheless, he would have done well to distinguish more carefully the results and methods of presentation which are of purely historical interest from those of permanent significance. The student is likely to find more hindrance than help, for example, in the triplet scheme on p. 17, as no hint is given of the inaccuracies it contains.

The presentation throughout is suited to the needs of the practical spectroscopist. The theoretical aspect of the subject is by no means neglected, but much more space is devoted to such questions as multiplicities and Zeeman resolutions. Most writers on these matters achieve a certain obscurity

by omission of numerical illustrations, but no such charge can be laid at Prof. Baly's door. He is so generous in this respect that the continuity of his argument frequently suffers by lengthy interpolations of wave-number tables. The very detailed and valuable account of intensity measurements deserves to be mentioned, and the chapter on band spectra is a useful survey of an extensive subject. There are one or two erroneous statements, as, for instance, the attribution of the cyanogen bands to nitrogen (an echo of an almost forgotten controversy), and of W. Wilson's work to H.A.

The production of the book is excellent and one is grateful for the two comprehensive indexes. In place of the customary, and in this case well-earned congratulations to the author we would venture to remark that of all the debts which physicists owe to chemistry the chief is probably that unique and invaluable compound known as E.C.C.B. W. E. CURTIS.

Creative Education in School, College, University, and Museum. By HENRY FAIRFIELD OSBORN, Sc.D., D.Sc., LL.D., etc. [Pp. xiv + 360, with frontispiece and 14 portraits.] (New York and London: Charles Scribner's Sons, 1927. Price 10s. 6d. net.)

WE have grown accustomed to the shameless puffs on the dust-covers of "best sellers," but cannot reconcile ourselves to the extension of the practice to books of a more serious character. When, therefore, the publisher assures us that "no personality in the whole world of science is more potent, more essentially creative as applied to present problems of education, than is Prof. Osborn," our first reaction is one of unfavourable prejudice rather than of pleasurable anticipation. However, the duty of the reviewer is to review, and, having overcome an initial repugnance for (the publisher again) this "clarifying and valuable compendium," we left the cover for the contents. On the whole, the book is well worth reading, though, owing to the large number of local allusions, it will appeal more to American than to English readers. It is composed of a selection of educational addresses given by Prof. Osborn during the last quarter of a century, together with an introduction and a concluding chapter which serve to enclose the somewhat disjointed matter between them.

The author's eminence as a biologist, and his well-known enthusiasm for the theory of evolution, would lead us to expect vigorous and characteristic views from him on the subject of education. Our hopes are realized. "Many years ago," he says, "I set out to solve the so-called mystery of education; I found it to be soluble. It culminates in one great final object, which was learned by the leaders of Palæolithic man 200,000 years ago, and is equally available to-day for the men of dirigible balloons and aeroplanes. It is, to follow the law of evolution, to follow in mind culture the principle of addition and accretion characteristic of all living things; namely, to develop the highest degree of productive power, centrifugal force, original, creative, individual efficiency." The chief aim of education should be to increase the output of original thought and ideas, whereas, according to the author, 95 per cent. of modern education is "the learning of facts and the memory thereof."

Prof. Osborn has some very pertinent remarks to make on the subject of free education. He believes that American eagerness for education makes it far too easy of access for the average parent as well as for the average child. Nine parents out of ten get education for their children with little or no effort on their part because there is no direct tax for education. The cost is borne almost entirely by a relatively small number of tax-payers, and the result is that the great benefit of free State education is not properly appreciated. We have heard similar opinions expressed very strongly on this side of the Atlantic, and there is much to be said in their support.

Finally, it may be noted that the woman teacher falls under Prof. Osborn's disapproval. "Admirable as are many of our women teachers, they have neither the natural mastery nor the natural leadership in the education of boys; even in the education of girls, especially in the upper grades, the virile quality is lacking in them."

E. J. H.

The Ley Hunter's Manual: a Guide to Early Marks. By ALFRED WATKINS [Pp. 90, with 56 figures.] (Hereford: Watkins Meter Co., 1927. Price 2s. net.)

THE object of this booklet, a supplement to *The Old Straight Track*, is to "tell" everyone who does not know or does not agree that such ancient objects as tumuli, mottes or mounds, megaliths or "mark-stones," were put there, not for the purposes usually supposed, but as *leyes* or beacons to enable travellers to make their way in a straight line from point to point. To support this thesis, the writer plays ducks and drakes with the facts of archæology, philology, and history, not to mention the principles of logic. He systematically argues in a circle. Certain objects lie along a straight line: therefore they must have been put there to work out such a line. It never occurs to him that the road or track was there before the objects were placed along it, as we still place buildings along a road, for reasons that are obvious to most intelligent persons. On Mendip, the author would find barrows alined upon the site of a prehistoric way. In the Aran Isles the natives still raise their tomb-stones and memorial crosses beside the one main road traversing the largest island. Mr. Watkins "tells" us that when the "leys" were planned Britain was comparatively bare of trees, that the Norman castle-mounds are pre-Roman; that there were no villages or towns in the Neolithic period; that A.-S. *btac*, pale, is the same word as A.-S. *blac*, black; that *coldharbour*, a place-name with a perfectly intelligible meaning, is derived from the Welsh *coel*, splendour, whence also the Cole Hills—and why not our friend "Old King Cole"? Everything he sees, hears, or misunderstands is fresh evidence for Mr. Watkins's fad.

E. A. B.

BOOKS RECEIVED

(Publishers are requested to notify prices)

La Géométrie Non Euclidienne. Par P. Barbarin. Troisième Édition. Paris: Gauthier-Villars et Cie. (Pp. 176.) Price 15 fcs.

Mathematical Preparation for Physical Chemistry. By Farrington Daniels. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. x + 308.) Price 15*s*.

L'Évolution des Idées Géométriques dans la Pensée Grecque. Point, Ligne, Surface. Par Federigo Enriques. Traduit sur la troisième édition Italienne par Maurice Solovine. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1927. (Pp. 48.) Price 12 fcs.

Dynamical Systems. By George D. Birkhoff, Ph.D., Sc.D. American Mathematical Society Colloquium Publications, Vol. IX. New York: American Mathematical Society, 501 West 116th Street, 1927. (Pp. viii + 295.)

The Quantum and its Interpretation. By H. Stanley Allen, M.A., D.Sc. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xiii + 274, with 30 figures.) Price 12*s*. 6*d*. net.

Numerische Infinitesimalrechnung von Dr. Martin Lindow. Berlin und Bonn: Ferd. Dummlers Verlagsbuchhandlung, 1928. (Pp. viii + 176, with 17 figures.) Price 15 M.

Die Elemente der Achten Gruppe der Periodischen Systeme. Erster Teil. Die Edelgase, von Dr. Eugen Rabinowit sch. Leipzig: Verlag von S. Hirzel, 1928. (Pp. 522, with 50 figures.) Price 45 M.

A Guide to the Constellations. By Samuel G. Barton, Ph.D., and W. H. Barton. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. x + 74.) Price 12*s*. 6*d*. net.

Adsorption und Kapillarkondensation von Erick Hückel. Privatdozent für Physik an der Eidgenössischen Technischen Hochschule Zürich. Leipzig: Akademische Verlagsgesellschaft, m.b.H., 1928. (Pp. vii + 308, with 34 figures.) Price 18 M.

Classified Problems in Physics. By Dennis Brook Briggs, M.A., F.I.C., F.C.S. London: Sidgwick & Jackson, 44 Museum Street, W.C.1. (Pp. Part I, vii + 128; Part II, vii + 127; Part III, viii + 182.) Price 3*s*. each, for Parts I and II, and 3*s*. 6*d*. for Part III.

The Electrical Conductivity of the Atmosphere and its Causes. By Victor F. Hess, Ph.D., Professor of Experimental Physics in the University of Graz. Translated from the German by L. W. Codd, M.A. London: Constable & Company, 10 Orange Street, Leicester Square, W.C.2, 1928. (Pp. xviii + 204.) Price 12*s*. 6*d*. net.

New Worlds for Old. The Realm of Modern Physics. By Robert G. Lunnon, M.D., M.Sc., Lecturer in Physics in the University of Durham, Armstrong College, Newcastle-on-Tyne. London: Methuen & Co., 36 Essex Street, W.C. (Pp. v + 106.) Price 2*s*. 6*d*. net.

Atomstruktor und Atombindung von Dr. J. Stark, Universitätsprofessor für Physik. Berlin: Polytechnische Buchhandlung A. Seydel, Königgratzerst. 31. (Pp. xx + 198.) Price Rm. 6.

Mémorial des Sciences Physiques. Fascicule I. La Mécanique ondulatoire. Par M. Louis de Broglie. Fascicule II La Télémétrie Monostatique, par M. Armand de Gramont. Paris: Gauthier-Villars et Cie., 55 Quai des Grands-Augustins, 1928. (Pp. Fascicule I, 54; Fascicule II, 59.)

Cohesion and Related Problems. A General Discussion held by the Faraday Society, November 1927. (Pp. 180.) Price 10*s*. 6*d*. net.

Kinetische Probleme Bearbeitet. Von Dr. E. D. Bruins und Dr. J. Reudler ins Deutsche ubersetzt von Dr. G. O. De Haas-Lorentz. mit 25 textfiguren. Leipzig: Akademische Verlagsgesellschaft m.b.H., 1928. (Pp. vi + 78.) Price Rm. 17.

Materiewellen und Quanten Mechanik. Von Arthur Haas. Leipzig: Akademische Verlagsgesellschaft, m.b.H., 1928. (Pp. vii + 160.) Price 6.50 M.

The Rise of Modern Physics. A Popular Sketch. By Henry Crew, Ph.D. London: Baillière, Tindall & Cox, 8 Henrietta Street, W.C.2, 1928. (Pp. xv + 356, with 36 illustrations.) Price 22*s*. 6*d*. net.

Intermediate Electricity and Magnetism. By R. A. Houstoun, M.A., D.Sc. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1928. (Pp. x + 170.) Price 4*s*. 6*d*. net.

Ceramics. Clay Technology. By Hewitt Wilson. London: McGraw-Hill Co., 1927. (Pp. xiv + 296.) Price 20*s*. net.

This book is founded upon the lectures on Clay Technology given by the author at Ohio State University and covers the general theory of clay-working, together with a valuable chapter upon the physical properties of mixed silicates from the point of view of the Phase Rule. It should be a very useful book for those studying for or engaged in the ceramic and clay industries.

Elementary Practical Physical Chemistry. By Frank Sherwood Taylor, M.A., B.Sc. Oxford: at the Clarendon Press, 1928. (Pp. xi + 130.) Price 3*s*. 6*d*. net.

Organic Synthesis. An Annual Publication of Satisfactory Methods for the Preparation of Organic Chemicals. Frank C. Whitmore, Editor-in-Chief. Vol. VII. New York: John Wiley & Sons; London: Chapman & Hall, 1927. (Pp. vi + 105.) Price 7*s*. 6*d*. net.

Hermes, or the Future of Chemistry. By T. W. Jones, B.Sc., F.C.S. London: Kegan Paul, Trench, Trübner & Co., Broadway House, Carter Lane, E.C., 1927. (Pp. 88.) Price 2*s*. 6*d*. net.

The Properties of Silica. An Introduction to the Properties of Substances in the Solid Non-Conducting State. By Robert B. Sosman, Ph.D. New York: The Chemical Catalog Company, 419 Fourth Avenue at 29th Street, 1927. (Pp. 855.) Price 12.50 dollars.

Chemical Encyclopædia. An Epitomised Digest of Chemistry and its Industrial Applications. By C. T. Kingzett, F.I.C., F.C.S. Fourth Edition. London: Baillière, Tindall & Cox, 7 Henrietta Street, Covent Garden, W.C.2, 1928. (Pp. viii + 808.) Price 35*s*. net.

Radio-Elements as Indicators and other Selected Topics in Inorganic Chemistry. By Fritz Paneth, University of Berlin. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. iv + 164.) Price 12*s*. 6*d*. net.

The Composition of Water. By J. R. Partington, M.B.E., D.Sc., Professor of Chemistry, East London College, University of London. London: G. Bell & Sons, 1928. (Pp. viii + 106.) Price 1*s*. 6*d*. net.

Organic Chemistry for Advanced Students. By Julius B. Cohen, Ph.D., D.Sc., F.R.S. Fifth Edition, in three volumes. London: Edward Arnold & Co., 41 Maddox Street, 1928. (Pp. Part I, vii + 427; Part II, vii + 487; Part III, vii + 440.) Price 18*s*. net.

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. Mellor, D.Sc., F.R.S. Volume VIII. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1928. (Pp. x + 1110.) Price 63*s*. net.

Radiation in Chemistry. By R. Alan Morton, B.Sc., Ph.D., F.I.C., Lecturer in Chemistry in the University of Liverpool. London: Baillière, Tindall & Cox, 7 Henrietta Street, Covent Garden, W.C.2, 1928. (Pp. xv + 284.) Price 15*s*. net.

Biological Chemistry and Physics of Sea Water. By H. W. Harvey, M.A., Hydrographer at the Laboratory of the Marine Biological Association, Plymouth. Cambridge: at the University Press, 1928. (Pp. x + 194, with 65 figures.) Price 10*s*. 6*d*. net.

Creatine and Creatinine. By Andrew Hunter, M.A., M.B., F.R.S.Can. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1928. (Pp. vii + 281.) Price 14*s*. net.

The Chemistry of Chemotherapy. By G. Malcolm Dyson, Ph.D., London: Ernest Benn, Bouverie House, Fleet Street, E.C.4. (Pp. viii + 272.) Price 32*s*. 6*d*. net.

A Comprehensive Survey of Starch Chemistry. Vol. I. Compiled and edited by Robert P. Walton, in collaboration with 20 others. New York: The Chemical Catalog Company, 419 Fourth Avenue at 29th Street, 1928. (Pp. 360.) Price 10 dollars.

The Geology of Malayan Ore-Deposits. By J. B. Scrivenor, M.A., F.G.S., Geologist to the Federated Malay States Department. London: Macmillan & Co., St. Martin's Street, 1928. (Pp. xv + 216.) Price 16*s*. net.

A Textbook of General Botany. For Colleges and Universities. By Richard M. Holman and Wilfred W. Robbins. Second Edition. New York: John Wiley & Sons; London: Chapman & Hall, 1927. (Pp. xiii + 623, with 415 figures.) Price 20*s*. net.

Truck Crop Plants. By Henry Albert Jones, Ph.D., and Joseph Tooker Rosa, Ph.D. London: McGraw-Hill Publishing Co., 6 Bouverie Street, 1928. (Pp. xiv + 538.) Price 25*s*. net.

Introductory Science for Botany Students. By K. E. Maris, M.A., Head Mistress, County Secondary School for Girls, Woking. London: John Murray, Albemarle Street, W. (Pp. vii + 181.) Price 3*s*. net.

Studies in the Ecological Evolution of the Angiosperms. By J. W. Bews, M.A., D.Sc., New Phytologist Reprint, No. 16. London: Wheldon & Wesley, 2 Arthur Street, New Oxford Street, W.C.2, 1927. (Pp. viii + 134.) Price 8*s*. net.

The Social World of the Ants. Compared with that of Man. By Auguste Forel. Translated by C. K. Ogden. In two volumes. London and New York: G. P. Putnam's Sons. (Pp. Vol. I, xlv + 551, with 9 plates and 95 figures; Vol. II, xx + 445, with 24 plates and 138 figures.) Price 3 guineas net.

Birds of the Ocean. A Handbook for Voyagers, containing Description of all the Sea-birds of the World, with Notes on their Habits and Guides to their Identification. By W. B. Alexander, M.A. New York: G. P. Putnam's Sons; London: The Knickerbocker Press, 1928. (Pp. xxiii + 428, with 140 illustrations.)

Selene, or Sex, and the Moon. By H. Munro Fox, Professor of Zoology in the University of Birmingham. London: Kegan Paul, Trench, Trübner & Co., Carter Lane, E.C., 1928. (Pp. 84.) Price 2*s*. 6*d*. net.

Ciliary Movement. By J. Gray, M.A., Fellow of King's College, Cambridge: at the University Press, 1928. (Pp. viii + 162, with 105 figures.) Price 10*s* 6*d*. net.

Animal Life of the Carlsbad Cavern. By Vernon Bailey, Biologist, United States Biological Survey. Baltimore, U.S.A.: The Williams & Wilkins Company, 1928. (Pp. xiii + 185, with 67 figures.) Price 13*s.* 6*d.* net.

Evolution by Symbiosis. By H. Reinheimer. With Preface by the Rev. Prof. H. Maurice Relton, D.D. Surbiton: Grevett & Co., 119 Ewell Road, 1928. (Pp. viii + 141.) Price 5*s.* net.

The Theory and Practice of Radiology. With a Synopsis of Radiography and Radiotherapy. A Treatise in Four Volumes. By Bernard J. Leggett, M.R.C.S., L.R.C.P., A.M.I.E.E. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1928. (Pp. Vol. I, xii + 237; Vol. II, xi + 307; Vol. III, xi + 549, all illustrated.) Price 18*s.*, 25*s.*, and 42*s.* per volume respectively.

History of the Physiological Society during its First Fifty Years, 1876–1926. By Sir Edward Sharpey-Schafer, F.R.S. Issued by the Society and published as a Supplement to the Journal of Physiology, December 1927. London. Cambridge University Press, Fetter Lane, E.C., 1927. (Pp. 198.) Price 15*s.* net.

Treatment for Diabetes Mellitus. By O. Leyton, M.D., D.Sc., F.R.C.P., Physician to the London Hospital. Fourth Edition. London: Adlard & Son, 21 Hart Street, W.C.1. (Pp. iv + 98.)

A Short History of Medicine. By Charles Singer, M.A., M.D., D.Litt. Oxford: at the Clarendon Press, 1928. (Pp. xxiv + 368, with 142 figures.) Price 7*s.* 6*d.* net.

Common Principles in Psychology and Physiology. By John T. Maccurdy, M.D., M.A. Cambridge: at the University Press, 1928. (Pp. xvii + 284.) Price 15*s.* net.

Electrical Engineering Economics. A Study of the Economical Use and Supply of Electricity. By D. J. Bolton, B.Sc., A.M.I.E.E. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1928. (Pp. xi + 305.) Price 21*s.* net.

The Principles of Electric Power Transmission. By Alternating Currents. By H. Waddicor, D.Sc., A.M.I.E.E. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1928. (Pp. xix + 399.) Price 21*s.* net.

Aerial Photography. A Comprehensive Survey of its Practice and Development. By Clarence Winchester and F. L. Wills, F.R.P.S., with a Foreword by Sir Alan J. Cobham, K.B.E., A.F.C., and Introductory Notes by Sir Peter Clutterbuck, C.I.E., C.B.E., and Sir Felix J. C. Pole. London: Chapman & Hall, 11 Henrietta Street, W.C., 1928. (Pp. xvi + 236, with 176 figures.) Price 25*s.* net.

Cours de Mécanique. Professé à l'École Supérieure des Mines. Par Paul Lévy. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1928. (Pp. 303.) Price 50 fcs.

An Approach to Winged Flight. By John D. Batten, M.A., LL.B. Brighton: The Dolphin Press, Spring Gardens, 1928. (Pp. 56, with 10 plates.) Price 5*s.* net.

An Introduction to the Technique of Section-Cutting. From the Notes of the late Mr. Peter Jamieson. Edited by Frances M. Ballantyne, M.A. Assistant in the Zoology Department, the University of Glasgow. Edinburgh: E. & S. Livingstone, 17 Teviot Place, 1928. (Pp. xii + 80.) Price 3*s.* 6*d.* net.

Traité Pratique de Navigation Aerienne. Par A-B. Duval et L. Hébrard. Avec Prefaces de Laurent-Eynac et Verdurand. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1928. (Pp. x + 196.) Price 30 fcs.

Brachiopod Morphology and Genera (Recent and Tertiary). By J. Allan Thomson, M.A., D.Sc., Director of the Dominion Museum, Wellington, New Zealand. New Zealand Board of Science and Art. Manual No. 7. Wellington, N.Z.: Dominion Museum, 1927. (Pp. vii + 338, with 11 plates and 103 figures.) Price 17*s*. net.

Intelligence and Mental Growth. By Claude A. Claremont, B.Sc. London: Kegan Paul, Trench, Trübner & Co., Broadway House, Carter Lane, E.C., 1927. (Pp. 138.) Price 2*s*. 6*d*. net.

Harmonia Harmonica. Vol. II. By Clarence S. Hill. London and Bournemouth: W. Mate & Sons, 1927. (Pp. 151.) Price 21*s*. net.

Orokaiva Magic. By F. E. Williams, with a Foreword by R. R. Marett. London: Oxford University Press, 1928. (Pp. xii + 231.) Price 12*s*. 6*d*. net.

The Rate of Living. Being an Account of Some Experimental Studies on the Biology of Life Duration. By Raymond Pearl of the Johns Hopkins University. London: University of London Press, 10 Warwick Lane, E.C.4, 1928. (Pp. 185.) Price 10s. 6d. net.

The Subject Index to Periodicals. Issued by The Library Association, 1926. London: The Library Association, 1927. (Pp. 554.) Price £3 10*s*.

The Basis of Sensation. The Action of the Sense Organs. By E. D. Adrian, M.D., F.R.C.P., F.R.S. London: Christophers, 22 Berners Street, W.1. (Pp. 122.) Price 7*s*. 6*d*. net.

The Hand and the Mind. By M. N. Laffan. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1928. (Pp. vii + 96.) Price 4*s*. 6*d*. net.

The Microtomist's Vade-Mecum. A Handbook of the Methods of Microscopic Anatomy. Ninth Edition. Edited by J. Brontë Gatenby and E. V. Cowdry, with the collaboration of ten others. London: J. & A. Churchil, 40 Gloucester Place, Portman Square, 1928. (Pp. x + 710, with 9 plates.) Price 30*s*. net.

Homer Lane and the Little Commonwealth. By E. T. Bazeley, M.A., with an Introduction by the Right Hon. the Earl of Lytton, P.C. London: George Allen & Unwin, Museum Street. (Pp. 200.) Price 7*s*. 6*d*. net.

Phenomenos Luminosos Terrestres Como Provas do Movimento de Terra no Espaco. By Padre Affonso M. Wenger, S.V.D. Brasil: Bello Horisonte, Minas, 1927. (Pp. 16.)

Manual of Meteorology. Vol. II. Comparative Meteorology. By Sir Napier Shaw, LL.D., Sc.D., F.R.S. With the Assistance of Elaine Austin, M.A. Cambridge: at the University Press, 1928. (Pp. xxxix + 445, with 225 figures.) Price 36*s*. net.

Science for You. By J. G. Crowther. London: George Routledge & Sons, 68 Carter Lane, E.C., 1928. (Pp. x + 241.) Price 5*s*. net.

Coloured Thinking and Other Studies in Science and Literature. By D. F. Fraser-Harris, M.D., D.Sc., F.R.S.E. London: George Routledge & Sons, 68 Carter Lane, E.C. 1928. (Pp. vii + 269.) Price 5*s*. net.

The Basis of Memory. By W. R. Bousfield, K.C., F.R.S. London: Kegan Paul, Trench, Trübner & Co., Carter Lane, E.C., 1928. (Pp. 132.) Price 2*s*. 6*d*. net.

The Garden Interests of Madeira. By Michael Comport Grabham, M.D., LL.D., Fellow of the Royal College of Physicians. London: William Clowes & Sons, 1926. (Pp. 100.) Price 5*s*. net.

Race and Civilisation. By Friedrich Hertz. Translated by A. S. Levetus and W. Entz. London: Kegan Paul, Trench, Trübner & Co.; New York: The Macmillan Co., 1928. (Pp. xii + 328.) Price 18*s*. net.

William Bateson, F.R.S., Naturalist. His Essays and Addresses, together with a short Account of his Life. By Beatrice Bateson. Cambridge: at the University Press, 1928. (Pp. ix + 473.) Price 21*s*. net.

The Pre-War Mind in Britain. An Historical Review. By Caroline E. Playne. London: George Allen & Unwin, Museum Street. (Pp. 444.) Price 16*s*. net.

Cain, or the Future of Crime. By George Godwin. London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1928. (Pp. 108.) Price 2*s*. 6*d*. net.

Introduction to the History of Science. Vol. I. From Homer to Omar Khayyám. By George Sarton. Baltimore: The Williams & Wilkins Company; London: Baillière, Tindall & Cox, 8 Henrietta Street. (Pp. xi + 840.) Price 45*s*. net.

Harrison of Ightham. A Book about Benjamin Harrison, of Ightham, Kent. Made up Principally of Extracts from his Notebooks and Correspondence. Prepared for Publication by Sir Edward R. Harrison. London: Oxford University Press, 1928. (Pp. iv + 395, with 2 plates.) Price 15*s*. net.

The Alchemy of Light and Colour. By Oliver L. Reiser. London: Kegan Paul, Trench, Trübner & Co., Carter Lane, E.C., 1928. (Pp. 86.) Price 2*s*. 6*d*. net.

The Standardisation of Error. By Vihjalmur Stefansson, M A., LL.D. London: Kegan Paul, Trench, Trübner & Co., Carter Lane, E.C., 1928. (Pp. 110.) Price 2*s*. 6*d*. net.

Textile Microscopy. By L. G. Lawrie. London: Ernest Benn, Ltd., Bouverie House, Fleet Street, E.C.4. (Pp. x + 144, with 44 figures and 3 plates.) Price 25*s*. net.

Silvicultural Systems. By R. S. Troup, C.I.E., D.Sc., F.R.S. Oxford: at the Clarendon Press, 1928. (Pp. xii + 199, with 86 figures.) Price 21*s*. net.

The Determination of Minerals under the Microscope. With Special Reference to the Interpretation of Interference Phenomena. By John W. Evans, C.B.E., D.Sc., LL.B., F.R.S., F.G.S. London: Thomas Murby & Co., 1 Fleet Lane, E.C.4. (Pp. xii + 110.) Price 7*s*. 6*d*. net.

The Year Book of the Universities of the Empire, 1928. Published for the Universities Bureau of the British Empire. London: G. Bell & Sons, 1928. (Pp. xiii + 866.) Price 7*s*. 6*d*. net.

The Sixth Sense and Other Studies in Modern Science. By D. F. Fraser-Harris, M.D., D.Sc., F.R.S.E. London: George Routledge & Sons, 68 Carter Lane, E.C., 1928. (Pp. vii + 174.) Price 5*s*. net.

Essentials of Scientific Method. By A. Wolf, M.A., D.Lit. Second Edition. Revised and Enlarged. London: George Allen & Unwin, Museum Street, 1928. (Pp. 174.) Price 5*s*. 6*d*. net.

Birds and Beasts of the Greek Anthology. By Norman Douglas. London: Chapman & Hall. (Pp. vii + 215.) Price 7*s*. 6*d*. net.

Culture. The Diffusion Controversy. By G. Elliot Smith, D.Sc., Bronislaw Malinowski, D.Sc., Herbert J. Spinden, Ph.D., and A. Goldenweiser, Ph.D. London: Kegan Paul, Trench, Trübner & Co., Carter Lane, E.C., 1928. (Pp. 89.) Price 2*s*. 6*d*. net.

Statistical Methods for Research Workers. By R. A. Fisher, Sc.D. Second Edition. Revised and Enlarged. Edinburgh: Oliver and Boyd, Tweeddale Court; and London: 33 Paternoster Row, E.C., 1928. (Pp. xi + 269.) Price 15*s*. net.

Printed in Great Britain by
Hazell, Watson & Viney, Ld., London and Aylesbury.

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By E. C. TITCHMARSH, M.A., University College, London.

Some Geometrical Problems.—The following problem was considered by A. S. Besicovitch, *Math. Zeitschrift*, **27**, 1927, p. 312: What is the plane figure of smallest area in which we can turn a line of given length through 360°, so that it returns to its original position? The answer is rather surprising. It is possible to do this in regions of arbitrarily small area. The regions considered are, of course, not convex.

The proof depends on the following lemma: Suppose we divide up a given triangle into n equal parts by straight lines through the vertex, and allow each of these parts to move separately in the direction of the base, as it were on parallel rails along the base and through the vertex. Then we can choose the number n, and the displacements of the n parts, in such a way that the area finally covered by all the parts together is arbitrarily small. A simplified proof of this is given by O. Perron, *Math. Zeitschrift*, **28** (1928), 383–6.

Some problems of vector-regions are considered by T. Estermann, " Über den Vektorenbereich eines konvexen Körpers," *Math. Zeitschrift*, **28**, 1928, 471–5. Suppose we have a plane convex region K; the line joining any two points of the regions forms a vector. Replace such vector by the corresponding one which starts from the origin. These new vectors trace out a " vector-space," say W. What is the area of W? It was proved by Rademacher, and a new proof is given by Estermann, that if A(K), A(W), are the areas of K and W,

$$4A(K) \leqslant A(W) \leqslant 6A(K).$$

The first of these inequalities reduces to an equality only when K is symmetrical about a centre, and the second only when K is a triangle.

In the corresponding problem in three dimensions, let

V(K), V(W), be the volumes of the original space and its vector-space. Then it is proved by Estermann that

$$8V(K) \leqq V(W) \leqq 20V(K).$$

The first inequality reduces to an equality only when K is symmetrical about a centre, and the second only when K is a tetrahedron.

These results certainly suggest an extension to any number of dimensions; but apparently, in more dimensions than three, new difficulties appear in the proof.

Taylor's Theorem.—Everyone knows that Taylor's theorem may be written in the form

$$f(a+h) = \sum_{r=0}^{n-1} \frac{h^r}{r!} f^{(r)}(a) + \frac{h^n}{n!} f^{(n)}(a+\theta h),$$

where $0 < \theta < 1$. It is perhaps not so well known that, for small values of h, θ is approximately $1/(n+1)$, at any rate if $f^{(n+1)}(a)$ is not zero. In a paper entitled "Sul resto di Lagrange nello sviluppo di Taylor," *Rend. di Palmero*, **52**, 1928, 44–57, P. Mazzoni finds that the error made in substituting $1/(n+1)$ for θ in the above formula may be written

$$\frac{nh^{n+2}}{2(n+1)(n+2)!} f^{n+2}(\xi),$$

where $a < \xi < a + h$.

If $f(x)$ is a polynomial of degree $n + 1$, we have $f^{(n+2)}(\xi) = 0$ for all values of ξ, so that in this case θ is exactly $1/(n+1)$.

The author makes various applications of the result, and extends it to functions of any number of variables.

Subharmonic Functions.—These functions, referred to in our previous article, are the subject of an important memoir by P. Montel, "Sur les fonctions convexes et les fonctions sousharmoniques," *Journal de Math.* (**9**), 7, 1928, 29–60. In the first part the author proves some new results about convex functions, the one-dimensional case of subharmonic functions; the main result is that the necessary and sufficient condition that the logarithm of a positive function $u(x)$ of the variable x should be a convex function of x, is that $e^{ax}u(x)$ should be convex in x, for all values of the real constant a. It is shown also that, if $\log u(x)$ is a convex function of $\log x$, then so is the function

$$\log \int_0^x u(t)dt.$$

Passing to the two-dimensional case, the fundamental condition for a subharmonic function is

$$u(x,y) \leqq \frac{1}{2\pi}\int_0^{2\pi} u(x + h\cos\theta, y + h\sin\theta)d\theta.$$

The author considers also positive functions whose logarithms are subharmonic, and obtains results similar to those for functions whose logarithms are convex. It is proved that the greatest of the limits of the values, at each point, of subharmonic functions belonging to a normal family, is a subharmonic function, a theorem which finds numerous applications.

In the last part a fundamental result is obtained on subharmonic functions which take a constant value at each point of a closed curve belonging to a family of curves U = constant, U being a harmonic function. The subharmonic function is then a convex function of U. The well-known " three-circles theorem " of Hadamard, and various recent results on mean values of moduli of analytic functions, due to Hardy and R. Nevanlinna, are particular cases of this theorem. In the case U = x the functions considered are defined in the strip between two parallel lines, and we obtain new proofs of convexity theorems due to Walther, Doetsch, and Hardy, Ingham and Pólya. Finally the author shows the connection between his work and that of Fabry on the associated radii of convergence of power series in two variables, and proves that, in the case of any number of variables, one of the radii of convergence is a subharmonic function of the others.

Functions of a Real Variable.—In a note in the Paris *Comptes Rendus* of May 21, 1928, Mlle N. Bary announces some remarkable results on the classification of continuous functions. The order of ideas is the same as that which led Jordan to introduce *functions of bounded variation*, viz. those functions which can be obtained by addition and subtraction of a finite number of *monotonic* functions. Here the fundamental functions are *absolutely continuous*, *i.e.* such that the sum of the absolute variations of the function over any number of intervals, whose total length is made arbitrarily small, itself becomes arbitrarily small. We recall the well-known theorem that any function which is an integral, in Lebesque's sense, is absolutely continuous, and conversely.

Now for the main result: *Any continuous function F(x) of a real variable is the sum of three absolutely continuous functions of absolutely continuous functions,*

$$F(x) = f_1\{\phi_1(x)\} + f_2\{\phi_2(x)\} + f_3\{\phi_3(x)\}.$$

The author had previously shown that a representation of

this kind was possible with a sum of four terms. The novel point of the present paper is that the fourth term is superfluous. Also the result is final, in the sense that two terms are not always sufficient.

We call the *rank* of a continuous function the smallest number of terms "absolutely continuous function of absolutely continuous function" which are necessary to represent it. Thus all continuous functions are of rank 3 at most; further, functions of rank 1, 2, and 3 actually exist. A characteristic property of functions of rank 1 is known; it is necessary and sufficient that the set of values of $F(x)$ at the points where its derivative $F^1(x)$ does not exist, or is not finite, should be of measure zero. This was proved by N. Bary and D. Menchoff, "Sur l'intégrale de Lebesque-Stieltjes et les fonctions absolument continues de fonctions absolument continues," *Annali di Matematica* (4), **5**, 1927–8, p. 43. Our information about functions of rank 2 is not so complete, though some properties of such function are known.

Finally we can define a class of functions of rank 3 exactly, the so-called "*fonctions ridées.*" A *fonction ridée* is a continuous function $F(x)$ with the following property. For any set E of positive measure in the domain of definition of $F(x)$, there exists a set E_1 of zero measure contained in E, such that $F(x)$ is monotonic in E_1, and the set of values of $F(x)$ in E_1 is of positive measure.

The Zeta-function of Riemann.—The theory of the zeta-function of Riemann,

$$\zeta(s) = \sum_{n=1}^{\infty} \frac{1}{n^s},$$

continues to make progress, however remote the ultimate solution of the problems which it presents may be. Several papers on this subject have recently appeared. An important one by A. E. Ingham, *Proc. London Math. Soc.*, **27**, 1927, 273–300, deals with various mean-value theorems. The classical theorem, that

$$\lim_{T\to\infty} \frac{1}{T}\int_1 \left|\zeta(\sigma + it)\right|^2 dt = \zeta(2\sigma)$$

if $\sigma > \frac{1}{2}$, has received various extensions; *e.g.* it has been proved by Hardy and Littlewood that

$$\int_1^T \zeta(\tfrac{1}{2} + it) \; dt \backsim T \log T.$$

The problem of formulæ of this type is now discussed by

Ingham in great detail. His general result gives an asymptotic formula for the integral

$$\int_k^T \zeta^{(\mu)}(a+it)\,\zeta^{(\nu)}(b-it)\,dt,$$

where a and b are any fixed complex numbers, and μ and ν are positive integers, $\zeta^{(\mu)}(s)$ being the μ-th derivative of $\zeta(s)$. The general result is rather complicated; in particular cases we obtain new results of striking precision. Thus

$$\int_0^T \left|\zeta(\tfrac{1}{2}+it)\right|^2 dt = T\log T - (1+\log 2\pi - 2C)T + O(\sqrt{T}\log T),$$

where C is Euler's constant. The novelty of the result consists in the comparative smallness of the remainder term, which had previously been shown by Littlewood to be $O(T^{3/4+\epsilon})$. As usual in these questions, the analysis is somewhat difficult.

In the second part of the paper the asymptotic formula

$$\int_0^T \left|\zeta(\tfrac{1}{2}+it)\right|^4 dt = \frac{1}{2\pi^2} T\log^4 T + O(T\log^3 T)$$

is obtained. As we take higher powers of $|\zeta(s)|$ in these formulæ, the difficulty of dealing with them increases greatly, and in fact no such formula is known for powers greater than the 4th. Apparently this would still be true, even if we could prove the Riemann hypothesis. The proof depends on an "approximate functional equation" for the square of (s), the particular form used being

$$\zeta^2(\tfrac{1}{2}+it) = \sum_{n=1}^{t/2\pi} \frac{d(n)}{n^{\frac{1}{2}+it}} + i\left(\frac{t}{2\pi e}\right)^{-2it} \sum_{n=1}^{t/2\pi} \frac{d(n)}{n^{\frac{1}{2}-it}} + O(\log t),$$

where $d(n)$ is the number of divisors of n. This is due to Hardy and Littlewood, but the proof of it has not yet been published.

The second paper on this subject is one by Prof. Littlewood, "On the function $1/\zeta(1+ti)$," *Proc. London Math. Soc.*, **27**, 1928, 349–57. The behaviour of the function $\zeta(1+it)$ for large values of t presents a very difficult problem. A simple argument shows that

$$\zeta(1+it) = O(\log t).$$

A far more complicated method (Wey's inequalities) shows that

$$\zeta(1+it) = O\left(\frac{\log t}{\log\log t}\right).$$

No more in this direction is known; but it is known that the order of the function is not *less* than log log t, and in fact that

$$\overline{\lim} \frac{|\zeta(1+it)|}{\log \log t} \geqslant e\mathrm{C}.$$

If, however, we assume the Riemann hypothesis, that $\zeta(s) \neq 0$ for $\sigma > \frac{1}{2}$, then we can prove that $\zeta(1+it)$ is really of the same order as log log t, and in fact that

$$\overline{\lim} \frac{|\zeta(1+it)|}{\log \log t} \leqslant 2e\mathrm{C}.$$

Thus the only remaining problem is that of the exact value of the constant.

The theorems on $1/\zeta(1+it)$ follow much the same course, though in some ways they are more difficult to prove. We can prove that

$$\frac{1}{\zeta(1+it)} = \mathrm{O}(\log t),$$

but naturally this requires some knowledge of the zeros of $\zeta(s)$ near $\sigma = 0$. If we use Weyl's inequalities as well, we obtain

$$\frac{1}{\zeta(1+it)} = \mathrm{O}\left(\frac{\log t}{\log \log t}\right).$$

See E. Landau, "Über die Riemannsche Zetafunktion in der Nahe von $\sigma = 1$," *Rend. di Palmero*, **50**, 1926, 423–427. On the other hand, we know that

$$\overline{\lim} \frac{1/|\zeta(1+it)|}{\log \log t} > 0.$$

Littlewood's paper is particularly concerned with the behaviour of $1/\zeta(1+it)$ on the Riemann hypothesis. He proves that there is a constant b such that

$$\tfrac{1}{4}b \leqslant \overline{\lim} \frac{1/|\zeta(1+it)|}{\log \log t} \leqslant 2b.$$

Here the constants differ by a factor 8. The paper ends with a discussion of the finer points of these theorems, and how the different constants arise.

In a second paper, "On the Class Number of the corpus $\mathrm{P}(\sqrt{-k})$," *Proc. London Math. Soc.*, **27**, 1928, 358–72, Littlewood says: "We owe to Landau the important observation that some of the arguments used to discuss the behaviour of the Riemann zeta-function for fixed σ and large t apply ***mutatis***

mutandis to the function $L(s)$ for fixed real s and large k, where

$$L(s) = \sum_{n=1}^{\infty} \frac{\chi(n)}{n^s} \quad (\sigma < 0),$$

and χ is a non-principal Dirichlet's character to modulus k." Using this idea, he obtains a number of results analogous to those of the previous paper, but with k as variable. Thus as $k \to \infty$

$$\frac{1 + o(1)}{2b \log \log k} < |L(1)| < \{1 + o(1)\}\, 2c \log \log k,$$

where b and c are certain constants, it being assumed that no $L(s)$ has a zero with real part greater than $\frac{1}{2}$.

In a paper entitled "Eine Illustration zur Riemannschen Vernutung," *Math. Annalen*, **99**, 1928, 150–61, G. Hoheisel constructs a function $\zeta^*(s)$ which has no complex zeros of real part $> \frac{1}{2} + K$, where K is an arbitrarily small positive number (the function depends on the value of K), and which has many of the properties of $\zeta(s)$. In fact :

1. $\zeta^*(s)$ satisfies the same functional equation as $\zeta(s)$.

2. $\zeta^*(s)$ has a simple pole at $s = 1$, and the same "trivial" zeros as $\zeta(s)$.

3. $\zeta^*(s)$ has the same Fourier-coefficients as $\zeta(s)$, *i.e.*

$$\lim_{T\to\infty} \frac{1}{T}\int_0^T \zeta^*(\sigma + it)e^{i\lambda t}\,dt = \lim_{T\to\infty} \frac{1}{T}\int_0^T \zeta(\sigma + it)e^{i\lambda t}\,dt$$

for all values of λ, and all values of σ which are not too small.

4. $\zeta^*(s)$ is a generalised almost periodic function in the sense of Besicovitch ; and for the same values of σ as in 3

$$\lim \frac{1}{T}\int_0^T \left|\zeta(\sigma + it) - \zeta^*(\sigma + it)\right|^2 dt = 0.$$

5. $\zeta^*(s) = \zeta(s)$ on the Riemann hypothesis. In fact $\zeta^*(s)$ is obtained from $\zeta(s)$ by replacing each complex zero of $\zeta(s)$, for which $|\sigma - \frac{1}{2}| \geqq K$, by a zero on $\sigma = \frac{1}{2}$ with the same absolute value. By a well-known theorem of Carlson, the number of zero of $\zeta(s)$ in $0 < t < T$, $\sigma \geqq \frac{1}{2} + K$, is $O(T^{1-4K^2+\epsilon})$; so that, roughly, at most an infinitesimal proportion of the zeros are altered, and $\zeta(s)/\zeta^*(s)$ is, if not unity (as it evidently is on the Riemann hypothesis), at least a fairly harmless function. Hence the results mentioned. The writer notes that if $\zeta^*(s)$ is, for any σ, actually almost periodic, then it follows from 3 that, for all K, $\zeta^*(s) = \zeta(s)$, and hence that the Riemann hypothesis is true. It is not suggested that there is any independent indication of $\zeta^*(s)$ being almost periodic.

Before leaving the subject, regard for completeness compels us to mention papers by E. C. Titchmarsh in *Proc. London Math. Soc.*, **27**, 1927–8, pp. 137–50, 449–58; *ibid.*, 1928, pp. 70–80; and *London Math. Soc. Journal*, **2**, 1927, 247–54.

Analytic Continuation.—In a paper "On Certain Functions represented by Dirichlet Series," *Proc. London Math. Soc.*, **27**, 1928, 435–48, T. Estermann deals with a class of functions of which the simplest are

$$f_{l,m}(s) = \sum_{n=1}^{\infty} \frac{\{d_m(n)\}^l}{n^s},$$

where $d_m(n)$ is the number of decompositions of n into m factors, and l is a positive integer. The functions are clearly all regular if σ, the real part of s, is greater than unity. If $m = 1$, the function is simply $\zeta(s)$, and so can be continued over the whole s-plane, except for a pole at $s = 1$. If $l = 1$, $f_{1,m}(s) = \{\zeta(s)\}^m$, and the continuation again follows. For $m = 2$, $l = 2$, we have, by a formula of Ramanujan,

$$f_{2,2}(s) = \{\zeta(s)\}^4/\zeta(2s),$$

so that again the function exists in the whole plane, though this time it has an infinity of poles, at the zeros of $\zeta(2s)$. The main result is that in no other case can the function be continued beyond the line $\sigma = 0$, though it can always be continued as far as this line, and has no singularities other than poles for $\sigma > 0$. However, as we approach $\sigma = 0$, the poles become denser and denser, and in fact every point of $\sigma = 0$ is a limit-point of poles.

A problem which is in some ways more subtle is suggested by the function

$$f(s) = \prod_{m=2}^{\infty} 1/(1 - m^{-s})$$

studied by MacMahon and Oppenheim in the theory of partitions. This function is regular for $\sigma > 0$, apart from certain poles on the real axis. It has now been proved by Estermann, *Proc. London Math. Soc.*, **27**, 1928, 471–82, that $\sigma = 0$ is a line of singularities of the function. The formula

$$\log f(s) = \sum_{n=1}^{\infty} \frac{\zeta(ns) - 1}{n}$$

suggests the general problem of functions of the type

$$\sum_{n=1}^{\infty} a_n\{\zeta(ns) - 1\},$$

and it is proved that this is singular at every point of the imaginary axis if $a_n = n^\alpha + O(n^\beta)$, where $\beta < \alpha - \frac{1}{2}$.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

Ozone in the Earth's Atmosphere.—G. M. B. Dobson, D. N. Harrison, and J. Lawrence have recently discussed (*Proc. R. Soc. A.*, **114**, 1927, pp. 521–41) the connection between the total amount of ozone in our atmosphere and certain other geophysical conditions. Regular spectroscopic determinations of the amount of ozone have been made at seven places, and use has been made of the figures for four of these, namely, Oxford, Lerwick, Arosa, and Lindenberg in the discussion under review. The following points have been noted :

(1) A maximum of ozone for April and a minimum for October.

(2) A not very clearly defined relationship between ozone amount and variations of the horizontal component of the daily range of magnetic declination, particularly on days of high magnetic character.

(3) No regular connection between ozone and sunspots.

(4) A very definite connection between ozone and the distribution of atmospheric pressure at the earth's surface : the amount is high in cyclonic systems and low in anticyclones.

(5) High correlation coefficients have been found between ozone and the mean temperature of the air from four to eight kilometres' height, and between the ozone and pressure from nine to fourteen kilometres.

Items (4) and (5) are surprising in view of the fact that several French observers have found that the region containing most of the ozone lies between 45 and 650 km. above the earth's surface, a height theoretically appropriate for conversion of oxygen to ozone under the influence of ultra-violet light. An attempt has been made to explain this anomaly by supposing that there are two heights of maximum concentration of ozone, one at about 50 kilometres, where it is formed in this manner, and another lower down to which it sinks. Variations in the upper zone would be correlated with solar and magnetic conditions, and variations in the lower zone with cyclones and anticyclones. It should not be forgotten that the amount of ozone present in the surface layers of the atmosphere is an infinitesimal fraction of the total amount.

The Diurnal Variation of Temperature and Humidity at the Upper Boundaries of Clouds.—There is still much to be known about the absorption and radiation of heat in the free atmosphere, although the laws governing such phenomena appear to be sufficiently well understood. Any direct evidence of the magnitude of such changes derived from observations made in balloons, such as is given in a paper by W. Peppler (Lindenberg, *Arbeit. Preuss. Aeron. Obs.*, **15**, 1926, pp. 185–93), is therefore

very welcome. The material discussed was obtained with the aid of kite-balloons during the period 1908–15. The ascents were made at 8 h. and 14 h., *i.e.* at times when the temperature was on the average not far from its minimum and maximum values for the day. The changes of temperature that were observed to take place above layers of stratus cloud were considered, whether the base of such stratus lay above the ground or on the ground (in the latter case the cloud being ordinary wet fog). It has long been known that above such clouds the temperature normally increases with height; giving a so-called "inversion" of temperature: in this layer there is low relative humidity, and often a very clear atmosphere. Peppler found that on the average the greatest rise of temperature between 8 h. and 14 h. took place near the top of the inversion layer, that is to say in cloud-free air at some distance above the normally very definite upper surface of the stratus cloud. On the other hand, the greatest nightly drop took place at the bottom of the inversion—*i.e.* near the upper surface of the cloud. This last result clearly means that the top of the cloud radiates heat in the same way that the ground radiates heat, but the first result is, at least at first sight, surprising, for dry cloud-free air is not generally believed to absorb solar radiation readily.[1] This difficulty Peppler endeavours to meet by supposing that a large quantity of dust is present, which absorbs the radiation and passes it on to the air by conduction.

The same supposition is made to explain another anomaly: If we assume that the full solar radiation reaches the top of a stratus cloud, and allow for 80 per cent. of this to be reflected on the average, there yet remains enough heat to cause the cloud to evaporate in a very short time during the day; but such clouds do not always evaporate, consequently it appears that much of the solar radiation must be absorbed before reaching the cloud—according to Peppler by dust particles. The obvious objection to this argument is the well-known fact, to which allusion has already been made, that the air is often remarkably clear above stratus cloud. Probably most meteorologists will defer accepting this suggestion about dust particles until some measurements of the dust content are available under suitable conditions. If correct the theory appears to imply that the factor which decides whether a sheet of stratus cloud shall or shall not disperse under the action of solar radiation may often be the state of the air above it in regard to dust particles—a very useful piece of knowledge for weather fore-

[1] Apart from laboratory experiments, there is no lack of evidence to show that the temperature of the air at a height of several feet does not, when a little moisture is present, change under the action of solar radiation until the ground has been strongly heated.

casting, if it can be substantiated. Another possible explanation may be put forward of the observed rise of temperature—namely, that it is due to heating of the kite-balloon, which leads to a rise of temperature of the air in its immediate neighbourhood: the free atmosphere may undergo no such change.

Some Studies in Terrestrial Radiation.—Under this heading Dr. G. C. Simpson (*Mem. Royal Met. Soc.*, vol. ii, No. 16) gives the results of some calculations made with regard to the outgoing radiation from the earth's atmosphere in different latitudes. Many simplifying assumptions had to be made, and a meteorologist who has not made a special study of radiation is not in a position to decide to what extent the introduction of these assumptions—some of which the author himself admits to be incorrect—invalidate the final conclusions. The main result found was that over about three quarters of the earth's surface, from the equator to about latitude 50°, the outgoing radiation is nearly uniform and independent of the temperature at the earth's surface. At higher latitudes the outgoing radiation is less, falling at the poles to about 80 per cent. of the equatorial value. The values found for the outward radiation were not very different for the latitudes of Lindenberg (52° N.) and Batavia (6° S.) from values obtained by Hergesell in 1919 (Hergesell used the numerous observations of temperature in the upper air made at those two places, and employed a more elaborate process of calculation), but differed greatly from Mügge's conclusions, based on observed temperatures at the base of the stratosphere, that more heat is radiated away per square centimetre of the upper atmosphere near the poles than near the equator. An interesting point that arose in the course of the work relates to the temperature at a considerable distance above the base of the stratosphere: according to figures collected by Sir Napier Shaw the mean temperature within the stratosphere does not differ greatly with latitude at a height of about 20 kilometres, and the graphs of temperature and height for different latitudes show signs of converging to 220° A. at a height of about 25 kilometres. The chief cause of the discrepancy between Mügge's and Simpson's results lies in the fact that the former used values ranging from 228° A. at the poles to 190° A. at the equator.

There is a further point of interest: when considering the contributions of different layers of the atmosphere towards the outgoing radiation, it was found that those layers with temperature between 220° and 286° contributed most. In those parts of the world where warmer layers are present—in particular near the equator—the large amount of radiation emitted by the lowest and warmest layers, where the water vapour, assumed throughout the paper to be the only source in the

atmosphere of long-wave radiation, is most abundant, is counterbalanced by the presence of much water-vapour in the layers higher up, the latter absorbing all or nearly all of the long-wave radiation coming from below. It results from these two opposing tendencies, that more heat escapes from the surface of the earth into space near the poles than near the equator, in spite of the fact that the stream of radiation leaving the ground is twice as great at the equator. On the other hand, the equatorial troposphere, with its high absolute humidity, radiates far more heat than does the polar troposphere.

Barometric Changes due to the Passage of Occlusions.—One of the most important points in regard to the structure of the cyclonic depressions of temperate latitudes that has been brought out by the Norwegian school in recent years is the phenomenon of "occlusion." The "warm sector" of a young developing cyclone has been shown to disappear from the surface as a result of the cold air in its rear overtaking the cold air in front of it. The continued presence of the mass of warm air at a gradually increasing altitude can generally be detected afterwards, often for several days, by the presence of a mass of high cloud, from which rain, sleet, or snow often falls, and its passage over any place is almost always accompanied by changes of the barometer. Soundings by aeroplane or registering balloon have been made through such "occlusions" and have confirmed in a general way the Norwegian view of the structure of the cyclone at this stage of its evolution. (For particulars of observations made on a very well marked occlusion, reference may be made to the *Meteorological Magazine* for February 1928.)

With regard to the changes of the barometer, these may be regarded as compounded of two independent parts. There will be a general change which we may regard as independent of the presence of the occlusion: for example, an upward "surge" of the barometer may be taking place over Western Europe, and within the area covered by the "surge" there may be an occluded depression. Occluded warm air will have a different density to those of the cold air masses behind and in advance of it, and its passage overhead at any place will be accompanied by some modification of the general upward surge of the barometer. Since this modification is dependent upon the nature of the occlusion, we might be able to learn something with regard to the physical structure of the latter, provided that we can deduct from the observed barometric changes the component which is independent of the occlusion. It was doubtless with this idea in mind that A. Gião of the Portuguese meteorological service has investigated mathematically (*L'Annuaire de l'Institut de Physique du Globe de Strasbourg pour*

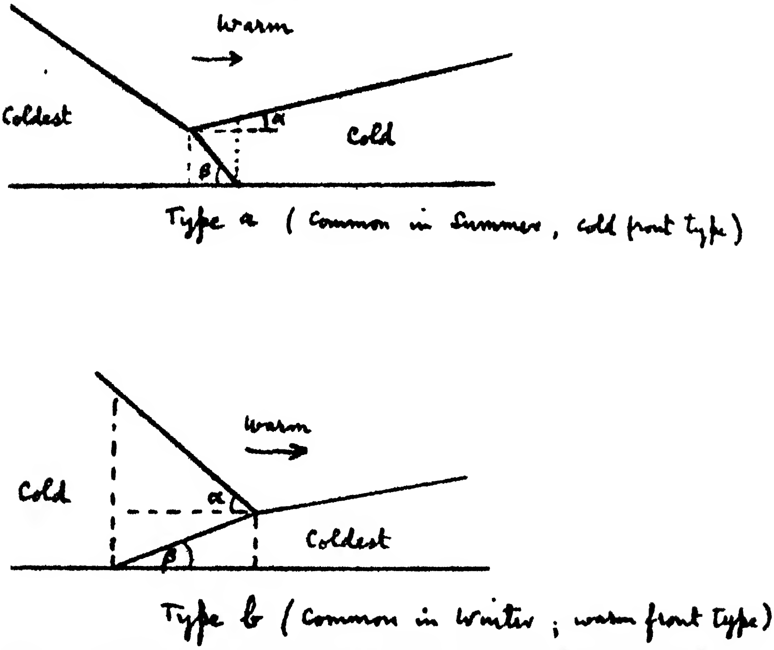


FIG. 1.—The sections shown above are from east to west through eastward travelling disturbances, looking towards north.

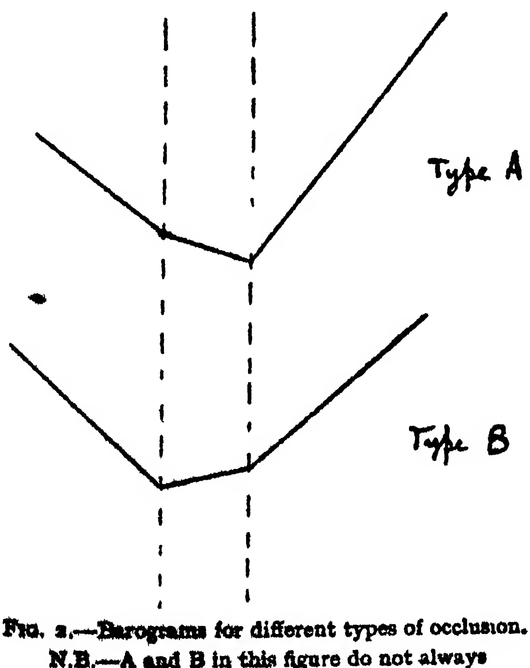


FIG. 2.—Barograms for different types of occlusion.

N.B.—A and B in this figure do not always correspond with *a* and *b* of Fig. 1.

1926) the changes of surface pressure that may be expected to occur during the passage of a surface of discontinuity separating air masses of different temperature. He assumes that the whole disturbance moves in a direction parallel with the isobars in the warm air, just as a depression with a warm sector often moves in a direction parallel with the isobars in the warm sector.[1]

Starting from this assumption, he derives the following equation for the rate of change of pressure at any fixed point on a moving surface of discontinuity :

$$\frac{dp_0}{dt} = \frac{g}{R} V \tan \alpha \frac{\Delta T}{T T'} p_0 \quad . \quad . \quad . \quad . \quad (1).$$

Here p_0 = pressure at the ground.
V = wind speed at the given point on the surface of discontinuity.
p = pressure at the given point on the surface of discontinuity.
α = slope of the surface of discontinuity.
T = temperature (absolute) in the warm air near the given point.
T' = temperature (absolute) in the cold air near this point.
$\Delta T = T - T'$.
g = acceleration of gravity.
R = the gas constant.

We may apply this to the two types of occlusion *a* and *b*, shown in Fig. 1, noting that in the region where two surfaces of discontinuity are passing overhead equation (1) takes the form :

$$\frac{dp_0}{dt} = \frac{g}{R} V p_0 \left[\frac{(\Delta T)_1}{T_1 T_1'} \tan \alpha - \frac{(\Delta T)_2}{T_2 T_2'} \tan \beta \right].$$

Where $(\Delta T_1) = | T_1' - T_1 |$; T_1 and T_1' refer to the temperature at the particular point of the upper surface of discontinuity that is overhead at the moment, and suffix 2 indicates the same quantities for the lower surface of discontinuity ; α and β are indicated in Fig. 1.

Let us first assume that pressure is governed by the upper surface of discontinuity, *i.e.* :

$$\frac{(\Delta T)_1}{T_1 T_1'} \tan \alpha > \frac{(\Delta T)_2}{T_2 T_2'} \tan \beta \quad . \quad . \quad . \quad . \quad (2).$$

[1] Exception may be taken to this : it seems impossible that the trend of the isobars at all heights in the occluded air should be exactly the same. To what extent the assumption invalidates the subsequent argument is not clear.

The barograms (after compensation for pressure change due to external causes) will be of the types shown in Fig. 2: A (for cold-front types of occlusion) and B (warm-front type).

If, on the other hand, the lower discontinuity is the governing one, *i.e.*:

$$\frac{(\Delta T)_1}{T_1 T_1'} \tan \alpha < \frac{(\Delta T)_2}{T_2 T_2'} \tan \beta \quad . \quad . \quad . \quad . \quad (3).$$

then A will be representative of the warm, and B of the cold-front types.

Consideration of the usual magnitudes of $(\Delta T)_1$, $(\Delta T)_2$, tan α and tan β has led the author to the conclusion that barograms of type B (Fig. 2) should be frequent with warm-front occlusions, whereas with the cold-front type little uniformity is likely to be found. In the latter case it is possible to have equality between the two parts of equation (2), and therefore no resultant change of pressure. In such cases bands of no barometric change may appear on the synoptic chart, but it should be remembered that barometric "tendencies" for a three-hour interval are not very suitable for exhibiting such effects.

The author then gives a short summary of an examination that he has made of synoptic charts in order to find illustrations of the theory outlined above. Contrary to expectation, he found many barograms that would have been more appropriate to depressions with warm sectors, for there was a steady fall of pressure, then a period of no change, and then a steady rise. He noted further that more cloud often occurred at the lower surface of separation (between "cold" and "coldest" air in Figs. 1 and 2) than near the "double front" higher up.

He concludes with a discussion of the problem of determining the slope of the discontinuities aloft from the pressure changes and a single set of values of height and temperature—a determination theoretically possible.

PHYSICS. By L. F. Bates, B.Sc., Ph.D., F.Inst.P., University College, London.

American Contributions.—In a paper on the diffraction of electrons by a single crystal of nickel Davisson and Germer (*Phys. Rev.*, **30**, p. 705, 1927) examined the scattering of homogeneous beams of electrons of known velocities incident normally upon a single crystal of nickel cut parallel to the 111 plane. They determined the distribution of the scattered electrons which had suffered little or no energy loss in the process. These electrons were scattered in all directions whatever the velocity with which they impinged upon the crystal, but in the neighbourhood of certain critical velocities, sets of three or six

sharply defined beams of electrons issued from the crystal in its principal azimuths. For a range of potential up to 370 volts thirty such beams were observed, and of these, six disappeared when prolonged heat treatment of the crystal caused the loss of gas absorbed upon its surface. The remaining twenty-four beams were therefore due to the crystal itself. The phenomenon may be regarded as the analogue of the regular reflection of X-rays by a crystal, for an incident beam of electrons with velocity v may be treated as a beam of waves of wave-length h/mv. Accordingly, a portion of this beam of waves is scattered from each of the layers of atoms lying parallel to the crystal face, the intensity of the scattered beam being a maximum when the beams reflected from the individual layers of atoms emerge from the crystal in phase. We should therefore expect the condition for maximum selective reflection to be given by the well-known Bragg formula, $n\lambda = 2 \cdot d \cdot \sin\theta$. Applying this conception to the scattering of electrons by the nickel crystal, it was found that of the twenty-four beams due to the crystal, twenty could be explained in this way, and three more could be explained by scattering at grazing incidence from a single layer of nickel atoms, whilst one beam, of low intensity, could not be accounted for. On the other hand, we should have expected eight more beams of low intensity, but these were not found. Moreover, the beams actually found did not coincide in position with those expected on the above theory, but behaved as if the grating constant was considerably less than its actual value, *i.e.* the values of h/mv were less than those given by the Bragg formula. In a further paper (*Proc. Nat. Acad. Sci. Amer.*, **14**, p. 317, 1928) the authors describe experiments in which a beam of electrons impinged on the crystal at various angles of incidence, and in which they measured the intensity of scattering in the plane of incidence as a function of the bombarding potential and the angle of incidence. They found that a sharply defined beam of scattered electrons issued from the crystal in the direction of regular reflection whenever the velocity of the incident electrons was comprised within certain ranges which depended upon the angle of incidence. For each of these ranges there is a definite velocity at which the intensity of the reflected beam is a maximum. Here, again, the wave-length h/mv of a reflected beam of maximum intensity was not that given by the Bragg formula. Eckart (*Proc. Nat. Acad. Sci. Amer.*, **13**, p. 460, 1927) has suggested that this discrepancy is perhaps due to the possession by the crystal of an index of refraction for electrons which differs appreciably from unity, but the available data is not sufficient to permit us to decide whether the phenomena may adequately be explained by the introduction of an index of refraction into

the Bragg formula. A more simple suggestion, however, may explain the phenomena. Bethe (*Naturwiss.*, **19**, p. 333, 1928) considers the X-ray whose wave-length, given by the Bragg formula, is nearest on the short wave side to that predicted on the above theory, instead of the X-ray whose wave-length is nearest on the long wave side, which was considered by Davisson and Germer, and he shows that if we assume that the electrons which enter the crystal are accelerated by a potential of about 15 volts by the internal field of the crystal, then we obtain good agreement between experiment and theory. Bethe shows, too, that since the electron wave-length inside the crystal is less than that outside, we should expect phenomena analogous to total internal reflection. The electron waves corresponding to the longest X-rays reflected from the 111 and 110 azimuths should not be observed experimentally, and the absence of certain beams of electrons which Davisson and Germer reported is thus explained.

English Contributions.—In an interesting study of the properties of single crystals of bismuth in magnetic fields Kapitza (*Proc. Roy. Soc.*, **119**, p. 358, 1928) found it necessary to examine in detail the mode of production of such single crystals. He found that a small strain set up in a bismuth rod during the growth of the crystal had a very pronounced effect on the orientation of the trigonal axis of the crystal. Bismuth crystallises in the hexagonal system and its crystal symmetry is that of the rhombohedron, which approaches very closely to a cube. The strain gave rise to a cleavage plane perpendicular to the rod, which was not the perfect cleavage plane, but one of the remaining three planes of the pseudo-octahedron, depending on the nature of the strain. In order to produce rods with the perfect cleavage plane orientated in any desired direction with respect to the axis of the rod, a method was devised in which no strain was set up in the rod during crystallisation. On investigating the resistance of these rods under pressure it was found that the specific resistance fell from about $1{\cdot}5 \times 10^{-4}$ ohm cm. quite rapidly with the applied pressure and reached a limiting value of $1{\cdot}39$ or $1{\cdot}40 \times 10^{-4}$ ohm cm. The specific resistance perpendicular to the trigonal axis was $1{\cdot}07 \times 10^{-4}$ ohm cm. The experiments suggested that the crystals contained cracks of appreciable resistance along their cleavage planes, and that these cracks were closed during compression, the limiting value of the specific resistance being reached when all the cracks had closed. The cracks were apparently produced during cooling at a temperature very close to the fusion point, and Kapitza suggests that they arose from the presence of two crystalline modifications of bismuth, of which only one kind is at present known to us. He suggests that the unknown

modification is probably cubic and changes to the familiar modification at a temperature just below the fusion point, the transition thus being accompanied by a change of shape which gives rise to cracks. This suggestion would explain the loss of the diamagnetic properties of bismuth just below the fusion point, observed by Curie, and the negative co-efficient of change of resistance under uniform pressure observed by Bridgman in this region. It would explain, too, the change of orientation of the perfect cleavage plane when slight strain is present during crystal growth, because the cubic modification might be transformed into the rhombohedral modification in four different ways, corresponding to the extension of the elementary cubes along the four possible diagonals. Thus a strain produced by external forces may play an exceedingly important part in in the orientation of the trigonal axis in the rhombohedral modification. In a further communication (*l.c.*, p. 400) Kapitza describes the method he developed for measuring the change of resistance of a bismuth crystal when it was placed in an intense magnetic field which existed for 1/100th of a second. The three main difficulties were the short time of duration the magnetic field, the production of induced *e.m.f.* by variation of the field, and the very low value of the resistance of the crystal. They were, however, partly compensated by the production of a large change in resistance by the intense field and the use of large currents of very short duration to give a reasonable value of the potential difference between the ends of the crystal, without heating the crystal. A known current was sent through the crystal and the potential difference between its ends was measured, both being recorded by oscillographs, whilst a third oscillograph measured the current in the coils producing the magnetic field. Measurements were made at the temperatures of the room, solid carbon dioxide and liquid air. The mounting of the crystal was very important because of the large forces acting upon it, and because of the possibility of induced currents. A special switch permitted the current to pass through the crystal for a very short time during the existence of the magnetic field; the timing of this switch was accurate to about 1/5000 second. The results showed that impurities and imperfections played a large part in the change of resistance particularly at low temperatures and with strong magnetic fields. When the current was perpendicular to the magnetic field an ideal crystal increased its resistance in accordance with a square law with low fields, but with strong fields it followed a linear law. The latter law held almost independently of the orientation of the crystal relative to the magnetic field. When the current was parallel to the magnetic field a saturation effect was found, the change of resistance in this case being very small

and not much affected by temperature, and very dependent on the imperfections in the crystal. The effect is probably due to the imperfections of the crystal and to imperfect alignment of the current in the crystal with the direction of the lines of magnetic force. It is interesting to view these facts in the light of Sommerfeld's recent theories of electrical conductivity. It is found that the discrepancy between experiment and theory is extremely large. In particular, no reason can be given for the proportionality of the change of resistance to the field, and not to the square of the field, with intense fields. It seems that we must assume that the magnetic field has a direct effect on the mechanism of collision between the electron and the atoms, and that the diamagnetism of bismuth is due to large electron orbits with a definite orientation in the crystal.

French Contributions.—Chatillon (*Ann. de Phys.*, **9**, p. 187, 1928) gives an account of some important researches on the different magnetic states of the Co^{++} ion. Cabrera and Trumpler had previously experimented independently on aqueous solutions of $CoCl_2$, and assuming that the Curie law $\chi \cdot T = C$ was satisfied, had found that the number of Weiss magnetons, p, based on observations at a single temperature, was 24 at infinite dilution but increased linearly with concentration to about 24·6 and then decreased as if the limiting value at infinite dilution was 25. Again, Trumpler with many solutions obtained a constant value of $p = 24·5$. Chatillon now shows that the above variation is due to the existence of an unstable state, since only once, and then by boiling on solution and on each successive dilution, was he able to obtain a similar curve. In all other cases, in spite of many varied treatments, he obtained a constant value of $p = 24·5$. If the Weiss law, $\chi_m (T - \theta) = C$ is assumed instead of the Curie law, p cannot be calculated from observations at a single temperature, but from a $(1/\chi, T)$ curve. Chatillon has investigated this curve in a large number of cases. For aqueous solutions of $CoCl_2$, $CoSO_4$ and $Co\,(No_3)_2$ of varied concentrations, the values of $1/\chi_m$ for the Co^{++} ion when plotted against T, fall accurately upon a straight line, from which $p = 25·05$ and $\theta = -12°$. Specimens of $CoCl_2$, received from Cabrera's laboratory, in aqueous solution at first showed a variation of χ_m with concentration, and a variation of $1/\chi$ with temperature which was not linear and not completely reversible. After five months the solutions showed none of these irregularities and gave $p = 25·02$ with $\theta = -11°$. Thus Chatillon shows that the only stable magnetic state for aqueous solutions of cobalt salts is one with a constant integral value of $p = 25$ and a value $\theta = -11°$.

Solutions of $CoCl_2$ in amyl alcohol gave $p = 23·04$ and $\theta = 0°$, and $p = 23·00$ and $\theta = 14°$ in ethyl alcohol. On evaporation

the residue from the amyl alcohol solution did not give a straight line for the $(1/\chi, T)$ curve, whilst that from the ethyl alcohol solution gave a straight line with $p = 26{\cdot}02$ and $\theta = 10°$. The latter solid, on solution in water, again gave $p = 25{\cdot}03$ and $\theta = -14°$. Ordinary crystalline $CoCl_2, 6H_2O$ gave $p = 25{\cdot}03$ and $\theta = -30°$ below 55° C., gave uncertain values from 55° C. to 110° C., and above 110° C., where it dissolved in its water of crystallisation, gave $p = 23{\cdot}95$ and $\theta = 13°$. After partial dehydration at 100° C., a straight line was obtained from 55° C. to 110° C. with $p = 25{\cdot}05$ and $\theta = 8°$. Anhydrous $CoSO_4$ showed interesting variations with conditions of preparation. After prolonged heating at dull red heat, the $(1/\chi, T)$ curve was reversible only above 400° C., where $p = 25{\cdot}96$ and $\theta = -55°$. If dehydrated below 400° C. the value $p = 25$ was found. Time effects were observed, and the temperature variation of one specimen only became constant after several heatings to 700° C., then giving $p = 25$. Another specimen was dissolved in boiling acid and the solution cooled to produce hexagonal crystals, which on calcination at dull red heat, gave a definite value $p = 25{\cdot}67$, which might be attributed to the constitution of the crystalline form. On recrystallisation and then dehydration at 400° C., $p = 25$ was obtained, but on heating to dull red the value became 25·67 instead of the usual value 26. This is an instance of what Chatillon terms "memory" in magnetic properties. It is clear that the crystalline form is of some importance.

Aqueous solutions of $CoCl_2$ diluted with hydrochloric acid gave results depending on the concentration of the acid. It appears that similar solutions do not always give the same result; the temperature variation is linear only over a limited range and magneton numbers of 22, 23, 23·5, 24, and 25 were found. Thus, in all, magneton numbers 22, 23, 24, 25, and 26 were found, together with numbers 23·5 and 25·67 under conditions which admitted the plausible explanation that they were due to mixtures. The variations of θ seemed to be quite independent of those of p. To the writer of this article it appears that many of the results above confirm the conclusion arrived at by Joos (*Ann. der Phys.*, **85**, p. 641, 1928), namely, that the magnetic carriers of colour and magnetic properties in aqueous solutions of paramagnetic salts are not simple ions.

German Contributions.—Honda has recently published (*Zeit. für Phys.*, **47**, 691, 1928) an interesting paper on the origin of magnetism. He reminds us that the optical electrons cannot behave as elementary magnets with respect to an external field, and that although they are presumably the cause of diamagnetic properties of atoms, they do not explain the occurrence of ferromagnetism or paramagnetism. He therefore ascribes the latter to the presence of rapidly rotating electrons inside the nucleus

of the atom, whose velocities are sufficiently great to account for elementary magnetic moments of the observed order of magnitude. Now these rotating electrons would possess such an enormous amount of angular momentum that no fields at our disposal would be capable of producing alignment of the magnetic particles. To overcome this difficulty Honda invokes the aid of Rutherford's conception of protons which rotate inside the nucleus, outside the electrons and in the opposite direction to them, so that the nett angular momentum possessed by the nucleus is quite small. Thus the nucleus of each element possesses a magnetic moment. The nucleus of a ferromagnetic atom possesses no angular momentum and may easily be acted upon by a magnetic field. In the case of a paramagnetic atom the angular momentum of the nucleus is small, whilst in the case of a diamagnetic atom it is extremely large. Moreover, the structure of a ferromagnetic substance is supposed to be such that the influence of atomic collisions at ordinary temperatures is very small, whilst that of paramagnetic and diamagnetic substances is such that the effects of atomic collisions are very great. Honda uses the familiar Langevin treatment to obtain an expression for the magnetic susceptibility of a paramagnetic solid. He denotes the energy of the atom corresponding to the angular momentum by Q, the kinetic energy due to the presence of neighbouring atoms in the space lattice by ϕ, and the energy of rotation of the atom about its magnetic axis by $s \cdot k \cdot \mathrm{T}$, where s is a small fraction. The paramagnetic susceptibility of the substance may then be written, neglecting for the moment any effects of diamagnetism, as

$$\kappa_p = \mathrm{L}_D^2/3\mathrm{N}(s \cdot k \cdot \mathrm{T} + \mathrm{Q} + \phi).$$

Here, in general, Q is very big and independent of T, whilst ϕ is either constant or decreases as T increases, so that we obtain the Weiss law, $\kappa_p \cdot (\mathrm{T} - \Delta) = \text{const.}$ It is easy to see on this theory that a diamagnetic compound may be formed from the combination of two paramagnetic atoms, and a paramagnetic combination from two diamagnetic atoms. For two paramagnetic atoms may so unite that the angular momentum of the combination is sufficiently great to make it appear diamagnetic. Similarly, two diamagnetic atoms, each with large angular momentum, may unite to form a system with such small angular momentum that it will appear paramagnetic. Again, a ferromagnetic substance may be formed of diamagnetic or paramagnetic constituents as long as the resultant angular momentum is very small. In the opinion of the writer of this article, there is a very grave difficulty which Honda does not appear to have considered, namely, that on this theory we

should expect no gyromagnetic phenomena. Yet the experiments of a large number of workers have shown conclusively that when a specimen is magnetised along a given axis, a definite amount of angular momentum is imparted to the specimen about that axis, and that this amount is exactly one half that expected on the theory that magnetic phenomena are due to the orientation of electron orbits.

The only well-established method for the measurement of the contact difference of potential between two metals is the separation method devised by Kelvin. There are many problems connected with contact potentials which await solution, and two new methods of measurement described by Mönch (*Zeit. für Phys.*, **47**, p. 522, 1928) are therefore of interest. In his first method a three-electrode tube was used, and he determined the potential, V_0, between the grid and the equipotential filament which was just sufficient to prevent electrons from reaching the grid. He then applied a potential $V_1 + V_0$ between the filament and grid, so that electrons with a velocity corresponding to V_1 passed through the grid. The potential, V_2, between the grid and plate was measured when electrons just failed to reach the plate. Consequently $V_2 - V_1$ was the contact difference of potential between the grid and plate. The apparatus was designed to permit thorough baking of all the component parts so that the contact potential difference was measured between metal surfaces which were free of adsorbed gas. The method, however, was not entirely satisfactory, *e.g.*, the value of the contact difference of potential between copper and nickel varied between 0·2 and 0·52 volt in one set of measurements. The second method was much more satisfactory. In this method the electrons passed through a grid and were collected by a plate, and in their passage they were under the influence of the contact difference of potential (A – X) between the metals of the grid and plate. The characteristic curve was obtained in the usual manner. The plate was then replaced by another of the same dimensions but of different metal, so that the contact difference of potential between the grid and plate was now (B – X). Hence, with respect to the first plate the electrons were accelerated or retarded by a potential (A – B). Now the effect on the velocity of the electrons should be the same at every point on the characteristic, and hence the curves obtained with the first and second plates should be displaced parallel to one another by an amount (A – B). In the apparatus used, a platinum filament was surrounded by a grid consisting of a cylindrical sheet of copper in which was cut a fine slit round the middle. The grid could be surrounded by a copper cylinder as a standard anode, or by a cylinder made of two metals sweated together

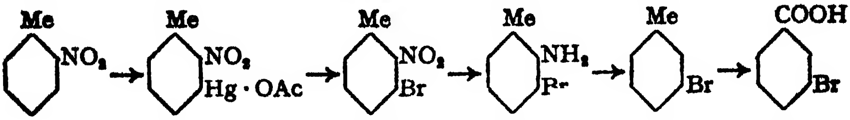
end to end. The metals nickel, iron, copper, platinum, and gold were used. An electric oven was provided for the proper heating of all the component parts. The displaced curves obtained in the early experiments with the apparatus were of different slope, but by careful manipulation of the plates, curves were obtained in which the slopes of the linear portions were the same. The sensitivity was then about 0·01 volt. It was found that the effect of prolonged heating of the plates *in vacuo* was to make copper more electropositive and nickel more electronegative, and that after remaining *in vacuo* for a considerable period the metals tended to recover their original potentials. Of the metals which were investigated after prolonged heating *in vacuo*, platinum was found to be the most electropositive and nickel the most electronegative. The most striking result was the change which occurred in the voltaic series when hydrogen was admitted to the freshly heated metals. The order was initially Pt, Au, Cu, Fe, and Ni, but after the admission of hydrogen it was Au, Pt, Ni, Cu, and Fe. The order when the cold metals were first placed inside the apparatus could not be obtained, owing to the impossibility of sealing off the apparatus without affecting the metals inside it.

ORGANIC CHEMISTRY. By J. N. E. DAY, M.Sc., A.I.C., University College, London.

As a result of work on the mercuration of organic substances, it has now been shown that the mercury may enter the *meta* position to an orienting group. This result is of interest in view of its bearing on the question of aromatic substitution.

In this connection reference may be made to three papers by Coffey, on the mercuration of aromatic substances (*J.C.S.*, 1925, **127**, 1029; 1926, 637; 1926, 3215). While, in the case of toluene, previous workers had not found any of the *meta* derivative, Coffey gives the result as $o : m : p = 43 : 13 : 44$. The method employed was to convert the monomercuri-compounds, obtained by boiling toluene with mercuric acetate, to a mixture of the corresponding bromo-toluenes. It was shown by thermal analysis that this mixture contained a quantity of the *meta* compound. The bromo-toluenes were oxidised to bromo-benzoic acids, which were separated by crystallisation of the free acids and their barium salts. The mercuration of *o*-nitrotoluene led, in addition to the isolation of the 1 : 2 : 4- and 1 : 2 : 6-compounds, to definite proof being obtained of the presence of some 1 : 2 : 3- or 1 : 2 : 5-derivatives

by the identification of the *m*-bromo-toluene obtained from the bromo-toluidine by removal of the amino group.



Burton, Hammond, and Kenner (*J.C.S.*, 1926, 1802) describe the preparation of 3-bromo-2-nitrotoluene and 4-bromo-2-nitrotoluene, by treatment of *o*-nitrotoluene with mercuric acetate and conversion of the mercury compounds to the bromo-compounds.

In the case of *p*-nitrotoluene, mercuration gave a very large proportion of the 3-compound. The mixture was converted to the corresponding bromo-compounds and by thermal analysis was found to contain 22·5 per cent. of the 2-bromo-4-nitrotoluene and 77·5 per cent. of the 3-bromo-4-nitrotoluene. With *m*-nitrotoluene, 4- and 5-chloromercuri-3-nitrotoluenes were obtained, and the presence of 6-chloromercuri-3-nitrotoluene was shown by the preparation of 6-bromo-*m*-toluidine.

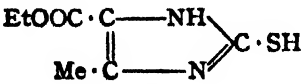
The mercuration of nitrobenzene also shows a large proportion of the *m*-derivative; thus Jürgens (*Rec. trav. chim.*, 1926, **45**, 61) obtained the values $o : m : p = 52{\cdot}6 : 38{\cdot}5 : 8{\cdot}9$.

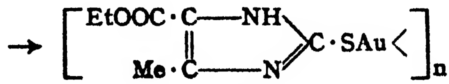
Henry and Sharp (*J.C.S.*, 1926, 2432) find that *p-tert.*-butylphenol and *p-iso*-amylphenol give both mono- and di-substituted derivatives; 2-acetoxymercuri-*p-tert.*-butylphenol and 2 : 6-diacetoxymercuri-*p-tert.*-butylphenol together with the corresponding *iso*-amyl compounds, being obtained. 2-Hydroxy-5-*tert.*-butylbenzaldehyde and 2-hydroxy-5-*iso*-amylbenzaldehyde both gave the 3-acetoxymercuri compound.

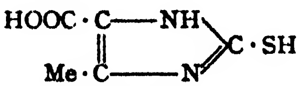
Balaban and King (*J.C.S.*, 1927, 1858) describe the preparation of gold and mercury derivatives of 2-thiolglyoxalines. When ethyl 2-thiol-4 (or 5)-methylglyoxaline-5 (or 4)-carboxylate [I] was treated in methyl alcohol solution with auric chloride, it gave ethyl 2-aurothiol-4 (or 5)-methylglyoxaline-5 (or 4)-carboxylate [II]. This ester cannot be hydrolysed without decomposition; the gold compound of the free acid [III] was therefore obtained by hydrolysing [I] to 2-thiol-4 (or 5)-methylglyoxaline-5 (or 4)-carboxylic acid [III], which in methyl alcohol with auric chloride gave 2-aurothiol-4-(or 5)-methylglyoxaline-5 (or 4)-carboxylic acid [IV]. The acid [IV] was always found to contain excess gold N : Au = 2 : 1·08. It is suggested that this result is due to the presence of [V], analogous to the arsenic compound [VI].

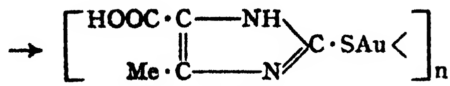
The behaviour of these compounds with mercury salts is interesting. Ethyl glyoxaline-4 (or 5)-carboxylate with mercuric acetate gave ethyl 1-acetoxymercuriglyoxaline-5 (or 4)-

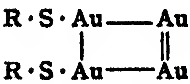
carboxylate [VII], while, on the other hand, ethyl 2-thiol-4 (or 5)-methylglyoxaline-5 (or 4)-carboxylate, and the corresponding free carboxylic acid, gave ethyl 2-chloromercurithiol-4 (or 5)-methylglyoxaline-5 (or 4)-carboxylate [VIII], and 2-chloromercurithiol-4 (or 5)-methylglyoxaline-5 (or 4)-carboxylic acid. These different constitutions are assigned to these compounds owing to their difference of behaviour with sodium hydroxide.

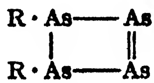

[I]

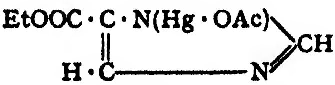

[II]

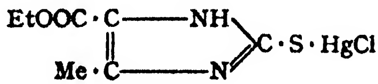

[III]


[IV]


[V]


[VI]

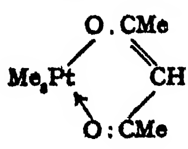

[VII]


[VIII]

Goddard and Yarsley (*J.C.S.*, 1928, 719) state that tri-*p*-tolylstibine on nitration with cold fuming nitric acid gave tri-*m*-nitrotri-*p*-tolylstibine dinitrate. The position of the entering nitro group was found by the preparation of 4-bromo-2-nitrotoluene. Goddard (*J.C.S.*, 1923, **123**, 2315) found that tri-*m*-xylylstibine on nitration gave tri-5-nitrotri-*m*-xylylstibine dinitrate, 4-bromo-6-nitro-*m*-xylene being obtained from it.

Menzies (*J.C.S.*, 1928, 565) describes a volatile platinum compound, trimethylplatinum acetylacetone [IX], which was prepared by treating trimethylplatinic iodide with thallous acetylacetone. It was found that when this compound was heated it sublimed, and that a platinum mirror could be obtained by decomposing the sublimate by bringing it into contact with a hot glass surface.

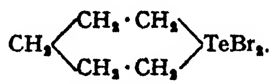
[IX]

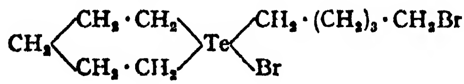


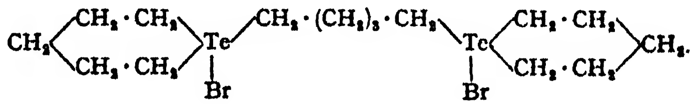
Morgan and Burgess (*J.C.S.*, 1928, 321) describe the preparation of *cyclo*telluropentane. The compound cannot be prepared directly according to the equation :

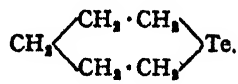
$$Al_2Te_3 + 3CH_2Br \cdot (CH_2)_3 \cdot CH_2Br = 3C_5H_{10} : Te + 2AlBr_3,$$

as it combines with the dihalide to form two additive compounds. When aluminium telluride and pentamethylene dibromide were heated, 1-ε-bromoamyl*cyclo*telluripentane-1-bromide [XI], pentamethylene-αε-bis*cyclo*telluripentane-1-1′-dibromide [XII], and *cyclo*telluripentane-1-1-dibromide [X], were obtained.

[X] 

[XI] 

[XII] 

[XIII] 

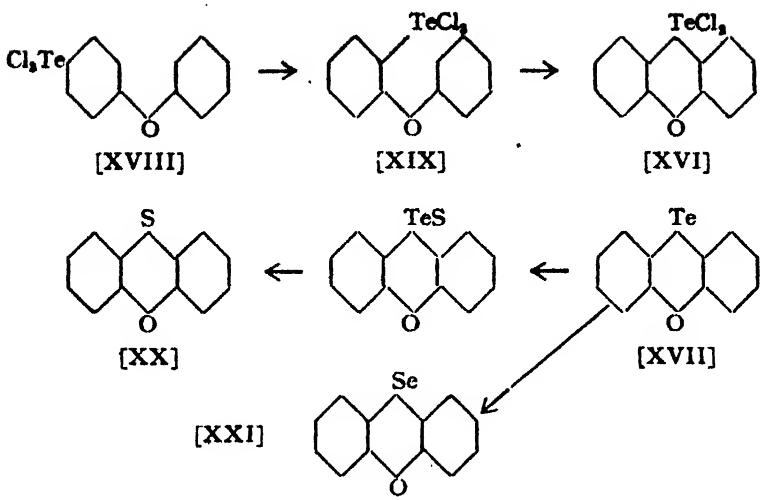
The *cyclo*telluropentane [XIII] was prepared by reduction of the dibromide [X] with metabisulphite. The compound [X] was also prepared from [XI] and [XII] by heating under reduced pressure and treating the distillate with excess bromine.

Drew (*J.C.S.*, 1926, 223) found that tellurium tetrachloride and diphenyl ether reacted together to give *p*-phenoxyphenyltelluritrichloride [XIV] ; on treatment with potassium metabisulphite this gave *pp*′-diphenoxydiphenyl ditelluride [XV]. When [XIV] was heated above 200° it lost hydrogen chloride, and gave a cyclic compound, 10 : 10-dichlorophenoxtellurine [XVI]. This is readily reduced to phenoxtellurine [XVII]. It is suggested that, in this reaction, as the -$TeCl_3$ group probably takes the *para* position, that the first change is an isomeric one into the *ortho* compound: [XVIII] → [XIX] → [XVI].

The same author (*J.C.S.*, 1928, 511) gives an improved method for the preparation of phenoxthionine [XX] by heating phenoxtellurine with sulphur, tellurium being set free. By using selenium in place of sulphur he obtained phenoxselenine [XXI].

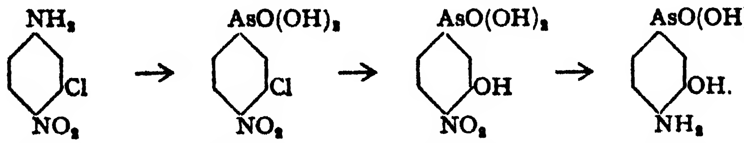
[XIV] $C_6H_5 \cdot O \cdot C_6H_4 \cdot TeCl_3$

[XV] $C_6H_5 \cdot O \cdot C_6H_4 \cdot Te \quad Te \cdot C_6H_4 \cdot O \cdot C_6H_5 \cdot$



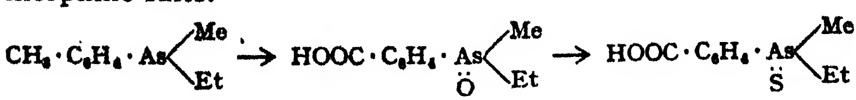
[XVIII] [XIX] [XVI] [XX] [XVII] [XXI]

Balaban (*J.C.S.*, 1928, 809) gives a new synthesis of 4-amino-3-hydroxyphenylarsenic acid; *m*-chloroacetanilide, on nitration gave a mixture of 4-nitro-chloroacetanilide and 6-nitro-chloroacetanilide; these were separated after hydrolysis. The 3-chloro-4-nitroaniline [XXII], when diazotised and treated with copper arsenite (Lewis and Cheetham, *J.A.C.S.*, 1921, **43**, 2119) gave 3-chloro-4-nitrophenylarsenic acid [XXIII]. This was converted to the 3-hydroxy compound [XXIV], which, on reduction with glucose, gave 4-amino-3-hydroxyphenylarsenic acid [XXV].



[XXII] [XXIII] [XXIV] [XXV]

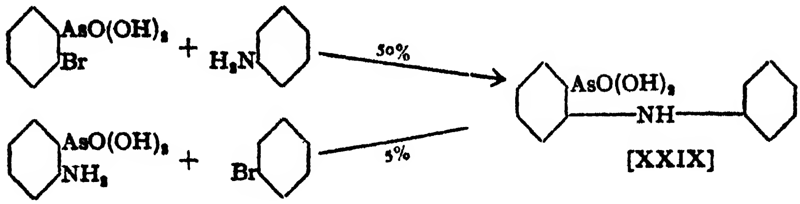
The resolution of an arsenic compound has been carried out by Mills and Raper (*J.C.S.*, 1925, 2479). *p*-Tolylmethylethylarsine [XXVI] was oxidised to *p*-carboxyphenylmethylethylarsine oxide [XXVII] which, on treatment with hydrogen sulphide, gave *p*-carboxyphenylmethylethylarsine sulphide [XXVIII]. This was resolved by means of its brucine and morphine salts.



[XXVI] [XXVII] [XXVIII]

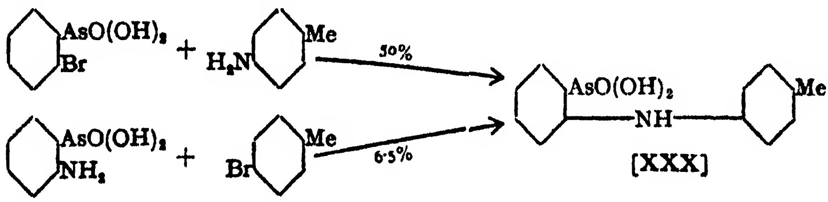
Diphenylamine-*o*-arsenic acid [XXIX] has been prepared in two ways (Gibson and Johnson, *J.C.S.*, 1927, 2499), (*a*)

starting with *o*-bromophenylarsinic acid, (*b*) starting with *o*-aminophenylarsenic acid.



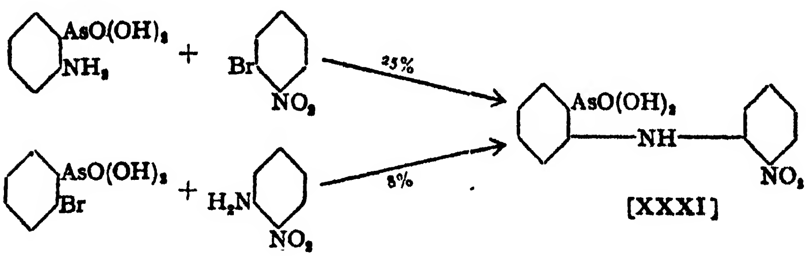
[XXIX]

In a similar way, 4-methyldiphenylamine-6′-arsinic acid [XXX] was prepared. In this case also the higher yield was obtained with the *o*-bromophenylarsinic acid.



[XXX]

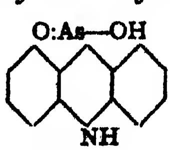
In the case of 2-nitrodiphenylamine -6′-arsinic acid [XXXI], however, the higher yield was obtained with *o*-aminophenylarsinic acid.



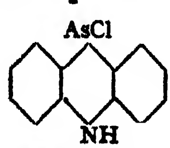
[XXXI]

It would seem, therefore, that the factor governing the reaction depends on the reactivity of the bromine atom, which is in the order: *o*-bromonitrobenzene, *o*-bromophenylarsinic acid, bromobenzene.

These acids can be converted into the heterocylic arsenic compounds; thus [XXIX] gives phenarsazinic acid [XXXII] and 10-chloro-5 : 10-dihydrophenarsazine [XXXIII].



[XXXII]



[XXXIII]

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., Ph.D., University, Glasgow.

Regional and Stratigraphical Geology.—In the first volume of his Geology of Europe (*Geologie von Europa*, Band 1, 1926, 321 pp.), which is part of a series on the Geology of the Earth, S. von Bubnoff gives a general synopsis of the stratigraphy and structure of the continent, followed by the detailed geology of Eastern Europe. Volume 2 is to deal with Western Europe excluding the Alpine System, and volume 3 will be concerned with the Alpine System and the Tertiary Mediterranean. The work is clearly of great importance and significance.

The pamphlet entitled "The Geological Structure of the Union," by Dr. A. W. Rogers (Union of South Africa: *Geol. Surv.*, 1925, pp. 34), is an explanation of the geological map of the Union (scale 1 : 1,000,000), and forms a most valuable compact summary of South African stratigraphy and geological structure.

A stratigraphical work of extreme importance is Dr. A. L. du Toit's memoir, "A Geological Comparison of South America with South Africa" (*Carn. Inst., Washington*, Publ. No. 381, 1927, 158 pp.), in which he gives the results of an extensive geological journey in South America. Throughout the work "the intention has been to detail the geological peculiarities of the opposed sections of the two land masses, to point out their resemblances and differences, and to draw such legitimate conclusions in regard to the geography, climatic, and other circumstances in each country, as the facts would appear to the writer to warrant." Dr. du Toit affirms that the union of the continents in former geological times is indicated by a wealth of practically indisputable evidence, but he does not favour the idea of one or more relatively narrow connecting links or land bridges across the Atlantic, lasting down to the early Mesozoic. On the contrary, he says the evidence strongly favours the view that South America and South Africa were geographically appreciably closer together in the past, and therefore supports the continental displacement theory of Wegener. He formulates seven points in which this view simplifies geological problems. It is possible to quote only one of these points: "It (the theory) would explain Gondwanaland as a well-defined and almost isolated unit down to about the end of the Jurassic epoch, after which the continental mass is viewed as proceeding to break up, and the sectors to begin drifting apart, with the Cretaceous ocean progressively penetrating between the blocks, and generally coming to overlap their borders in the fully established marine transgressions of the late Mesozoic."

The first part of the great monograph on the Sulitelma

mining region of Norway by Dr. Th. Vogt has just appeared, dealing with the physiography, general geology, and petrology of the district ("Sulitelmafeltet Geologi og Petrografi," *Norske Geol. Undersök.*, No. 121, 1927, pp. xii, 1–447; English summary, pp. 449–560). Part II is to describe the ore deposits. The part now issued contains a generous English summary of 111 pages, and we are thus able to assess the very important and significant contribution to stratigraphy, igneous geology, and the petrology of igneous and metamorphic rocks, that Dr. Vogt has made. The igneous geology and petrology will be commented upon at the appropriate time, leaving the general geology to be dealt with in this place. The sedimentary rocks of Sulitelma belong to the upper part of the Cambro-Silurian which builds up the Scandinavian mountain range. A great series of mica-schists, the Seve Group, which occurs below the main limestone horizon, has been regarded as Pre-Cambrian, but Vogt and certain Swedish geologists now consider it to be Cambro-Silurian. The upper part of the succession has been metamorphosed to the chlorite-muscovite schist stage, but nevertheless retains a fossiliferous horizon of crinoids and bryozoa. Spilitic lavas are interbedded with this series, and the whole mass has been invaded by huge phacoliths of gabbro, from the study of which some new points in igneous geology have been elicited. The whole work is deserving of the closest study by systematic geologists.

The Geologists Association have recently published two useful papers describing the regional geology of the Edinburgh and Dolgelly districts (Sir J. S. Flett, *et alia*, "The Geology of the District around Edinburgh," *Proc. Geol. Assoc.*, xxxviii, 1927, pp. 405–517; A. H. Cox and A. K. Wells, "The Geology of the Dolgelly District, Merionethshire," *ibid.*, pp. 265–331).

The general geology and physiography of Morvern (Argyll) has been very fully described by Dr. J. F. Scott (*Trans. Geol. Soc. Glasgow*, xviii, pt. 2, 1928, pp. 149–89). Morvern consists of a Pre-Palæozoic foundation consisting of Moine Gneiss, which is intruded by the Morvern or Strontian Granite presumably of Lower Old Red Sandstone age, and covered in places by sediments varying in age from Carboniferous to Upper Cretaceous. These strata have been preserved from denudation by a capping of Kainozoic plateau basalts. The stratigraphy of these formations is dealt with in detail, and, in addition, Dr. Scott has sections on the tectonics and physiography of this interesting and little-known area of the Highlands.

Prof. H. H. Swinnerton has critically examined the old generalisation that periods of violent tectonic movements have a causal connection with the rise and fall of certain

groups of organisms (" Earth Movements and Extinction," *Geol. Mag.*, lxiv, 1927, pp. 529–32). He shows that such connections cannot be demonstrated with regard to trilobites, ammonites, reptiles, and mammals, and questions the general application of the principle.

In his important paper on " The Problem of the Pre-Cambrian Atmosphere " (*South African Journ. Sci.*, xxiv, 1927, pp. 155–72) Mr. A. M. Macgregor suggests that the Pre-Cambrian atmosphere was deficient in oxygen and was otherwise radically different from that of the present day. The extent to which silicate minerals in early Pre-Cambrian rocks have been replaced by carbonates, probably soon after their formation, is taken to indicate a higher proportion of CO_2 in the atmosphere than now exists. Similarly Rhodesian Pre-Cambrian sediments are shown to be characteristically rich in the lower oxide of iron and poor in the higher oxide. It is suggested that the problems connected with the origin of the Pre-Cambrian banded ironstones, which are of world-wide distribution, are explicable on the assumption that the atmosphere of that time lacked oxygen and was rich in CO_2.

In their valuable discussion of the major stratigraphical terms of the Pre-Palæozoic, Prof. J. W. Gregory and Mr. B. H. Barrett (*Journ. Geol.*, xxxv, 1927, pp. 734–42) show how much the Pre-Cambrian problem is hampered by uncertainty as to the use and scope of the nomenclature. " Proterozoic," for example, is used in three different senses: for the whole of the Pre-Palæozoic, for the upper part of the Pre-Palæozoic, and, in Britain, for the Lower Palæozoic (Lapworth). It is suggested that appropriate names for the successive stages in the development of life through the dawn of life, archaic life, ancient life, middle life, to recent life, would be Eozoic, Archeozoic, Palæozoic, Mesozoic, and Kainozoic.

The fourth instalment of the work by Prof. T. J. Jehu and Mr. R. M. Craig on the geology of the Outer Hebrides (Part IV, South Harris. *Trans. Roy. Soc. Edin.*, lv, Part 2, 1927, pp. 457–88) deals with South Harris. Practically the whole area consists of the Lewisian Gneiss; but the authors have brought to light a remarkable mass of anorthosite-gneiss, which is associated with a gabbro-diorite intrusion, garnet-amphibolite, and eclogite. Anorthosites are rare in Great Britain, only one other example, at Portsoy in Banffshire, being known.

In his paper entitled " Some Geological Notes on Three Districts of Northern Scotland " Mr. A. G. Steavenson (*Trans. Geol. Soc., Glasgow*, xviii, pt. 1, 1928, pp. 193–233) describes some foliated granites in Western Lewis; the Stromness (Orkney) granite-gneiss inliers (of Moine Gneiss age), and the surrounding rocks (Middle Old Red Sandstone); and the

"Torridonian" outlier of Stornoway (Lewis). The red conglomerates of the last-named district have been assigned by different observers to the Pre-Cambrian, Torridonian, Cambrian, Old Red Sandstone, and Triassic. From the scanty available evidence Mr. Steavenson concludes that it probably belongs to the lowest group (Diabaig) of the Torridonian.

The unending puzzle of Grampian stratigraphy and structure has again been attacked by three investigators, working in three different regions. Mr. W. J. McCallien, writing on the geology of Gigha, an island between North Kintyre and Islay, shows that the island consists of quartzites and phyllites, probably belonging to the Erins Quartzite Group of the mainland. These sedimentary schists, however, are quite subordinate in amount to intrusive epidiorites. The close approach of the Gigha Schists to the Kintyre or Cowal sequence, and the cutting-off of the Erins Quartzite to the north-east, are best explained by assuming the reality of the thrust postulated by Mr. E. B. Bailey at the base of the Loch Awe Nappe.

Mr. E. B. Bailey continues his investigations into the structure of the Central Grampians, now advancing into the Braemar, Glen Clunie, and Glen Shee region (*Trans. Roy. Soc. Edin.*, lv, pt. 3, 1928, pp. 737–54). Much of Mr. G. Barrow's previous work in this region is confirmed. The results of the work are set out in sections and a geological map. It is believed that the dark schist of Glen Clunie is better grouped with the Ben Eagach Schist than with the Blair Atholl Series. The Cairnwell fold in the latter is probably synclinal. The Perthshire Quartzite Series, which contains an important mica-schist group (Killiekrankie) in Perthshire, is represented in the region dealt with entirely by quartzite.

Dr. H. H. Read's present contribution to Grampian geology deals with the region of Middle Deeside and East Glen Muick (*Trans. Roy. Soc. Edin.*, lv, pt. 3, 1928, pp. 755–72). The schists are here subjected to a triple subdivision: (1) Glen Tanner Group; (2) Deeside Limestone; (3) Queen's Hill Group; which are respectively correlated with the Pitlochry Schists, Loch Tay Limestone, and Ben Lui Schists, of Perthshire. There is a culmination of pitch in Tarfside which has been recognised by Mr. E. B. Bailey. Both the Loch Tay Limestone and the Deeside Limestone lie structurally far above this culmination. The correlation of the Deeside and Loch Tay Limestone is a most important step, as a single horizon has now been traced from the southern extremity of Kintyre to the centre of Aberdeenshire. This will provide a foundation on which to build the structure of a large part of the north-eastern Grampians.

Mr. E. S. Cobbold records in detail the results of his study

of the natural, and the many artificial, exposures in the Cambrian area of Comley (Shropshire) (*Quart. Journ. Geol. Soc.*, lxxxiii, 1927, pp. 551–73). The most important achievement, according to Prof. W. W. Watts, is the detection of an unconformity in the middle of the Cambrian, and its correlation with the lithology of the formation.

Mr. H. Williams's paper on the geology of Snowdon (North Wales) (*Quart. Journ. Geol. Soc.*, lxxxiii, 1927, pp. 346–431) contains valuable contributions to stratigraphy, igneous geology, and petrology. He describes in detail the Upper Cambrian, Arenig, Llanvirn, Lower Llandeilo, and Lower Caradoc sediments and volcanic rocks which constitute the mountain, together with acid and basic intrusive rocks. The latter are regarded as consanguineous, and practically contemporaneous, with the lavas. "The dominant folding is of N.E.–S.W. strike, and occurred almost wholly after the period of doleritic intrusions. It was succeeded, first by a cleavage of similar strike, and subsequently by a minor folding and thrusting due to pressure directed to the S.E. Finally, the area was dissected by faults, most of which belong to the same family of movements as those just enumerated, though some may have moved again during the Tertiary Era."

The Silurian inlier of Woolhope (Herefordshire) is described in detail by C. I. Gardiner (*Quart. Journ. Geol. Sci.*, lxxxiii, 1927, pp. 501–30). The area discloses beds ranging from Downtonian to the Llandovery Sandstone. Denudation has not yet uncovered any coarse conglomerates corresponding to those seen at May Hill, a few miles to the S.S.E. The detailed mapping shows that the usual description of this area as a dome requires alteration. The rocks have been bent up by pressure from two directions, N.W.–S.E. (Charnian), and N.N.E.–S.S.W., and relief has been obtained by faulting which, from the map, appears to be disposed in radial fashion. One fault has passed right through the area, and has profoundly modified the arrangement of the rocks.

Prof. P. G. H. Boswell deals with the Salopian Rocks and Tectonics of the district south-west of Ruthin (Denbighshire) (*Quart. Journ. Geol. Soc.*, lxxxiii, pt. 5, 1928, pp. 689–710). The region yields a succession of the Denbigh Grits with six graptolite zones ranging from *Cyrtograptus rigidus* to *Monograptus leintwardinensis*. Ribbon-banded mudstones and silt-stones in the lower zones, and sandstones interbedded with cleaved shales in the upper parts, constitute the lithology of the series. Folding is but little in evidence, but the district is heavily faulted, the major fractures running N.E. or N.N.E. The folding, cleavage, and dominant faulting are attributed to movements of Caledonian age.

According to L. G. Anniss (*Quart. Journ. Geol. Soc.*, lxxxiii, 1927, pp. 492–500), the Upper Devonian of Saltern Cove, Torbay, is represented by 500 feet of shales, thin limestones, and tuffs, mainly belonging to the Frasnian. The position of the overlying massive limestones (Lower Frasnian ?) is due to overthrust, probably from the S.E. The tuffs and an intrusion of albitised dolerite show through a " window " eroded in this overthrust sheet. Species of Goniatites from the Iγ zone of Wedekind (Frasnian) are recorded here for the first time in Great Britain.

Dr. C. F. Kolderup concludes his study of the Devonian fields of Western Norway with descriptions of the largest (Hornelens devonfelt, *Bergens Mus. Aarbok. Naturvid. Raekke*, No. 6, 1926, 56 pp.), and of one of the smallest areas (Byrknesøyenes og Holmengraas devonfelter, *ibid.*, No. 8, 1926, 18 pp.) of these rocks. The Hornelen region, 1,080 sq. kms. in area, consists of an enormous mass of strata 20,000 to 25,000 m. in thickness, which, from the fossil fishes and plants, belong to the upper part of the Orcadian series of the Old Red Sandstone. Breccias and conglomerates from the lower part of the succession are followed by green and red sandstones. There has been a subsidence of the basin along the northern side of the area, and at one place on the eastern margin the junction is formed by a thrust plane. The Devonian rocks of the small islands described in the second paper are identified by their lithology and by their relations to the Ordovician-Silurian basement rocks.

Mr. W. J. McCallien's lengthy paper on the Post-Dalradian Geology of Kintyre (*Trans. Geol. Soc. Glasgow*, xviii, pt. 1, 1927, pp. 40–126) deals with the Old Red Sandstone and Carboniferous formations of a little-known region. Lavas of Old Red Sandstone age are absent, but there are volcanic vents, and beds of agglomerate and volcanic conglomerate, of this age. The sediments of the Kintyre Old Red Sandstone are regarded as of dominantly fluviatile origin. Lower Carboniferous lavas of various types fill up basin-shaped hollows in the Old Red Sandstone and metamorphic rocks. Intrusions of the same age belong mainly to a keratophyre-bostonite series. Kainozoic igneous rocks are scarce, and consist of dykes of olivine-dolerite, crinanite, and monchiquite.

In his paper on " Post-Carboniferous Movements in the Northumbrian Fault Block," Mr. H. C. Versey (*Proc. Yorks. Geol. Soc.*, xxi, 1927, pp. 1–16 reprint) deals only with movements belonging to the Hercynian orogeny, and attempts to show their relation to contemporaneous igneous activity, and to the deposition of Permian strata. The Northumbrian Fault Block is considered to represent a fractured *pli de fond*, the

fracture lines being related to areas of varying thicknesses in the Carboniferous rocks. The Whin Sill is considered to be a result of the block-faulting, and as of phacolithic form beneath the Teesdale anticline.

Part II of Dr. R. L. Sherlock's memoir on the correlation of the British Permo-Triassic rocks, dealing with England south of the Pennines and Wales, has now appeared (*Proc. Geol. Assoc.*, xxxix, 1928, pp. 49–95). The chief conclusion drawn from his penetrating analysis of the British evidence is that the " Permian " system has no real existence in this country. According to Dr. Sherlock, the " Permian " has also broken down in Germany and Western Europe generally ; and he even gives a destructive criticism of Murchison's type Permian of Russia. The question thus arises as to a suitable name for the rocks which intervene between the Carboniferous and the Rhætic. The terms " New Red Sandstone," " Poikilitic," " Dyas," and " Trias," are successively rejected for the reason that not one of them is everywhere applicable. The term Epiric System, meaning continental, is suggested, as the outstanding feature of the period was the great spread of the continents. The lower boundary of this system would be the base of the Zechstein or Magnesian Limestone Series which in Britain is everywhere unconformable to the beds below ; the upper boundary would be drawn at the top of the Keuper Series, as the Rhætic transgression which follows provides the natural beginning of a new period in Western Europe.

In a paper on " Contemporaneous Volcanic Activity in East Fife " Mr. D. Balsillie (*Geol. Mag.*, lxiv, 1927, pp. 481–93) presents further evidence in support of his view of contemporaneous volcanic activity amongst the Lower Carboniferous rocks, and opposed to the assumption of Permian age for the old volcanoes of East Fife.

Mr. W. J. Arkell's long and important memoir on the Corallian Rocks of Oxford, Berks, and North Wilts (*Phil. Trans.*, Ser. B, vol. 216, 1927, pp. 67–181) is a very exhaustive treatment of the lithology, stratigraphy, palæontology, and palæogeography of the Corallian epoch within the above-mentioned areal limits. He regards the formation as comprised of five divisions : (1) Lower Calcareous Grit ; (2) Berkshire Oolite Series ; (3) Osmington Oolite Series ; (4) Trigonia clavellata Beds (which are, however, only doubtfully represented in this region) ; and (5) Upper Calcareous Grit. The very rapid lithological and palæontological variations which characterise this formation are illustrated by sections and a correlation table. According to Mr. Arkell's interpretation of the geography of the epoch, the thin strip of Corallian rocks extending through Berkshire and Wiltshire must be regarded

as a coastal deposit fringing the north-western shore of the Palæozoic land mass from the denudation of which its materials were derived.

A paper by Dr. J. A. Douglas and Mr. W. J. Arkell (*Quart. Journ. Geol. Soc.*, lxxxiv, pt. 1, 1928, pp. 117–78) gives an account of the stratigraphical distribution of the Cornbrash in the south-west of England from Oxford to near Weymouth. The authors advocate and utilise a twofold subdivision of the formation rather than the threefold subdivision suggested by Mr. Buckman. By the detailed account of many typical exposures a general description of the faunal sequence and zonal distribution is given throughout the area. Criticism is made of Mr. Buckman's conclusions regarding the palæogeography and zoning of the formation.

G. C. Martin deals exhaustively with the Mesozoic Stratigraphy of Alaska in *Bull. 776 of the U.S. Geological Survey* (1926, 493 pp.).

The Cretaceous shales of Jamaica, generally occurring associated with, and below, Rudist Limestones, are described by Dr. C. T. Trechmann with a wealth of palæontological detail (*Geol. Mag.*, lxiv, 1927, pp. 27–42; 49–65). The fossils described show that the Jamaican beds occur at a very high horizon in the Cretaceous, Upper Senonian, if not Maestrichtian.

Prof. V. C. Illing describes the geology of the Naparima Region of Trinidad (*Quart. Journ. Geol. Soc.*, lxxxiv, pt. 1, 1928, pp. 1–56). The formations constitute a broken succession from the Lower Cretaceous to the present time, the main breaks appearing to be in the Lower Eocene and Oligocene. The rocks are folded along an E.N.E.–W.S.W. direction, and the folding diminishes in intensity southwards. In the north thrusting is the dominant tectonic feature. The folding occurred intermittently in the Tertiary, although the culminating movement was probably not earlier than the Upper Miocene.

ZOOLOGY. By F. W. Rogers Brambell, Ph.D., D.Sc., Lecturer in Zoology, King's College, London.

Genetics.—The problems connected with the origin of vestigial structures in animals have been recognised for a long time to be of great interest from the evolutionary point of view. The vestigial wings of many species of insects afford good examples of such structures. They occur in many orders of insects which are normally winged. In some cases, such as the female Vapourer Moth, they are confined to one sex. In other cases they are not confined to one sex, but occur in both. Some species include forms with normal wings and forms with

reduced wings. Miss Jackson, in a recent paper (*Trans. R.S. Edin.*, vol. 55, 1928), has described a number of these in weevils. She succeeded in breeding and rearing the weevil *Sitona hispidula*, and effected crosses between the long and short winged forms. It was possible to show from these that the brachypterous condition behaves as a simple Mendelian dominant. It is shown that the reduction of the wings of beetles is only one of a series of conditions frequently found associated together, including atrophy of the wing muscles, ankylosis of the elytra, etc. It is suggested that such changes must be attributed either to a regressive orthogenesis or to a succession of chance mutations. The latter theory would be compatible with the conditions in Sitona where the reduction of the wings and their muscles is often correlated. It is pointed out that once the wing muscles were suppressed and flight consequently rendered impossible, further mutations resulting in reduction of the wings and ankylosis of the elytra could not be harmful to the species. Owing to the frequent occurrence of the apterous condition in beetles in all sorts of environments it appears probable that the capacity or incapacity for flight is of little importance in determining the survival of most species.

Chromosomes.—Several papers have appeared recently dealing with the chromosomes, and especially with the sex-chromosomes during spermatogenesis.

A great deal of confusion has arisen concerning the chromosomes in the various species and races of Gryllotalpa. Authors have disagreed as to the number of chromosomes, and the arrangement of the sex-chromosomes. De Winiwarter (*Arch. de Biol.*, t. 37, 1927) has done much to clear up the results, and has shown that many of the discrepancies between previous accounts have been due, in all probability, to the use of different races. De Winiwarter worked with the races of *Gryllotalpa gryll* (L) from Belgium, Naples, and Rumania. The Belgian race, he considers, have the diploid number of 12 chromosomes in the male. These include an X-Y pair. The Italian and Rumanian races have 15 chromosomes in the diploid state which include an X-chromosome. These results, which on the whole support and extend those previously obtained by other workers, reveal an extraordinary chromosomal dissimilarity between geographical races, to which entomologists are unable to assign specific differences. The presence of a Y-chromosome in the Belgian race and its absence in the others appear to offer strong evidence in support of Wilson's view that this chromosome is one in process of degeneration. The two additional pairs of autosomes in the Rumanian and Italian forms may have originated by fragmentation of one

or two pairs of chromosomes in the Belgian form. Such an hypothesis is supported by the fact that the chromosomes of the former are on the whole rather shorter than those of the latter.

Kenji Nakamura (*Memoirs, Coll. Sci., Kyoto Imp. Univ.*, Ser. B., vol. 4, 1928) has worked on the spermatogenesis of a snake (*Natrix tigrina*). The diploid number of chromosomes was 40, consisting of 8 pairs of large chromosomes and 12 pairs of small chromosomes. The large number and difference in size is characteristic of all the Sauropsida examined so far. He considers that one of the larger pairs constitutes an homologous pair of sex-chromosomes. These are not recognisable as such in the spermatogonia, but appear in the leptotene stage of the primary spermatocytes as a pair of karyosomes in contact with each other. The author considers that these results suggest that the male is the homogametic sex, although all previous workers on the sex-chromosomes in reptiles have considered it to be heterogametic He suggests that the female *N. tigrina* will be found to be heterogametic, but admits that definite conclusions regarding the sex-chromosome mechanism in this form cannot be arrived at until the conditions in the female are known.

The chromosomes of the guinea-pig have been studied many times, always with different results, the diploid number having been claimed to be 16, 24, 32, 38, 56, and 64. Some agreement has been arrived at over the sex-chromosomes in the male, consisting of an X-Y pair. This regrettable disagreement has called for reinvestigation. Moles (*Arch. de Biol.*, t. 38, 1928) has studied the condition in the amnion and in spermatogenesis. He found the amnion unfavourable material for determining the chromosomal number, but was able to demonstrate a pair of X-chromosomes which passed to opposite poles at mitosis. The spermatogonia studied all gave counts of 65 chromosomes and the primary spermatocytes had 33. Half the secondary spermatocytes had 32 and half 33. He concludes, therefore, that the sex-chromosome is unpaired, no Y being present. The diploid condition of the male is therefore 32 pairs autosomes + X. The X-chromosome, which is one of the largest present, is not recognisable in the spermatogonia, unlike other species.

Endocrine Organs.—It has been known for some time that the endocrine organs are not independent of each other, but are often closely linked up together in function. Recent work has tended to show still more clearly that many of the ductless glands produce their visible effects indirectly by hormonal stimulation of other endocrine organs. Allen (*Univ. California Public. in Zool.*, vol. 31, 1927) has described an exhaustive

and carefully controlled series of experiments which show clearly that amphibian metamorphosis is effected by the action of the anterior lobe of the pituitary in stimulating the activity of the thyroid gland. His technique consisted in the removal of the hypophysis and thyroid from tadpoles and of implantation into these of fragments of the anterior, intermediate, or posterior lobes of the hypophysis of adult frogs.

Hypophysectomy resulted in retardation of metamorphosis accompanied by reduction in the growth of the thyroid owing to the small quantity of colloid stored in its follicles. Hypophysectomised tadpoles into which posterior or intermediate lobe substance was grafted exhibited similar peculiarities, although the development of pigment showed that the grafts were functional.

Successful implantation of the anterior lobe into hypophysectomised tadpoles resulted, however, in metamorphosis and thyroid growth which equalled or actually exceeded the normal. The large size of the thyroid gland in these tadpoles was due to the distention of the follicles with large quantities of colloid. The excessive reaction often obtained was accounted for by the fact that the mass of the anterior lobe graft, having been taken from an adult frog, usually exceeded that of an anterior lobe of a normal tadpole. Implantation of the anterior lobe into thyroidectomised tadpoles, on the other hand, completely failed to induce metamorphosis.

These experiments demonstrate that the process of metamorphosis in anuran larvæ is effected by the action of the anterior lobe of the hypophysis working through the thyroid gland. Both the thyroid gland and the anterior pituitary are essential for the result.

Blacker (*Trans. Lab. Exp. Biol. Moscow*, vol. 4, 1928) obtains results that are difficult to correlate with these. He finds, like Allen, that hypophysectomy prevents metamorphosis in amphibia. Metamorphosis can be induced, however, in these hypophysectomised animals, if preparations of thyroid and of iodine are administered. He finds that preparations of anterior lobe accelerate metamorphosis, but that grafting of the anterior lobe does not. He observed that the thyroid gland tended to atrophy after hypophysectomy, which confirms this observation of Allen's. The contradictory results obtained separately by these two workers regarding the effects of grafts of the anterior lobe of the pituitary in effecting metamorphosis in hypophysectomised amphibian larvæ appear to necessitate further investigation.

Gonad-grafts.—Welti (*Revue Suisse de Zool.*, vol. 35, 1928) has described an extensive series of gonad-grafts on toads. He found that testis grafts took better when implanted on to

the kidney than on to the peritoneum, and both these situations were more favourable than the subcutaneous. The site of implantation appears to have much less effect on ovarian grafts. The relationship of donor and host is of fundamental importance. With the testis 89 per cent. of autografts, 77 per cent. of homografts, and 70 per cent. of heterografts took. With the ovary no heterografts persisted at all and only about 52 per cent. of the homografts, whilst 91 per cent. of the autografts gave positive results and some persisted after a lapse of two or three years. Many of the testis grafts exhibited a transitory phase of oogenesis, such as has been recorded in grafted and regenerate testes of Anurans by Guyenot and Ponse.

A similar intersexual condition has been observed by Du Bois and Beaumont (*C.R. Soc. Biol.*, vol. 97, 1927) in grafted and regenerate testes in Triton. This condition appears to be of fairly general occurrence in the testes of Amphibia and indicates a latent tendency towards sex-reversal in the male. This is of peculiar interest since it is in the opposite direction to the cases of sex-reversal in the frog, described by Crew some time ago, where the female transformed into the male. Both sexes appear, therefore, to be reversible in Amphibians, whilst in birds all the known cases of sex-reversal are from female to male.

Experimental Embryology.—Some interesting results of the effects of temperature gradients on segmenting eggs and embryos of the frog have been obtained by Dean, Shaw and Tazelaar (*Brit. Jour. Exp. Biol.*, vol. 5, 1928). The gradients were applied either at right angles (lateral) or parallel to the egg axis. In the latter case they were either " adjuvant " (animal pole heated, vegetative pole cooled) or " antagonistic " (animal pole cooled, vegetative pole heated). Adjuvant gradients accentuated the normal difference in cell size between the animal and vegetative poles. Antagonistic gradients produced a double gradient in cell size with the smallest in the equatorial region and, in extreme cases, the animal cells larger than the yolk cells. Adjuvant gradients, when the temperature was too high, resulted in inhibition of the animal pole, where large retarded cells occurred, sharply marked off from the surrounding small cells. Lateral gradients produced great differences in cell size on the two sides of the egg, the larger being, of course, on the cooled side.

Eggs treated with adjuvant gradients exhibited more advanced stages of development than similar eggs treated with antagonistic gradients. This resulted in the yolk cells of the " adjuvant " eggs being smaller than those of the " antagonistic " eggs, although the former were cooled and the latter

heated. The effects of gradients applied during segmentation appear to be permanent in a slight degree. These eggs treated with antagonistic gradients tended to develop into microcephalous tadpoles and *vice versa.* Antagonistic gradients caused the eggs to gastrulate sooner than " adjuvant " eggs under similar conditions; they inhibited the head and dorsal region in embryos at the neurula stage, whilst " adjuvant " gradients inhibited the tail and ventral regions.

Lateral gradients during gastrulation and later stages produced marked asymmetry, which persisted in some individuals but was regulated back to normal in others. This method should prove useful in tackling experimentally many problems connected with developmental mechanics.

Comparative Anatomy.—The Spiny Dogfish (*Squalus acanthias* and *S. sucklii*) is the type Elasmobranch commonly used for laboratory dissection in North America. Despite this fact no satisfactory account of the blood vascular system has been available. This deficiency has been met by a paper by O'Donoghue and Abbott (*Trans. R.S. Edin.*, vol. 55, 1928). They found that the blood vascular system of the two species of Spiny Dogfish studied are strikingly similar and only differ in unimportant details. This system is one of the most primitive and least specialised of any Elasmobranch. It furnishes, therefore, an excellent basis for comparison with other forms. The embryos, like those of other vertebrates, have six complete branchial arches between the dorsal and ventral aortæ. These are retained in a remarkably complete manner in the adult. This primitive arrangement indicates that the Spiny Dogfish, amongst living forms, approaches most closely to the ancestral condition of higher vertebrates.

AGRICULTURAL PHYSIOLOGY.

By John Hammond, M.A., School of Agriculture, Cambridge.

Reproduction.—(1) *The Breeding Season.*—Courrier (*Arch. de Biol.*, **37**, p. 173, 1927) has investigated the changes in the male organs throughout the year in the bat, mole, etc., and finds that the curves of activity of the interstitial cells and seminiferous tubules vary greatly at different times of year, that they are independent of one another, and that it is with the former rather than the latter curve that the changes in secondary characters appear.

Asdell (*Jour. Agr. Sci.*, **16**, p. 632, 1926) found in the goat, where the height of the breeding season occurs in October and the minimum in May, that a cool August produces an early œstrus while a hot one has the opposite effect; rainfall has no influence. The hypothesis is advanced that most

mammals breed only in the spring and autumn because the body temperature may be too high in summer for follicular development. Riddle and Honeywell (*Amer. Jour. Physiol.*, p. 337, 1924), on the other hand, find in pigeons that cooling results in a marked lowering of the concentration of sugar in the blood and also diminishes the rate of ovulation.

Parkes and Brambell (*Jour. Phys.*, **64**, p. 388, 1928) find in mice that a considerable decrease in the temperature causes an initial increase in the length of diœstrus, but it is very shortly restored to normal; they suggest that the prolonged winter anœstrus of wild rodents is probably to be attributed to a diminished food supply under conditions of temperature which, for maintenance of normal function, would require an increase of food.

(2) *Œstrus and the Œstrous Cycle.*—Allen and Doisy (*Physiol. Reviews*, **7**, p. 600, 1927), in a review of work on ovarian and placental hormones, conclude that partial chemical isolation of at least one substance has been accomplished and that no differentiation has yet been made in material from ovaries and from placenta. The reactions induced in ovariotomised animals are growth in all parts of the reproductive tract and mammary glands, secretion by uterine glands and epithelium of the uterine tubes, typical contraction rhythm of uterine and tubal musculature, congestion, swelling, and other external signs of œstrus, typical female mating reactions, and scanty menstruation (in monkeys) after injections were stopped (see also Allen, *Contrib. to Embryol.*, No. 98, 1927). In a series of papers on the internal secretions of the ovary Parkes and Bellerby (*Jour. Phys.*, **61**, p. 562, 1926; **62**, p. 145, 1926; **62**, p. 301, 1927; **62**, p. 385, 1927; **64**, p. 233, 1927) find that there appears to be some correlation between the size of the follicle and the relative activity of liquor folliculi and residual tissues of the ovary as regards the œstrus-producing extract, that injection of œstrin during the early stages of pregnancy invariably causes the rapid reappearance of œstrus and the termination of pregnancy, but larger amounts are required to do this at the later stages of pregnancy, that injection of œstrin during lactation (when œstrus is normally inhibited) caused œstrus and that the amount required to do this was proportional to the number of young suckling; they also suggest that the presence of œstrin in the placenta is more easily explained on the grounds that the placenta withdraws œstrin from the maternal circulation to protect the fœtus from its action rather than on the grounds that the placenta elaborates the hormone. They also find that œstrin is contained in the fluid found in the cavities of corpora lutea only and not in the solid tissue; extracts of the corpus luteum were prepared which

inhibited œstrus in the mouse. Tuisk (*Jour. Phys.*, **63**, p. 180, 1927) has shown that by continuous injection of extracts continuous œstrus can be maintained in mice as long as thirteen days, while Lipschütz and Adamberg (*C. r. Soc. d. Biol.*, **93**, p. 1413, 1925) attributed the hyperfeminisation of males into which an ovary had been transplanted to the ripe follicles which persist instead of forming corpora lutea. The histological features of the ripening and rupture of the follicle have been described by Shaw (*Jour. Obs. & Gyn. Brit. Emp.*, **34**, No. 3, 1927). Fels (*Klin. Woch.*, No. 50, 1926), who investigated the ovarian hormone in the blood, found a great increase in this substance during the latter months of pregnancy. This was also found by Aschheim (*Med. Klin.*, No. 53, 1926), and Zondek and Aschheim (*Klin. Woch.*, No. 10, 1926) have also described the distribution of the hormone in the ovary and during pregnancy; they also (*Klin. Woch.*, No. 28, 1927) have found ovarian and anterior pituitary hormone in the urine. The uterine effects of injections of liquor folliculi into castrated rabbits have been described by Courrier and Potrin (*C. r. Soc. d. Biol.*, **94**, p. 878, 1926), while follicular extracts were shown to produce typical pro-œstrous changes in the uterus of the bitch and rabbit by Asdell and Marshall (*Proc. Roy. Soc.*, B, **101**, p. 185, 1927). Evidence on the regulation of the œstrous cycle has been produced in a series of papers by Parkes (*Proc. Roy. Soc.*, B, **100**, p. 172, 1926; **101**, p. 71, 1927; **101**, p. 421, 1927; **102**, p. 51, 1927) as a result of X-ray sterilisation; although the ovaries of animals X-rayed at three weeks old contained no apparent cyclic structures (oocytes, follicles), complete cycles of normal length were exhibited when puberty was reached, and it is concluded that the normal ovary possesses some cyclic mechanism which brings about the rhythmic production of œstrus and the coincident maturation of follicles; ovariotomy of the sterilised gonad immediately stops the cycle so the actual sterilised tissue removed is the actuating factor; similar results were obtained in animals irradiated in adult life, and the fact that periodicity in the mouse is not altered by the elimination of the corpus luteum (as it is in the guinea-pig and the cow) is attributed to the absence of any marked luteal phase in the cycle of the mouse. The histological changes in these ovaries have been described by Brambell, Parkes, and Fielding (*Proc. Roy. Soc.*, B., **101**, p. 95, 1927; **101**, p. 315, 1927; **102**, p. 385, 1928). Brambell and Parkes (*Q.J. Exp. Phys.*, **18**, p. 185, 1927), by double ovariotomy on mice shortly before the œstrous period, found that the œstrous-producing stimulus becomes operative nearly two days before the appearance of œstrus and that the follicles undergo nearly the whole of their maturation

growth after the time when the œstrous stimulus becomes operative; they suggest that follicular maturation is not responsible for œstrous production, but that follicular maturation itself results from the operation of the œstrous-producing stimulus. Frei and Stäheli (*Deut. Tierarzt. Woch.*, **34**, p. 755, 1926), who investigated the absence of heat in cows, found 87 cases due to persistent corpora lutea, of which 56 came on heat again 2–8 days after the corpus luteum was squeezed out, and 80 per cent. became pregnant; cysts were the cause in 10 cases, while in 36 cases inactive or atrophic ovaries were found which were successfully treated by massage of the ovary (6 cases) or intramuscular transplantation of ovary (13 cases). A book giving an account of original research and a summary of the literature on the œstrous cycle, pregnancy, and the development of the udder in the cow, has been published by Hammond (*Reproduction in the Cow*, Cambridge, 1927). The vaginal cycle in the cow has been described by Frei and Metzger (*Berlin Tierarzt. Woch.*, **42**, p. 645, 1926), while McKenzie (*Missouri Agr. Exp. Sta., Res. Bul.*, **86**, 1926) has given an account of the vaginal changes in the sow; the latter suggests that the growth-producing stimulus is directed for the most part to the vestibule and vagina prior to œstrus and is then shifted to the area of the uterus to prepare that organ for possible pregnancy. Observations on the œstrous cycle in the mare have been made by Aitken (*Vet. Pract. Bul., Iowa State College of Agric.*, **8**, p. 178, 1926, and *Jour. Amer. Vet. Med. Ass.*, **70**, No. 4, 1927); he finds no limited breeding season, a cycle averaging 22 days and a heat averaging 7 days, with ovulation occurring during the last or next to last day of heat while the corpus luteum of pregnancy is reduced below what would be considered as functional size before the middle of the gestation period. Corner (*Jour. Amer. Med. Ass.*, **89**, p. 1838, 1927) finds that in the monkey menstruation frequently occurs without ovulation, and is therefore not dependent on the corpus luteum; menstruation without ovulation is, however, not preceded by the so-called premenstrual changes in the endometrium, which occurs only after the formation of a corpus luteum. In an account of pro-œstrum and pseudo-pregnancy Marshall (*Q.J. Exp. Phys.*, **17**, p. 205, 1927) concludes that the menstrual discharge in man and monkeys represents pro-œstrous destruction often accompanied by pseudo-pregnant degeneration. In the ferret, which only ovulates after coitus, it was found by Hammond and Marshall (*Proc. Phys. Soc., Jour. Phys.*, **65**, 1928) that the cycle (pseudo-pregnancy) can be produced at any time desired during the breeding season and so does not depend on any innate rhythmical process apart from ovulation and the formation of corpora lutea. A review

dealing with the whole question of the physiology of ovarian activity has recently been published by Parkes (*Biol. Rev.*, **3**, p. 208, 1928). The circumstances affecting the œstrous cycle in the mouse have been described by Parkes (*Proc. Roy. Soc.*, B. **100**, p. 151, 1926); it is prolonged by mating with vasectomised males and is inhibited during lactation, except where one or two young only are reared. Cameron and Amies (*Australian Jour. Exp. Biol. & Med. Sci.*, **3**, p. 37, 1926) find that thyroid feeding causes in mice an increase in the duration of œstrus and in the length of the whole cycle.

(3) *Fertility.*—General accounts of sterility and fertility in domestic animals have recently been published by Frei (*Sterilität der Weiblichen Haustiere*, Berlin, 1927) and Marshall and Hammond (*Ministry of Agric. Research Monograph*, No. 2, London, 1926). Most of the literature can be conveniently considered under the three successive stages at which fertility is controlled.

(*a*) *The Number of Follicles which ripen.*—In a paper on some fundamental laws of ovarian dynamics, Lipschütz (*Biol. Rev.*, **2**, p. 263, 1927) concludes that the number of primary follicles which enter into follicular development depend not upon the total number present but upon general internal factors outside the ovary. Crew (*Biol. Gen.*, **3**, p. 207, 1927) found in rats that litter size was unaffected following unilateral ovariotomy. Domm and Juhn (*Biol. Bul.*, **52**, p. 458. 1927), from experiments on unilateral castration in fowls, similarly conclude that the general bodily metabolism favours the growth of a definite amount of gonad tissue and no more, and that on removal of one gonad the surviving member thereupon responds in proportion to its growth capacity up to the limits of the favourable metabolism. Hammond (*Reproduction in the Rabbit*, Edinburgh, 1925), adopting Heape's idea of a "generative ferment," a product of intermediary metabolism existing only in small quantities in the blood, explains on this basis variations in the number of follicles which ripen and the dependence of the gonad for its germ-cell production and internal secretions on somatic nutrition. Smith (*Proc. Soc. Exp. Biol. & Med.*, **24**, 1926–7), and Smith and Engle (*Proc. Soc. Exp. Biol. & Med.*, **24**, 1927, and *Amer. Jour. Anat.*, **40**, 1927) discovered that by the regular transplantation of anterior lobe of pituitary into very young mice follicles could be caused to ripen and ovulate before weaning age. Zondek and Aschheim (*Arch. f. Gynäk.*, **130**, p. 1, 1927), as a result of similar experiments, also conclude that the anterior pituitary is the primary factor and the ovary the secondary factor in sexual functions, that the anterior pituitary hormone brings the follicular apparatus into action, induces the ripening of the follicles, and secondarily through

these the production of ovarian hormone. The application of this discovery to the control of fertility should be of great importance in the future.

The seasonal variation in fertility has been investigated by several workers. Cole and Rodolfo (*Proc. Amer. Soc. Animal Prod.*, p. 116, 1924) found in cows a larger percentage of twins in animals breeding in October–December than in the other months of the year. King (*Arch. f. Ent.*, **112**, p. 61, 1927) found in the rat that the fertility as measured by the number of litters born is at its maximum in spring and summer, but that there was no marked correlation between seasonal variations in litter production and litter size, the average size of the litter being fairly constant throughout the year. Feldman (*Carnegie Inst. of Washington*, Pub. 337, p. 49, 1926), however, while confirming these observations on the number of litters born, obtained the largest litter size (7·3) in June and the smallest (5·2) in October. He also studied the effect of age and the litter size, this increasing to a maximum (6·3) at 180 days and then decreasing again. Parkes (*Brit. Jour. Exp. Biol.*, **2**, p. 21, 1924) finds in the mouse that the second litters are the largest and that afterwards the average size decreases again. Jones and Rouse (*Jour. Dairy Sci.*, **3**, p. 260, 1920) find that the proportion of multiple births in cattle rises rapidly to 5 years old, and then more slowly, with no subsequent decrease up to 15 years, whereas with sheep it rises to 5 years and then falls. Fertility in sheep has been studied by Nichols, who shows (*Jour. Min. of Agric.*, **33**, No. 3, 1926) that the improvement of nutrition at tupping time raises the fertility of the flock ; he also investigated fertility in Southdown sheep (*Jour. Agr. Sci.*, **16**, p. 365, 1926) and the effect of metrological conditions (*Zeit. f. ind. Abs. v. Vererb.*, **43**, p. 313, 1927). White and Roberts (*Welsh Jour. of Agric.*, **3**, p. 70, 1927) found great differences in fertility between mountain and lowland flocks of Welsh sheep, the percentage twinning depending principally on the condition of the ewes at tupping time and the date of tupping. Polyovular follicles in the opossum and other mammals have been investigated by Hartman (*Amer. Jour. Anat.*, **37**, p. 1, 1926). Several cases of freemartins have been described by Tuff (*Festskrift B. Bang*, Copenhagen, p. 175, 1928).

(*b*) *The Number of Ova which get fertilised.*—While under normal conditions many more sperm are present than are required to fertilise the ova of any female, yet the chances of any one sperm arriving at the tops of the tubes at the right time are small. Walton (*Proc. Roy. Soc.*, B. **101**, p. 303, 1927) has shown that by artificially inseminating rabbits with semen diluted in successive degrees, first full fertility (and

large litters), then reduced fertility (and small litters), and eventually sterility is reached at a point when there are still fairly large numbers of sperm in the injected fluid. Hammond and Asdell (*Brit. Jour. Exp. Biol.*, **4**, p. 155, 1926) found that, whereas the sperms will retain their fertility up to 38 days in the epididymis of the male rabbit, once they are deposited in the female tract their life is short—only up to 30 hours. In cases where the sperm had remained in the female tract for 24–30 hours before ovulation small litters were frequently produced; it is suggested that this is due to the end-point of the vitality of the sperms falling within the limits of time during which the process of ovulation occurs. That the ova of the rabbit are not capable of being fertilised for more than 3–4 hours after being shed has been shown by Hammond (*Reproduction in the Rabbit*, Edinburgh, 1925). Asdell (*Jour. Amer. Med. Ass.*, **89**, p. 509, 1927) finds in man that fertility is higher in the early part of the cycle and falls rapidly from the sixteenth to twentieth day. Crew (*Proc. Roy. Soc., Edinb.*, **46**, p. 230, 1926) has shown that in the fowl fertile eggs are produced within 24–48 hours after the introduction of the male, that the length of life of the sperm within the body of the female is about 15–20 days, although eggs laid after the first week commonly fail to complete their development; he also found that if after the removal of a male a second male is introduced the influence of the first sire is removed by the seventh to the tenth day.

Dunn (*Poultry Sci.*, **6**, p. 201, 1927) could find no evidence of an effect of the age of the sperm up to 2 weeks on the viability of the embryo, but found that a great difference existed between the number of offspring produced from two cocks when a hen was mated to both in quick succession, there being no discrimination in favour of the sperm of close relations. Differences in the fertility of stallions, where the average fertility is low (54 per cent.), have been found by Sanders (*Jour. Agr. Sci.*, **16**, p. 466, 1926); each individual's percentage in successive seasons tends towards a constant figure, so that there are definitely good and bad "getters" of foals. There was a slight increase in fertility from 3–13 years old in light horse stallions and a marked decline after 16 years. Savage, Williams, and Fowler (*Trans. Roy. Soc., Canada*, **21**, p. 425, 1927), from a determination of head-length distributions of sperms in bulls, find that abnormal curves unfailingly indicate genetically unsound sires, while Williams and Savage (*Cornell Vet.*, Oct., p. 374, 1927) have described the methods by which unsatisfactory sires can be detected. Redenz (*Pflüger's Arch. f. g. Phys.*, 216, p. 605, 1927) has shown that spermin does not prolong the life of the sperm.

Vitamin-A deficiency in the rat has been shown by Parkes and Drummond (*Brit. Jour. Exp. Biol.*, **3**, p. 251, 1926) to produce sterility, but histological examination failed to reveal a cause, and it was concluded that sterility was due to physiological disability and disinclination to copulate. Arrested development of the Muellerian ducts associated with inbreeding was found by Fincher and Williams (*Cornell Vet.*, Jan., 1926) to be a cause of sterility in cattle.

(*c*) *The Number of Fertilised Ova which develop to Birth.*—Fœtal resorption in cattle has been described by Turner (*Wisconsin Agr. Exp. Sta.*, Pub. 82). Long and Parkes (*Bioch. Jour.*, **18**, 1924) have shown that the process is one of autolysis on the part of the dead fœtus. Hammond (*Brit. Jour. Exp. Biol.*, **4**, p. 337, 1927) found that fœtal tissues transplanted into the uterus are absorbed by the mucosa, although the graft will grow where attached to the muscle; it is suggested that the fœtal trophoblast has an active erosive function which is absent in other fœtal tissues. MacDowell and Lord (*Arch. f. Entwick.*, 109, p. 549, 1927) have shown that light doses of alcohol have no effect on the pre-natal mortality of mice, while heavy doses increased the pre-natal mortality from one to two embryos per litter. Reynolds and Macomber (*Amer. Jour. Obs. & Gyn.*, **2**, No. 4, 1921) find that dietary differences in rats too slight to produce ill-health cause lowered fertility, and Macomber (*Jour. Amer. Med. Ass.*, p. 6, 1927) states that calcium deficiencies increase the intra-uterine mortality. The effect of different diets on fertility in the rat has also been investigated by Kennedy (*Q.J. Exp. Phys.*, **16**, p. 281, 1926). Evans and Burr (*The Anti-sterility Vitamine Fat Soluble E, Mem. Univ. of California*, 1927) describes experiments with Vitamine-E, the absence of which causes sterility in the male and absorption of embryos in the female, in these cases all the embryos are absorbed, and not a few only. Hartman (*Contrib. to Embryol.*, **19**, No. 108, 1927) describes in the opossum cases of defective corpora lutea correlated with pathological uteri and death of all the embryos. Wriedt (*Vererbungslehre der landwirtschaftlichen Nutztiere*, Berlin, 1927) gives an account of fœtal degeneration in various species and explains those cases where only certain of the embryos perish by lethal Mendelian factors.

(4) *Pregnancy and Parturition.*—The passage of the ova through the Fallopian tubes of the sow has been studied by Andersen (*Amer. Jour. Phys.*, **82**, p. 557, 1927), who concludes on the basis of the distributions in the different segments that it passes through the upper portion quickly, stays a comparatively long time in the lower middle portion, and moves rather quicker through the uterine end. By means of unilateral

ovariotomy Warwick (*Anat. Rec.*, **33**, p. 29, 1926) has demonstrated that intra-uterine migration of the ovum takes place in the sow; there was an indication that migration had occurred in 42 per cent. of cases in normal pregnant uteri. Ancel and Bouin (*Gyné. et Obstr.*, **13**, p. 401, 1926) have again brought forward evidence that it is the corpus luteum which prepares the uterus for the fixation of the egg and deny Schèckelé's contention that it is the follicle which prepares the uterus for fixation. Teacher (*Zeit. f. Anat. v. Entwick.*, **76**, p. 360, 1925) concludes that the human ovum burrows into the endometrium like a vigorous parasite destroying maternal tissue and provoking inflammatory and reparative action. Shaw (*Jour. Obs. & Gyn. Brit. Emp.*, **34**, p. 1, 1927) finds that decidual cells may be found in the premenstrual period of the cycle and that their production is controlled by the corpus luteum; he also finds (*Jour. Obs. & Gyn. Brit. Emp.*, **33**, p. 183, 1926) that the interstitial cells of the human ovary are derived from the theca interna cells of the follicle and that they are produced during pregnancy. Tamura (*Brit. Jour. Exp. Biol.*, **4**, p. 81, 1926) has observed hypertrophy of the zona fasciculata and degeneration of the zona reticularis of the suprarenals of the mouse during pregnancy. Cases of superfœtation in pigs and sheep have been reported by Buchanan Smith (*Jour. Anat.*, **61**, p. 329, 1927, and **62**, p. 100, 1927). The action of posterior pituitary extract upon the pregnant uterus of the rabbit has been studied by Knaus (*Jour. Phys.*, **61**, p. 383, 1926), who found that from the first to the seventeenth day it was not possible to disturb pregnancy with a reasonable dose, that from the eighteenth to the twenty-eighth day no quantity of pituitary was ever found sufficient to induce abortion on that day, but the fœtuses were killed inside the uterus and were aborted some days (2–6) subsequently, that from the twenty-ninth to the thirty-second day a very small dose caused the birth of one fœtus, while a larger one caused the whole litter to be born immediately. In this and further publications (*Arch. f. Exp. Path. u. Pharm.*, **124**, p. 152, 1927) he stresses the effect of the degree of development and sensitivity of the uterus muscle at the different stages as being an important factor in the cause of birth. A general review of the conditions governing parturition has been published by Marshall (*Biol. Rev.*, **2**, p. 129, 1927).

(5) *Artificial Insemination.*—Lush (*Jour. Agr. Res.*, **30**, p. 893, 1925) attempted the centrifugal separation of male and female producing sperm in the rabbit, but found no differences in the sex ratio of young produced from the inseminations. Yamane and Egashira (*Jour. Jap. Soc. Vet. Sci.*, **4**, p. 101, 1925) found in rabbits that artificial insemination without a sterile mating at the same time only gave a low fertility (8 per

cent.), whereas with a sterile mating they obtained a high degree of fertility (62 per cent.), showing that ovulation only rarely takes place spontaneously in the rabbit. They also found (*Dobutsugaka Zasshi*, Tokyo, **36**, p. 299, 1924) by artificial insemination that crosses between the hare and rabbit were not possible. Walton, Hammond, and Asdell (*Verh. I. Int. Kong. Sexualfors.*, Berlin, **2**, p. 217, 1928) have investigated the effect of temperature on the life of the rabbit's sperm outside the body; the optimum temperature was found to be 10° C., at which young were produced after 78 hours, and the possibility of transporting semen by post was demonstrated. Iwanow (*C. r. l'Acad. d. Sci.*, **183**, p. 456, 1926) has been able to keep the sperm of the rabbit 7 days, and that of the guinea-pig 8 days in the epididymis separated from the body at 1°–2° C. and has obtained young on artificial insemination.

(6) *Male Organs.*—Moore (*Biol. Bul.*, **51**, p. 112, 1926), as a result of replacing abdominal testes in the scrotum, concludes that only in the scrotum are spermatozoa produced by the testis. Cunningham (*Brit. Jour. Exp. Biol.*, **4**, p. 333, 1927) suggests that the adaptation of spermatogenesis to temperature is the consequence of the evolution of the scrotum; his experiments failed to confirm Steinach's theory that rejuvenation depends on increase of the interstitial cells of the testis, and that this increase can be produced by ligation of the vas deferens. Tamura and Crew (*Proc. Roy. Soc., Edinb.*, **46**, p. 283, 1926) found that degeneration of the germinal epithelium invariably follows vasectomy in the mouse, but does not become evident until some time has elapsed; similar degeneration follows epididymo-deferentectomy, but after a much shorter interval of time. Warwick (*Anat. Rec.*, **31**, p. 19, 1925) found that vasectomy of itself does not cause degeneration of the seminiferous tubules in pigs. In a report on Voronoff's experiments on the improvement of live stock by testicular grafts Marshall, Crew, Walton, and Miller (*H.M. Stationery Office*, 1928) suggest that it is highly desirable that the matter be put to further and more critical test in this country, where conditions are more favourable to scientific control.

PREHISTORIC ARCHÆOLOGY. By L. J. P. Gaskin, Librarian to the Royal Anthropological Institute London.

Antiquity, June 1928.—In this number Mr. Randall-MacIver completes his studies on "Forerunners of the Romans." Mr. MacIver states that at the date of about 1000 B.C., a little after the beginning of the Iron Age, Northern and Central Italy was divided into five distinct spheres of civilisation, occupied by the following peoples: in the north-west, the Comacines, in Venetia, the Atestines, in the Bolognese region, the northern

Villanovans, in Tuscany and part of Latium, the southern Villanovans, and east of the Apennines, from Rimini to Aufidena, the Adriatic coast and the central Apennines, by the Picenes.

In the cultural development of these peoples, the author stresses the importance of the Villanovans as metal workers and attributes the rapid pre-eminence of the Etruscans in the copper, bronze, and iron trade of the Mediterranean to the presence of skilled and experienced Villanovan coppersmiths among them as serfs and dependents.

In "Two Great Dolmens of Central France" Admiral Boyle Somerville describes: (1) *Dolmen de Saint Antoine du Rocher.* This Dolmen consists of a single chamber 30 feet long (inside), 11 feet wide, and 6 feet high. The remains of an *allée couverte* are to be noted and the orientation indicates the line of apparent sunrise on the days of the equinoxes (March 21 and September 21). (2) *Le Grand Dolmen de Bagneux.* This is said to be the largest Dolmen in existence; it is about 61 feet long, 16 feet wide, and 9 feet high. There are, as at Saint Antoine du Rocher, the remains of an *allée couverte,* and the orientation of the Dolmen is similar. Admiral Somerville is to be commended for the excellent drawings which accompany this article, and for his careful measurements of the two Dolmens.

Dr. Einar Gjerstad, the head of the Swedish Archæological Expedition to Cyprus, who worked last year at Lapithos on the north and Karavostasii (ancient Soli) on the north-west of the island, writes on the results of the excavations.

At Lapithos, twenty-three tombs from the early and middle Copper Age and twenty tombs from the early Iron Age were opened and examined. Dr. Gjerstad adduces from the results of the excavations that the Greeks colonised Cyprus in the late Cypriote III period just at the end of the Bronze Age.

At Karavostasii (Soli) the theatre has been excavated. It was built in the Hellenistic period and rebuilt in the late Roman period. Of great interest is the discovery of a Greek temple within the theatre walls; it is evident that when the theatre was built a Doric archaic temple was demolished to serve as building stones. The foundations of the temple have been found; but it yet remains to be seen whether the architectural fragments found in the theatre belong to this temple.

In "Recent Finds at Beisan" Mr. Alan Rowe describes the excavations of the southern and northern temples of Thothmes III. In the southern temple the following are the main divisions: the inner sanctuary, a rectangular room with a brick altar for cult objects and a stone altar for meat offerings; a sacrificial altar-room, containing a great altar of sacrifice with two steps leading up to it—the remains of a bull were discovered

in this room; a courtyard with tables; a room north of the inner sanctuary, southern corridor, and mazzebah.

The northern temple has not yet been thoroughly excavated; from a room to the south of it came an object of great interest, a bowl with an undulating serpent carved on it in high relief. The importance of this find lies in the fact that serpent-worship, so common in Beisan during the reigns of the later Egyptian kings, was already practised there in the reign of Thothmes III.

Eurasia Septentrionalis Antiqua (vol. 3, 1928).—In a long and well-illustrated article, entitled "Permian Studies," Prof. Tallgren discusses the genealogy of the "Permian Idols." He traces their descent from the old cultures of the Near East which established themselves in the forest districts of Russia in the second millennium B.C. In this connection it should be pointed out that Hittite and Russian Bronze Age Cultures have many common traits, and that Hittite bronze idols have been found as far north as Bohemia and East Prussia.

In the second part of this article Prof. Tallgren writes on the "Permian-Scythian Fish and its Descendants." He points out that the horse was a valuable property in the eyes of the nomadic Scythians; it enjoyed the special protection of a great deity "Anahita," who was at the same time the deity of waterways and flowing water. Its symbol was the fish, a fish which resembles the sturgeon, and is commonly found carved in silver on horse-trappings, etc. Prof. Tallgren thinks that Scythian art motives did not spread to Perm from ancient Scythia, but from the east and south-east, from the Asian, not from the European, side.

M. N. Bortvin reports on the Verkhny-Kizil discoveries. This is a Bronze Age find made near the village of Verkhny Kizil in the district of Troitsk in the Ural region. The inventory includes such articles as bracelets, bugles, neckbands, rings, daggers, sickles, fishing-hooks, hoes, spear-heads, and pottery. M. Bortvin connects this find with the Abachevo Bronze Age culture and dates it in the second half of the second millennium B.C.

Antiquaries Journal (July 1928).—In an interesting article on early magnifying-glasses Mr. H. C. Beck mentions Mr. Forsdyke's discoveries this year in Crete of two crystal magnifying lenses that date back to 1200 B.C. He hopes that the results of Mr. Woolley's excavations at Ur will bring to light lenses of an earlier date. Of an interest as an early reference to magnifying-glasses in literature is a mention of the burning glass by Aristophanes in the *Clouds* (*circa* 343 B.C.).

Mr. C. F. Fox writes on a Bronze Age refuse-pit at Swanwick (Hants). Remains of one or two primitive looms, of loom weights of burnt clay, imperfectly fired, centrally pierced, and

of different sizes were found. Mr. Fox assigns a late Bronze Age date to these finds (*circa* 1000–600 B.C.). Close to the pit a hoard of bronze palstaves was discovered.

In view of the fact that Bronze Age hoards are very frequently discovered in forest areas, Dr. Cyril Fox has come to the conclusion that the bronze founders sought the forest because of the supply of fuel close at hand.

Prace i Materjaly Archeologiczne, (tome 4, 1927).—In a long and profusely illustrated article on sites of the "Hallstatt Culture" in the valley of the Dunajec, a tributary of the Vistula, M. J. Zurowski first surveys the whole of the finds in the Dunajec Valley, he then analyses the material found, and finally gives the chronology of the objects discovered and the different cultures which they represent. In his survey of the pre- and proto-history of the Dunajec Valley M. Zurowski finds traces of a Neolithic population succeeded by Bronze Age peoples of the "Hallstatt Culture," and, of this culture, he distinguishes (after Reinecke) three distinct groups, namely Hallstatt, B. 1000–800 B.C.; Hallstatt, C. 800–500 B.C.; Hallstatt, D. 700–550 B.C. M. Zurowski lays special emphasis on cultural influences from Hungary, and in a lesser degree from Slovakia. The influence of the Scythians in Little Poland is very marked. There is an excellent map of the sites.

Tijdschrift van het Koninklik Nederlandsch Aardrijkskundig Genootschap (July 1928).—In reporting three implements of the Upper Palæolithic Era, recently discovered in Sumatra, Prof. J. P. Kleiweg de Zwaan asks the question, "What is the extent of our knowledge of prehistoric man in the Dutch East Indies and the neighbouring continents?" He approaches the subject from two points of view: (1) the human remains, (2) implements made by prehistoric man.

For the human remains he surveys the finds of Dubois in Java, the Australians and Tasmanians, the Veddas of Ceylon, the inhabitants of the Celebes, the kitchen-middens of Malacca, the cave dwellers of Perak, and Verneau's finds in Indo-China.

Of the stone implements he finds that the oldest culture is to be found in Sumatra, but palæoliths have been found in the Malay Peninsula, Ceylon, Sarawak, and Indo-China. Implements of the Neolithic Period, to which he assigns four cultural phases each with a distinct technique of its own, are found spread all over the Middle East.

Der Bayerische Vorgeschichtsfreund (Heft 7, 1927–8).—In an article on the Münchshöfer pottery in Western Bavaria, Herr P. Reinecke discusses its technique and its cultural affinities with other types of Neolithic pottery in Germany, Austria, and Moravia. Distinctive types of Münchshöfer pottery are the large pedestal dish, or vase, and the thick-lipped beaker.

Herr Reinecke assigns a date of 2500 B.C. to this culture.

Dolgozatok (tome 4, 1–2, 1928).—In a well-illustrated article, Dr. M. Roska discusses the Bronze treasure at Tetétlen. While tracing cultural affinities in the objects found with an early type of "Hallstatt Culture," Dr. Roska dates the treasure as not earlier than 900–800 B.C.

In "Excavations near Oszentivan," Dr. Banner writes on the different types of pottery found, discusses the technique, and assigns it an early Bronze Age date. In the excavation of the graves, bronze arm-bands, bracelets, and pearls have been found among the associated grave goods.

Præhistorische Zeitschrift (Band 18, Heft 3–4, 1927).—In a well illustrated article on the Stone Age material from the Southern Mediterranean in the Museum für Völkerkunde, Berlin, Fräulein Baumgärtel and Herr Brotzen survey the Palæolithic, Mesolithic, and Neolithic cultures of Northern Africa. Their inquiry extends from the Sahara on the west to Palestine on the east.

Dr. Jacob-Friesen, in a long and well-illustrated article, discusses the technique of the bronze wheels discovered at Stade.

Of interest is the fact of the distribution of the Stade type of wheel, similar finds being made at Hassloch, Langres, Cortaillod, La Côte-Saint-André, Nîmes, and Fa.

Bulletin de la Société préhistorique française (April, May, and June, 1928).—*April.* M. Vignard records the results of four years' work at Sébil, Assouan Province, Upper Egypt. In the work of excavation he distinguishes three levels; in the first he has found implements of Mousterian form, in the second implements of a similar character not so well formed, and in the third microliths. M. Vignard finds parallels between the implements of the first level and those of the Egyptian Mousterian period; he thinks that the finds in the second level combine the forms of the Aurignacian, Magdalenian, Solutrean, and Azilian cultures which separate in Europe and that those of the third level correspond to the Tardenoisian industry. To the whole he gives the comprehensive name of Sébilien.

The article is illustrated by a series of plates.

May. In a preliminary note on the archæological expedition to Southern Tunisia (1927–8) Dr. E. Passemard states that late Palæolithic and early Neolithic objects have been discovered. The discoveries will be treated more fully in future numbers of the *Bulletin*.

June. M. Vayson de Pradenne, in an article on the sign of the sickle on the Breton Megaliths, points out that the flint-edged wooden sickle was very common at the beginning of the Bronze Age. He traces this form of sickle from the Upper Fayum, Egypt, through Italy to Brittany.

Exhibitions.—Since archæological exhibitions record in the most concrete fashion the results of recent discoveries in archæology, some mention of them has been made here.

British Museum, Antiquities from Ur.—The excavations at Ur, 1927–8, have resulted in a rich harvest of archæological objects of great interest and value. In the grave of Mes-Kalam-Dug, inside the coffin itself, were found a golden dagger, lamp, and bowls inscribed with the name of the prince; against the body was a tumbled mass of ear-rings, bracelets and beads, and resting on the arms and skull a golden wig-like helmet in a state of perfect preservation. The grave of Queen Shub-Ad yielded a most elaborate form of headdress comprised of gold leaves, golden pendants, and gold flowers inlaid with white shell and lapis lazuli. Of interest was the discovery in the tomb of the king of a beautiful silver model of a boat, similar in type to those in use in the marshes of Mesopotamia to-day.

Royal Anthropological Institute.—The archæological objects found by Miss Caton-Thompson during the 1927–8 expedition of the R.A.I. to the Fayum were exhibited at the Institute from July 9 to 21. Plans and photographs in the exhibition illustrate the important discovery of a Ptolemaic irrigation system in the northern desert. The work is dated by a series of coins of which the earliest belong to the reign of Ptolemy II (285–247 B.C.) and the latest to Ptolemy III (247–222 B.C.).

The discovery of gypsum quarries some 30 miles from the Pyramids led to the further discovery of some 250 stone-hut-circles which had housed a population of about 1,000 people employed in grinding the gypsum to powder for use as plaster in the erection of the Pyramids.

A large number of hand-picks and crescent grinders were found which had obviously been used in the manufacture of alabaster vases, some 3,000 of which were found on the site. Of interest is the fact that in this subsidiary industry not one of the vases was finished, which suggests that it was the custom to finish them elsewhere.

University College, British School of Archæology in Egypt.—The principal work of the British school during the last season lay in Palestine at Tell Fara, Beth-Phelet. A large number of tombs were opened extending back to the nineteenth dynasty; all had been tampered with. Amongst the finds was a Bronze Age couch of Mesopotamian pattern and a silver dipper, having the handle modelled as a swimming girl (photos only, originals in the Palestine Museum). Pottery, pieces of jewellery, strings of beads, and about 200 scarabs (proving connection with Egypt) were also found.

ARTICLES

THE J PHENOMENON IN X-RAYS

By B. L. WORSNOP

Lecturer in Physics, King's College, London

The J Radiation

In the Bakerian lecture for 1916 Professor Barkla announced that he had evidence in elements of low atomic weight, of a characteristic radiation which was more penetrating than the K radiation of those elements; to this he assigned the name "J radiation." It was detected by three methods. These were: (1) ionisation method; (2) corpuscular radiation method; (3) absorption and fluorescence method.

We will consider the first two together. The method employed was to pass a beam of X-rays from the same source into two ionisation chambers and to measure the ionisation produced in air in the one and, say, SO_2 in the other. It was found that the ionisation produced in air and SO_2 was in constant ratio when the penetration of the incident X-rays was small, as seen to the *right* of Fig. 1. For an incident radiation corresponding to the point A, however, this constant relation was no longer found. As seen in the figure, a rise occurred, which was ascribed to extra ionisation in the nitrogen of the air produced by the electrons associated with the emission of the "J radiation" of nitrogen. The second point of interest in this experiment was the result which is shown at point C, where it may be noticed that the relative ionisation curve falls quite suddenly. This fall was said to be due to a similar increase in ionisation in the SO_2, caused by the emission of the J electrons of sulphur.

As is well known, if a beam of X-rays passes through hydrogen the ionisation produced is very small, but if a metal is placed within the atmosphere of hydrogen, and in the beam of rays, a large amount of ionisation is produced by the electrons which are ejected from the metal by the "photoelectric" action of the X-rays. This type of experiment was then performed in one ionisation chamber and the ionisation so pro-

duced was compared with the ionisation produced directly in air in the second chamber. As with the first experiment, the ratio of the two ionisations was constant for the more absorbable rays, but, both in the case of a copper and an aluminium source of photoelectrons, there was a sudden rise at the same point A at which a divergence had been observed for SO_2 and air. The comparison between air and aluminium is illustrated in curve 2 in Fig. 1. It will be seen in this case that a drop occurs in the ratio at a point B, whereas in curve 1, for copper and air, no such drop is to be found. A general examination of the results makes it clear that the change at

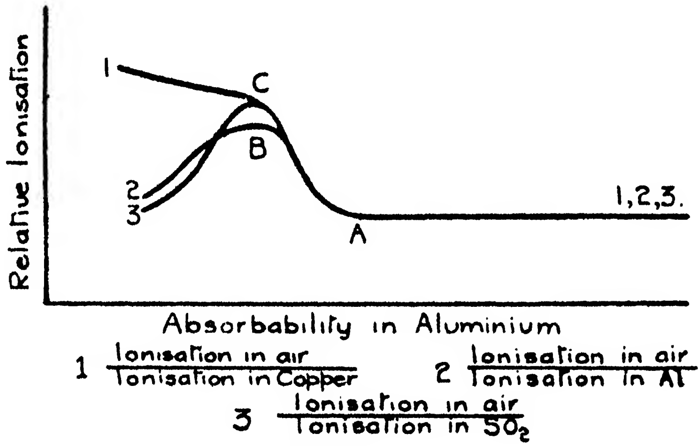


FIG. 1.

A, B, and C, corresponding to the supposed excitation of the J radiation in nitrogen, aluminium, and sulphur, is for an incident penetration which increases as the atomic weight of the source increases. The suggestion of a characteristic radiation under the conditions represented therefore seemed not an improbable one.

The second method of showing the effect, depended on absorption measurements very similar to those used to establish the K and L series at an earlier date. A beam of heterogeneous X-rays whose average wave-length was reduced for successive observations, was absorbed in iron and aluminium and the ratio of the mass absorption coefficients (μ/ρ) in these substances was plotted against an estimated value of the mean wave-length of the radiation used. It will be remembered that the early work of Barkla and others showed that such a ratio as this is constant when the radiation used is well removed from an absorption limit of the material of the absorbers.

The results of Barkla in this investigation are shown in Fig. 2, where it is to be noticed that the ratio of the absorption in iron to the absorption in aluminium for wave-lengths greater than 1·9 A.U. is definitely a constant, but at this wave-length the ratio shows a very marked rise. This rise is to be anticipated as being due to the extra absorption in iron brought about by the excitation of the K radiation of that substance. In other words, the incident radiation at this point is just a little more penetrating than the most penetrating of the iron K series.

The fall that occurs at ·5 A.U. is not to be anticipated, however, in terms of known radiations; Barkla explained it

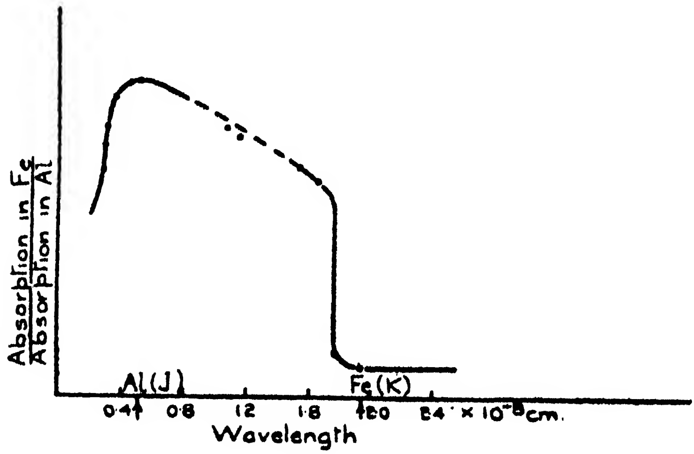


Fig. 2.

as being caused by a similar extra absorption in aluminium brought about by the excitation of the J radiation in aluminium. The wave-length was found to be of the same order as that corresponding to point B in Fig. 1.

The third method was another previously used in the early experiments on the K and L series. An X-ray tube was set at one definite hardness and the absorption coefficient of the rays was determined in paper, aluminium, paraffin, wax, and copper; the tube was then hardened or softened, and for each state the absorption coefficient was determined in these substances. The experiments already described had shown that copper apparently did not readily emit a J radiation; the absorption coefficient of copper was therefore taken as measuring the hardness of the rays, and the absorption coefficients in the other substances were plotted against that of copper. These results were shown in Fig. 3. Discontinuities

were found in aluminium, oxygen (both in water and paper for the same μ/ρ in copper) and carbon (paraffin wax) as seen in the figure. Using the results of Siegbahn and Hull and Rice, they found the following as the probable wave-length for the corresponding J radiation :

$$\lambda_J = \cdot 37 \times 10^{-8} \text{ cm. (Al)}; \ \cdot 39 \times 10^{-8} \text{ cm. (O)}; \ \cdot 42 \times 10^{-8} \text{ cm. (C)}.$$

It will be realised that these breaks resemble very closely the kind of discontinuity which Barkla and Sadler found in their work on the K- and L-rays.

All the experiments seemed to indicate the conclusion which Barkla reached, that the elements investigated did

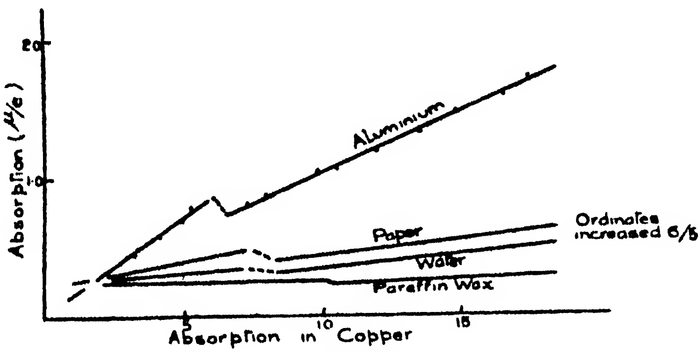


FIG. 3.

indeed emit a characteristic radiation with a wave-length of that given above. Some independent support for this conclusion was to be found incidentally in the work of Owen, C. M. Williams, and Dauvillier. One very noticeable feature, however, was that λ_J for the different elements investigated varied quite considerably. It has since been shown that all these results may be quite well explained in other ways, *e.g.* in terms of absorption in the anticathode, absorption in iodine, which was in the ionisation chamber of the spectrometer as methyl iodide, etc.

J. A. Crowther, in 1921, discussing his observations on the scattering of X-rays, suggested two alternatives to account for the softening of the scattered beam. These were : (*a*) a possible increase in wave-length, and (*b*) an emission of a softer characteristic radiation such as Barkla had suggested. He came to the conclusion that the first explanation was not consistent with his observations as nearly as the second, but he pointed out that the wave-length which it was necessary to adopt for the J radiation in the elements investigated,

namely, copper, aluminium and paraffin wax, differed from those found by Barkla.

In all the cases considered above there had been no direct determination of the emission of J radiation. In 1919 Duane and Shimizu attempted to find the J emission lines of aluminium, by making that metal the anti-cathode of an X-ray tube and examining the spectrum of the radiation given out, using the Bragg spectrometer. They covered the range of wave-length from ·18 to 1·259 A.U., *i.e.* from the upper limit of tube safety to the lower limit set by the absorption in the glass of the tube. They found no trace of a radiation corresponding to any of the values suggested in the previously described experiments. All the variations in intensity in the ionisation curves were accounted for, and they concluded that if a J radiation did exist it was less than 1 per cent. of the *general* radiation of the tube.

Richtmeyer, in absorption experiments, and Siegbahn, in emission experiments, came to the same conclusion. These conclusions had the support in 1923 of Barkla, who admitted that his attempts at a more direct manifestation of the J radiation had not succeeded, but that the discontinuities of the previous experiments remained.

The end of this phase of the subject may be very well summed up in the following quotation from the Silvanus Thompson lecture delivered by Professor Barkla in 1924 (*Nature*, November 1924): "No direct evidence of the emission by the absorbing substance of a characteristic radiation (of the J series) has been obtained. If it exists it must be very feeble. What originally appeared to be a fluorescent radiation (and to others a modified scattered radiation) has been found to be merely the scattered radiation transformed by transmission through matter after its production. The radiation is neither characteristic nor scattered, but it is a scattered radiation after having been transformed by the J process (a similar process in many ways to fluorescence with the emission of characteristic X-rays)."

The J Phenomenon

Although the position as to the lack of J radiation was quite definitely established, there still remains the sudden change in the ionisation, and in the absorption as observed in the experiments recorded. These discontinuities are now referred to as the "J discontinuities" caused by the "J phenomenon," which is, according to Barkla, distinguished from the characteristic radiation by the following features :

"(1) The phenomenon is conditional on some unidentified factors (possibly of intensity or superposition), whereas as in fluorescence Stokes's law is the only condition.

"(2) The sudden changes in absorption and ionisation appear to be absolutely abrupt, even when a heterogeneous primary beam is used.

"(3) The point at which the discontinuity occurs appears to be more closely connected with the rate of absorption of energy than with the critical wave-length (though the former, of course, depends on the wave-length)" (*Nature*, 1924).

If these characteristic features are to be taken as established, the negative results of Dunbar do not have the significance they otherwise would have. Dunbar repeated the work of Barkla and White (Fig. 3) and was unable to find any trace of discontinuities in the same substances over the same wave-length. He performed these experiments in the Edinburgh laboratories, and his work is the only recorded consistently negative results obtained there. He concluded his account of them by suggesting that "there is an alternative process of scattering," and that under the conditions of his experiments no J discontinuities appear. He used a Coolidge tube, whereas the other workers used gas tubes; in both cases the tubes were excited by induction coils run from mercury jet interrupters.

His conclusions are concordant with the results given by Barkla as supporting the first feature just quoted, *i.e.* many of the effects were obtained at one time and not at another, in fact, the discontinuities "appeared and disappeared before our eyes while we were unable to detect change in the conditions."

Below are a few of the results obtained in support of the three characteristic features of the J phenomenon. Fig. 4 (*a*) shows the relation between the absorption in aluminium and an estimate of the wave-length for two beams taken at the same time but in different directions from the anti-cathode of a tube which was hardened and softened to cover the range shown. It will be seen that a break occurs in both beams when μ/ρ is reduced to ·7 and not when the estimated wave-length is the same. This suggests that the discontinuity is more intimately connected with the absorption of the beam as a whole than with the wave-length. Fig. 4 (*b*) shows the results obtained under identical conditions at another time.

More support for condition (3) was claimed from a type of experiment illustrated in Fig. 5. X-rays from a tube were absorbed in aluminium, paper, and copper. When the rays had not been previously filtered a linear relation between

$(\mu/\rho)_{Al}$ and $(\mu'\rho)_{Cu}$ was obtained as the tube was softened; but it was found that if some of the softer components of the radiation were removed by filtering the beam in an aluminium

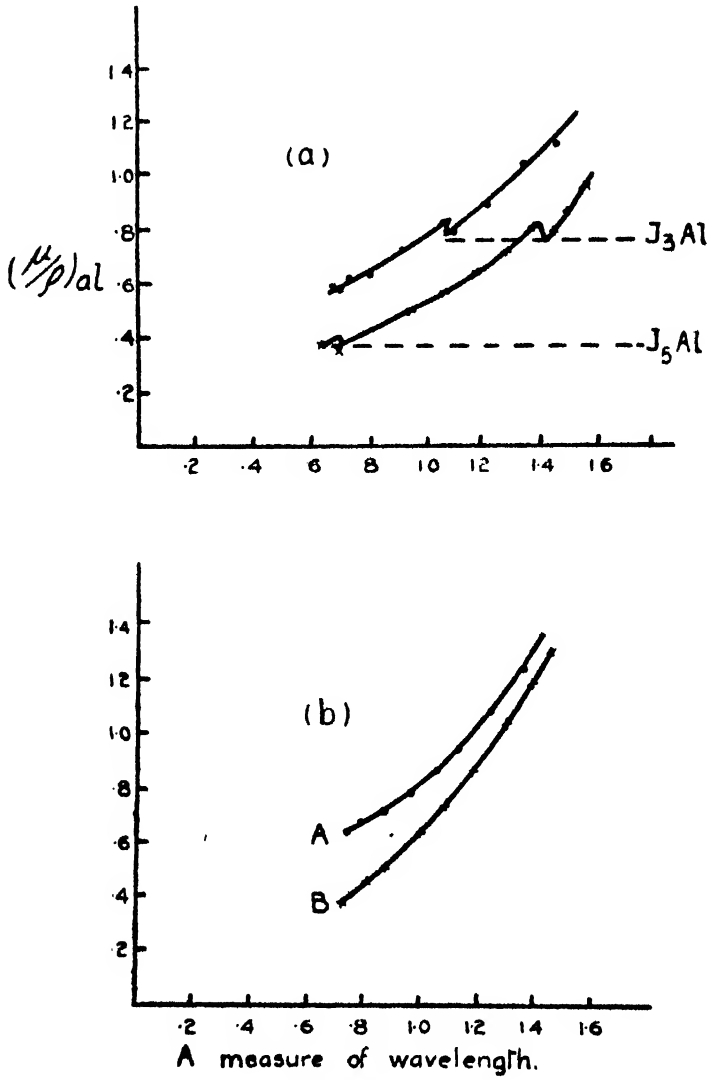


FIG. 4.

screen before measuring the absorption, a discontinuity occurred. The same thing happened with paper. It was held that the unfiltered beam had all the constituents of the filtered; the wave-lengths of the second case were present in the first, but no discontinuity occurred, and therefore it would appear that

the J phenomenon is not so much dependent on the wave-length as on the mean absorbability of the beam as a whole. It seemed to Barkla that the absorption coefficient represented a mean condition, something analogous to temperature for the cohered beam, and he often refers in his papers to the " temperature " of the radiation in this sense.

Probably a better analogy would be to refer to the " colour " of the rays, for the same mean absorption may be obtained by a mixture of a hard and a soft component as by mixture of two components of medium hardness.

In whatever way one interprets the mean mass absorption coefficient of a heterogeneous beam, the remarkable fact is

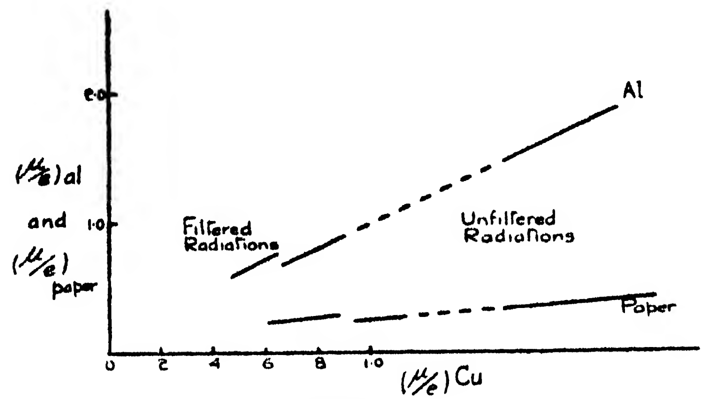


FIG. 5.

that the discontinuities, when they appear, do so suddenly. This makes an explanation of the facts in terms of the heterogeneity of the beam less convincing, and untenable in the case of sharp breaks. It looked as if the absorbing substances became temporarily more opaque in the J phenomenon.

THE J PHENOMENON IN THE SCATTERED BEAM

The classical theory of scattering leads to the conclusion that scattered X-rays are identical with the incident primary beam. While this was shown to be so in general by the early work of Barkla, discrepancies had arisen in experiments, for example in those of Barkla, and in the work of Crowther, already quoted.

Some of the discrepancies could be explained in terms of electron distribution, but there still remained experimental results which could not be reconciled, by any device, with the classical theory. To account for the growing amount of evidence of this nature, A. H. Compton put forward quantum

theory of scattering which led to the conclusion that a quantum of incident radiation, $h\nu$, after impact with an electron, lost energy equal to that of the recoil of the electron, and was consequently scattered as a quantum $h\nu'$, where ν' was less than ν, *i.e.* this theory led to one of the alternatives which Crowther had suggested, but abandoned two years previously, that there was an increase of wave-length on scattering. This conclusion was supported by convincing X-ray spectroscopic evidence.

On the other hand, Barkla and Sale, on experiments on scattering, found that " when the primary and secondary were compared by their power of penetrating thin sheets, they

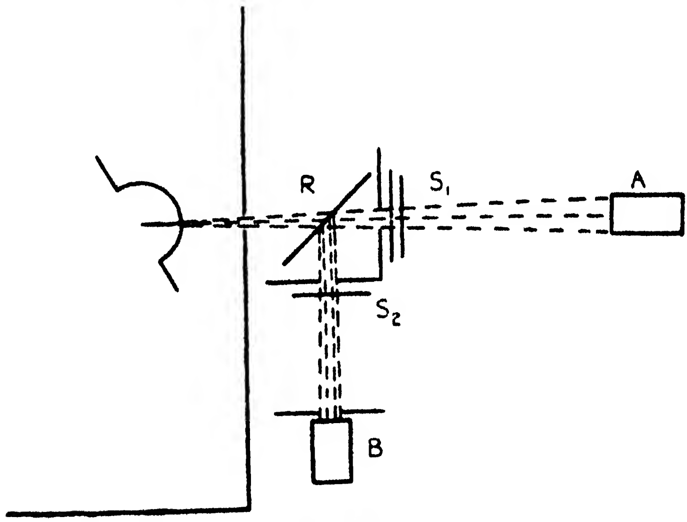


Fig. 6.

appeared practically identical; but thicker absorbing sheets showed a most decided difference (about 12 per cent. in absorption coefficient)."

The difference in the two points of view was that Compton's calculations led to the conclusion that a change in wave-length took place in the process of scattering. This change in wave-length given by the expression

$$\lambda' - \lambda = \frac{h}{mc} \cdot (1 - \cos\phi)$$

where λ is the incident wave-length, λ' is the scattered wave-length, h is Plank's constant, c is the velocity of light, and m is the mass of an electron, *i.e.* the change is independent of the wave-length. Barkla, on the other hand, maintained

that the rays were truly scattered, *i.e.* were of the same wave-length as the primary, but that the difference in hardness, when it occurred, was due to the "J phenomenon quite outside the scatterer," in fact, in the absorbing sheet used to test the radiations. In the absence of the absorbing sheets the radiation was said to be of the same wave-length as the primary beam. The difference in absorbability was, it was suggested, a result of the fact that the scattered beam showed the J phenomenon more than did the primary, and a large mass of evidence has been accumulated for such beams.

The type of apparatus used is shown in Fig. 6. R is a paper, paraffin wax, or aluminium scattered, A and B are

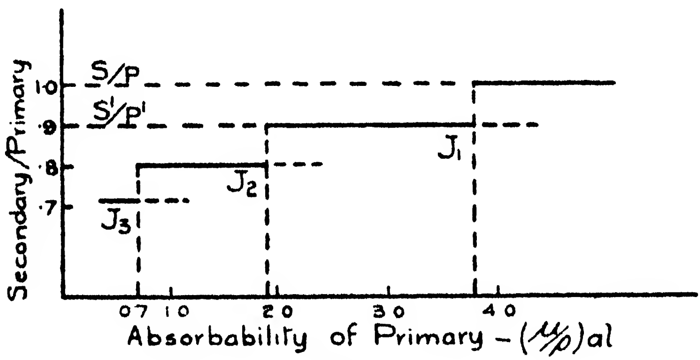


FIG. 7.

identical electroscopes or ionisation chambers, and S_1 and S_2 are equal absorbing sheets.

The experiments dealing with scattering at right angles to the incident beam were of two sorts: (*a*) the X-rays were varied in penetration and the absorbers S_1 and S_2 were made the same, and (*b*) the X-rays were kept constant and the sheets of S_1 and S_2 were increased equally.

Fig. 7 shows a typical set of results by the first method. The ratio of the ionisation in B (scattered) and A (primary) when no absorber was in the beams is called S/P; the ratio when equal thicknesses of absorber were at S_1 and S_2 is called S'/P'. By means of screens the ratio S/P was made unity. It was then found that S/P remained at this value however the primary beam was altered, as seen by the broken horizontal line in the figure. The ratio S'/P' was found to be the same for soft rays, but at a certain mean penetration of the incident ray (about $\mu/\rho = 3{\cdot}8$) there was a sudden decrease to some new value about 10 per cent. less than before. The new value held until the mean penetration of the incident beam

was of the order $\mu/\rho = 2$, when another steady value of the ratio held for a range of penetration to $\mu/\rho = \cdot 7$.

In some cases the value of S′/P′ for rays of $\mu/\rho = 3{\cdot}8$ came on the lower level, *i.e.* S′/P′ was less than S/P for all values of μ/ρ in the primary rays. This constitutes Barkla's case *b*, which is illustrated in Fig. 8.

In all cases it is seen that the value of S′/P′ was the same as S/P, or something quite definitely different, and the change took place in a sharp manner in spite of the fact that the rays used were heterogeneous.

The results of the second method, where the beam is constant for all the observations, are as represented in the typical

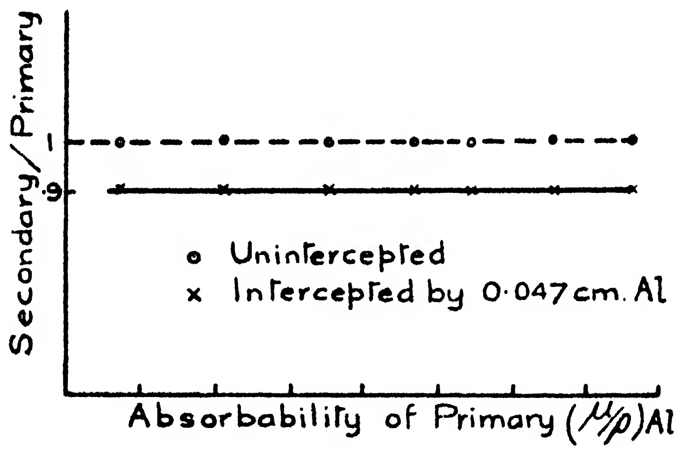


FIG. 8.

curves shown in Fig. 9. As the thickness of the absorbing sheets, which are placed in both beams, increases the ratio of S′/P′ in case (*a*) passes through a discontinuity, as shown in Fig. 9. For greater thicknesses than that required to produce the discontinuity S′/P′ decreases regularly with increase of thickness. It is clear that the ratio therefore is either the same as the ratio when no absorber is in the beams or else it suddenly changes. Alternatively the beam behaves, as in case (*b*), as though the scattered radiation were softer as demanded by the Compton theory. The position of the discontinuity, when it occurred, depended on the penetration of the incident rays which determined the amount of the absorber which it was necessary to place in the path to increase the penetration of the emerging rays to a mean value $\mu/\rho = \cdot 7$, 2, $3{\cdot}8$—the values corresponding to the J discontinuities. If a more penetrating incident radiation was used the discontinuity

moved to the left, and with very penetrating rays it entirely disappeared.

The arrangement of apparatus and general experimental conditions which resulted in observations of type (*a*) or (*b*) was indistinguishable.

An Unconfirmed Theory to Account for the Phenomenon

The above is a brief summary of the more outstanding evidence of the J phenomenon published up to the end of 1925.

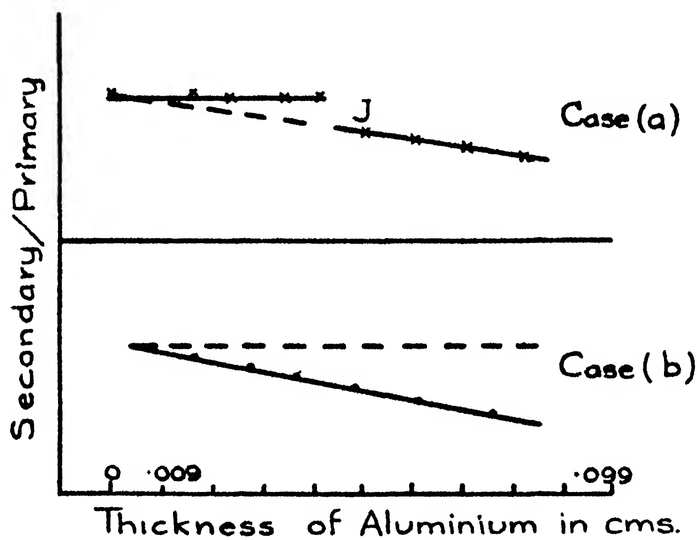


Fig. 9.

At that time it appeared to the writer that in constant repetition a strong case had been made for : (1) the sudden ionisation, due to the electronic emission at certain critical penetration of the incident beam ; and (2) a sudden absorption of the radiation under similar conditions. Whatever the phenomenon may be, it appeared to have the power of producing these two effects in the testing substance.

The fact which Barkla found, and emphasised in his papers, that the effect occurred at one time and not at another when the conditions were apparently the same led one to the conclusion that in the J phenomenon there was some new thing which perhaps might lead to the solution of the curious anomalies which had been reported from time to time by various observers on the scattering of X-rays.

The source of the electrons which Barkla had found in the J phenomenon was from some region presumably within the

K ring of the atom, for it appeared that the critical wave-length to excite this emission was shorter than the K radiation of the atom which was bombarded. Accepted atomic models had no place of origin for them unless they were ejected from the nucleus. If an electron were ejected from the nucleus the residual nuclear charge would be raised by unity, or, in other words, the atomic number of that atom would be raised by unity. This rise in the atomic number of the substance in the J phenomenon would account for the increase in absorption.

This theory, which amounts to suggesting a transient artificial radio-activity, seemed as unusual as the facts it sought to explain, but nevertheless worth testing. This was done by an examination of the optical spectra of aluminium, copper, chlorine, and mercury when these substances were bombarded by X-rays of suitable penetration to produce the J phenomenon in them (the μ/ρ appropriate to each substance was obtained from the data given in Barkla's papers).

An extensive examination, both visual and photographic, was made in the visible region, and further search was made, using a quartz spectrograph, in the ultra-violet regions. The line spectra of all the elements and the band spectra of chlorine were examined, but in no case was there any difference to be found from control spectra taken for the non-irradiated elements. In the case of aluminium and copper the irradiated elements were made the poles between which arc and spark sources were formed for spectroscopic examination. Chlorine was contained in the usual glass tube, and mercury was contained in a quartz mercury vapour lamp.

The absence of change seems to suggest either that there was no alteration in the atomic or molecular structure, or that the J phenomenon was not present in the experiments.

A similar theory was tested in Edinburgh by Watson about the same time. He used an X-ray absorption method of testing for a possible increase in the atomic number of a substance which showed the J discontinuity in another beam of X-rays. Watson also found no evidence of a change in the atomic number of the absorbing sheets.

More Recent Experiments showing the J Phenomenon

Since that time Professor Barkla and his collaborators have accumulated a large amount of evidence supporting the previous announcements with regard to this phenomenon. To go into this in detail would require a considerable space, but the results obtained may be perhaps seen in a general way from a consideration of the following, in which an attempt is made to classify them.

(*a*) Further experiments had been performed, the results of which are interpreted as showing that the mean mass absorption coefficient (μ/ρ) of the beam is more fundamental than the wave-length.

For example, the sudden discontinuities, such as shown in Fig. 9, have been found at a position which depends on the absorption of the beam as a whole rather than on its constituents. Two heterogeneous beams when mixed show a single discontinuity (appropriate to the (μ/ρ) of the composite beam)—the two breaks which the individual beams produce in the absorber do not appear when they are mixed, as might be expected to result if the discontinuity was appropriate to a wave-length.

(*b*) One rather unusual set of results showed that the discontinuity appeared to depend on the frequency of the mercury jet interrupter, but no further evidence has been brought forward in support of this.

(*c*) According to Compton's theory of scattering the change in wave-length, which he predicts and finds in spectroscopic examination, depends on the angle of scattering, *i.e.* the angle ϕ, between the incident direction and the direction of scattering. Barkla and Mackenzie, however, find that this is not the case in their experiments, but the scattered radiation when compared with the incident radiation by absorption measurement is equally penetrating until the J discontinuity occurs in the testing substance. When this occurs the scattered beam appears to be softer than the primary. S'/P' takes a new fixed value until a second or third J discontinuity occurs.

This kind of thing was obtained for several angles of scattering, and, further, the amount of change, when it occurred, was independent of the angle of scattering.

To this set of experiments, as to the others, there was the exception—the case (*b*) in which there was apparently a " modified scattered " beam. In this case the ratio S'/P' was reduced even for the first thin testing sheet. " The modified scattered beam " seemed to depend for its presence on the substance used to test the radiations. For example, Fig. 10 shows results of this kind. Here the ratio of S'/P' is plotted against the thickness of the absorber in the two beams. For different substances the thickness used was such as to produce equal reduction in intensity. This is seen by reference to the abscissæ in that diagram. It is seen that the ratio decreases gradually in the case of aluminium and copper, showing that the scattered rays are more absorbed than the primary rays, *i.e.* that the scattered beam is a " modified " beam and appears softer than the primary when tested in these substances ; but when the same scattered beam is tested in silver it appears to be

identical with the primary (unmodified) for all thicknesses of absorber used; when tested in tin the scattered radiation appears to be at one time modified and at another unmodified.

This kind of thing is further emphasised in the results shown graphically in Fig. 11. Fig. 11(A) shows a scattered

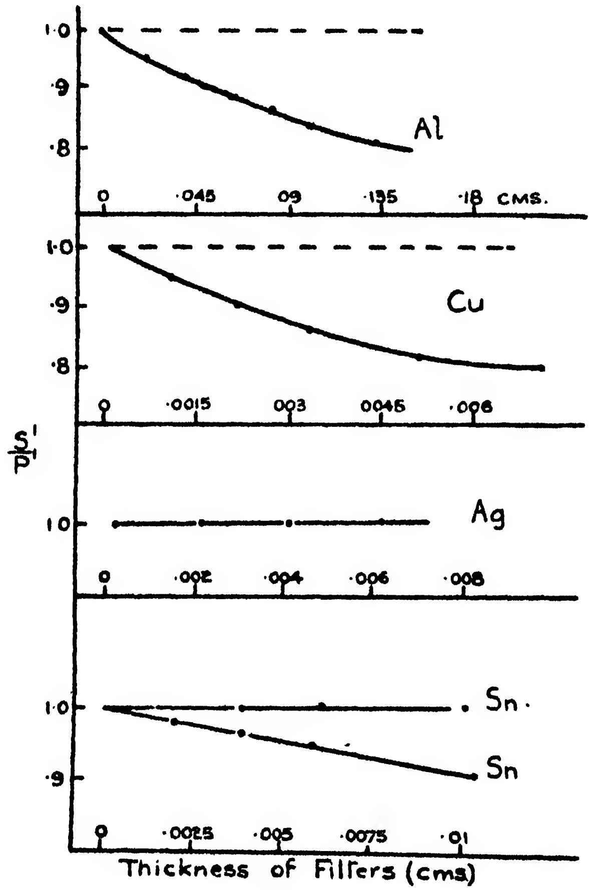


FIG. 10.

beam as a modified beam when measured in aluminium, but unmodified in an additional silver absorber, whereas when tin is added to the aluminium (Fig. 11(B)) the beam shows extra scattered ray absorption in some cases and not in others. Finally, in Fig. 11(C) is shown a case where the scattered beam is modified in paper and also is a modified beam in aluminium—the addition of aluminium to the paper causes a further decrease in the ratio S′/P′, as would be expected if the scattered beam were softer than the primary.

(*d*) Barkla and Watson thought that they had obtained control of the J phenomenon by keeping the current through the tube constant. They used a Coolidge tube in these experiments, and found more of the discontinuities (seven in all)

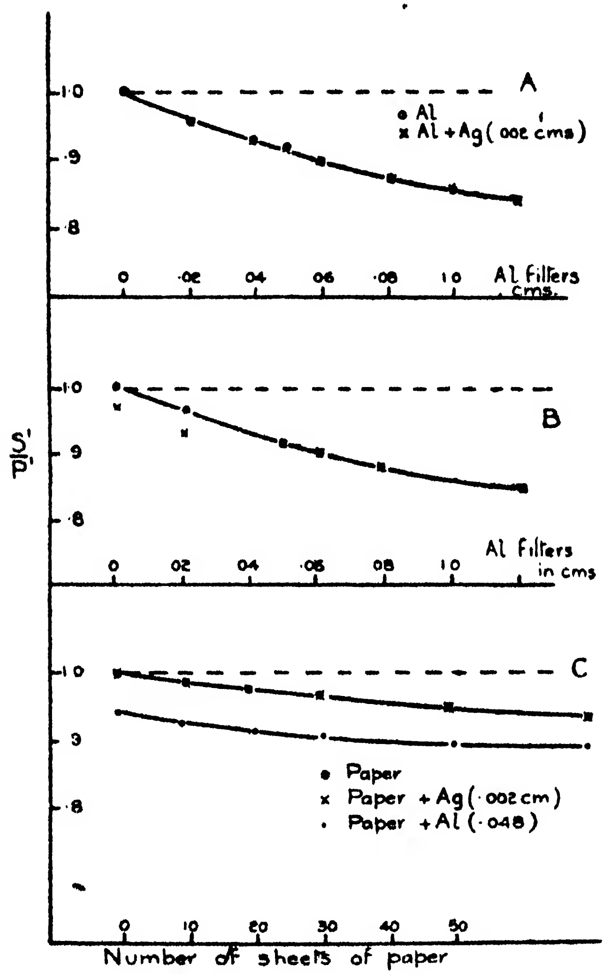


FIG. 11.

and further were able to repeat the experiments at will. The paper published in October last, however, seems to make it clear that the control is not complete.

(*e*) In the account given in the latest paper referred to, Barkla and Khastgir found in a long series of results that the scattered rays behaved as though a change of wave-length had occurred. The scattered rays appeared to be modified in aluminium, gold, silver, copper, and tin. Using the method

which produced the results shown in Fig. 7, they were able to trace the effect of the K-rays of silver and tin, which were within the range of wave-length contained in the incident beam, when silver and tin were used to test the beams. The effect in the scattered beam, which was increased in wave-length on scattering, occurred for a more penetrating incident beam than was required to produce it in the primary. For several experiments described in this paper there seemed to be little doubt that they were observing something very similar to Compton's change. It was found, however, on repeating the experiments, without making any known change in their apparatus, conditions, etc., that the results changed—results like those described in the original experiments were then obtained, viz. the scattered beam, while still modified when tested in aluminium, now became an unmodified beam in silver—the scattered beam appeared to be softer when tested in aluminium, but was identical with the primary when tested in several other substances. Incidentally this illustrates the difference in the results of Compton and Barkla. The former, according to Barkla, is observing a mass effect, whereas the absorption experiments show the intermittent nature of the process—the Compton method, as it were, sees the moving picture, whereas the Barkla method examines each picture before it is projected and merged into a general effect.

Other Absorption Experiments

In continuation of the spectroscopic tests referred to earlier in this account, the present writer tried to repeat the type of result shown in Fig. 7. Many of the original experimental conditions were retained, but a sensitive balance method was introduced and special precautions were taken to maintain the potential on the Coolidge tube at a steady value. For all potentials used the wave form was the same. After a long series of experiments, results which were consistent within themselves to within 2 per cent. did not in a single case produce a recognisable discontinuity.

A preliminary set of observations, made by F. Barraclough in collaboration with the writer, using a simple ionisation method, also failed to show any discontinuities.

O. Gaertner repeated the type of experiment of which Fig. 9 is a typical result and found that in no case was a trace of J discontinuity to be found.

A. E. Owen also tried to find the discontinuities by means of a photographic method. The beam of rays was sent through a wedge of aluminium which was photographed. The shadow image on the plate when measured with a photo-

micrometer showed a regular change in density as distinct from a sharp edge such as would be suggested by consideration of Fig. 9.

It seems, therefore, since all the positive results have been exclusively obtained in the Edinburgh laboratories, that the exact conditions for the excitation of the phenomenon have not been fully recognised. This conclusion seems to have the support of Professor Barkla, who states in several of the papers that the change from the state when the J phenomenon appears to the state when it does not appear is brought about by a process which he has been unable to recognise.

Concluding Speculations

Perhaps one might conclude this summary with a few tentative remarks and suggestions on the subject. It has been suggested in criticism of the results that the effects are probably due to slight impurities of heavy elements in the absorbing screens. But it seems highly improbable that such a state of affairs would go undetected for some ten years, and it does not explain the differential action on the two beams at right angles, giving always the same type of results. Others sought to explain the phenomenon in terms of the heterogeneous nature of the rays used.[1] Prof. Barkla suggests that the effects are intimately related with the "*coherence*" of the beam. If that is to be interpreted as meaning a close association of the waves employed, perhaps one may venture further and suggest a combination of the X-rays and some other radiation or wave system. According to the new quantum theory it appears that one must associate with a moving electron a system of waves whose wave-length is simply related to the velocity of the electron by the relation $\lambda = h/mv$, where h is Planck's constant. It may be that such waves can form something equivalent to "difference tones" with the incident beam. In the absorbing screens placed in the scattered beam in the experiments a large area is irradiated, whereas in the primary beam a very restricted area is in the beam. This might give a higher probability for such combination to the scattered beam. Photo-electrons and recoil electrons are present each with its own phase wave-length. Curiously enough such "difference tones" when calculated for individual cases produce a wave-length which, when allowance is made for a statistical factor, would be sufficient to give the change in penetration of the issuing beam which Barkla observes.

[1] A paper by Dunbar, which I have just received, in the current *Phil. Mag.*, is a case of this. Dunbar appears to have shown that a large amount of the J discontinuity may be explained on these lines.

It is also significant that Shroedinger, in explaining the interaction of radiation and electrons (when deducing the Compton effect) expresses the relation between the frequency of the X radiation and the frequency of the phase wave of the electron, before and after the impact, by the following relation :

$$\nu + mc^2/h = \nu' + m'c^2/h.$$

For an electron initially at rest this is really equivalent to stating that the scattered rays are a " difference tone " between the incident frequency of the X-rays and the frequency of the mechanical wave of the recoil electron.

These ideas are capable of some extension, but for the present purpose it seems that on these lines the appearance or non-appearance of the J phenomenon is dependent on the formation, or otherwise, by " coherence," of combination tones, and, in particular, of difference tones.

This point of view does not demand anything not already contained in the work of Barkla, as it is apparent that the non-appearance of the effect in outside experiments, and the uncertainty of the appearance of the effect at Edinburgh, show that the conditions of its excitation are not yet known.

WHEATSTONE LABORATORY,
May 1928.

REFERENCES

Barkla, Bakerian Lecture, *Phil. Trans.*, 1917, p. 315.
Barkla and White, *Phil. Mag.*, 1917, p. 270.
Owen, *Proc. Roy. Soc.*, 1918, **94**, p. 341.
Williams, *Proc. Roy. Soc.*, 1918, **94**, p. 567.
Dauvillier, *Ann. de Physique*, 1920, p. 49.
Crowther, *Phil. Mag.*, 1921, p. 719.
Barkla, *Nature*, November 1923 ; November 22, 1924.
Duane and Shimizu, *Phys. Rev.*, 1919, p. 389.
Richtmeyer, *Phys. Rev.*, 1919, p. 21 ; 1921, p. 13.
Auren, *Phil. Mag.*, 1921, **41**, p. 733.
Dunbar, *Phil. Mag.*, 1925, **49**, p. 210.
Barkla, *Phil. Mag.*, 1925, p. 1033.
Barkla and Khastgir, *Phil. Mag.*, 1925, p. 1115.
Compton, *Phys. Rev.*, 1923, p. 22.
Barkla and Sale, *Phil. Mag.*, 1923, p. 747.
Barkla and Mackenzie, *Nature*, June 1925.
Barkla and Mackenzie, *Phil. Mag.*, 1926, p. 542.
Barkla, *Nature*, March 1926 ; May 1927.
Barkla and Khastgir, *Nature*, February 1926.
Barkla and Khastgir, *Phil. Mag.*, 1926, 642.
Barkla and Mackenzie, *Phil. Mag.*, 1926, p. 1116.
Barkla and Watson, *Phil. Mag.*, 1926, p. 1122.
Worsnop, *Proc. Phys. Soc.*, 1927, p. 305.
Gaertner, *Phys. Zeit.*, 1927, p. 493.
Barkla and Khastgir, *Phil. Mag.*, 1927, p. 735.
Dunbar, *Phil. Mag.*, May 1928.

THE DISCOVERY OF THE GAS LAWS

I. BOYLE'S LAW

By W. S. JAMES, B.Sc., A.I.C.

In searching for the origins of any law expressing a quantitative variation, it must be remembered that the qualitative result must necessarily be established before any attempt to discover a numerical relationship is made. Thus the fact that air could exert a pressure at all had to be accepted before any attempt at measuring the co-variation of the pressure and volume became possible. Only when the doctrine of Nature's abhorrence of a vacuum had been overthrown by the labours of Torricelli, Pascal, von Guericke, and Boyle, could an interest in the quantitative effects arise. This interest was first exhibited by Boyle, whose attention was aroused by an account of von Guericke's work in Schott's *Mechanica Hydraulico-pneumatica* of 1657. In this book also occurred descriptions of von Guericke's clumsy air-pump and the Magdeburg hemispheres. As a result, in 1659, Hooke, under Boyle's direction, constructed an improved air-pump, thus enabling Boyle to perform many new experiments which were described by him in 1660 in a letter entitled "New Experiments Physico-mechanical, touching the Spring of the Air, and its Effects; made for the most part in a new Pneumatical Engine"; experiments with barometers were described, and following Torricelli and Pascal, he claimed that the mercury column was supported by the pressure of the atmosphere. He asserted that air had weight, and spoke of it as having a "spring."

He placed a lamb's bladder in the receiver of the pump and noticed that it became inflated as the receiver was exhausted, but contracted again when the air was readmitted.

To show that the sole cause of the suspension of the mercury in the barometrical column was the pressure of the air, Boyle filled a tube 3 feet long with mercury and inverted it in a vessel of mercury, which he then placed in the receiver, the barometrical tube passing air-tight through the cover (17th Physico-mechanical experiment). With each stroke of the pump the mercury fell lower and lower till the levels inside and outside the narrow tube were nearly the same; while on re-

admitting air into the receiver the mercury rose in the tube until the column was 29 inches again. The height of the column was thus shown to depend on the density of the air pressing on it. As the capacity of the receiver and of the air-pump cylinder could be readily determined, Boyle hoped to be "enabled to give a near guess at the proportion of force betwixt the pressure of the air (according to its various states, as to density and rarefaction) and the gravity of the quicksilver," but on account of several difficulties, mainly of a mathematical nature, he contented himself with having suggested the experiment.

Thus in 1660 Boyle had already definitely suggested a method for determining the relation between the density of the air and the weight which it could support. This stimulus made the discovery of the law now inevitable.

The Discovery of the Law

The course of events between the publication of Boyle's work of 1660 and the publication of his next work in 1662, is rather uncertain, and it cannot be definitely said who first verified the law.

We have it on Boyle's authority that Richard Townley, after reading the "Physico-mechanical Experiments," had endeavoured—acting no doubt on the suggestion in the 17th experiment—to determine the relation between the pressure and volume. He suggested that "the pressures and expansions" were in reciprocal proportions. Townley commenced a paper on the subject which he showed to Boyle, but the latter made no mention of the type of apparatus Townley employed. The paper, apparently, was never published, Townley possibly regarding this as unnecessary after the appearance in 1662 of Boyle's verification of the law.

About the middle of 1661 Boyle informed Hooke of Townley's hypothesis, and in his work of 1662, referring to the verification of the law, Boyle wrote:

". . . I had the assistance of the same person that I took notice of in the former chapter as having written something about rarefaction, whom I rather make mention of on this occasion, because when he first heard me speak of Mr. Townley's suppositions about the proportion, wherein air loses its spring by dilatation, he told me he had the year before (and not long after the publication of my pneumatical treatise) made observations to the same purpose, which he acknowledged to agree well enough with Mr. Townley's theory." This passage obviously refers to Hooke; and moreover the statements are supported by what Hooke himself wrote in his *Micrographia*

of 1664. He said that he first experimented on the subject in the summer of 1660, being then unaware of Townley's hypothesis. Although he drew no conclusions from his results, they were afterwards found to agree reasonably well with the hypothesis. After learning of Townley's suggestion from Boyle, on August 2, 1661, he repeated his experiments with a view to testing its accuracy. He verified the law and arrived at the conclusion that "the Elater of the air is reciprocal to its extension, or at least very near." For our knowledge of this latter experiment, however, we are entirely dependent upon Hooke's own account of it written in 1664. A more detailed description of his work will therefore be postponed until Boyle's experiments have been considered.

Boyle also mentioned that Lord Brouncker informed him that he had also about 1660 made experiments which agreed well enough with Townley's theory.

The following is an extract from *History of the Royal Society* by Birch, for the year 1661: "Sept. 11. Mr. Croune produced two experiments, one of the compression of the air with quicksilver in a crooked tube of glass, the nipt end of which broke; and the other . . . Mr. Boyle gave an account of his having made the former of these experiments by compressing twelve inches of air to three inches, with about an hundred inches of quicksilver.

"Sept. 18th. Mr. Boyle brought in his account in writing of the experiment made by him of the compression of air with the quicksilver tube; which was ordered to be registered."

This written account of Boyle's results is preserved in the library of the Royal Society (Register Book, Vol. I, p. 103), and is identical with the table of results for pressures greater than atmospheric only, published in Boyle's reply to Linus in 1662. He compared the pressures necessary to compress "12 inches" of air into various volumes down to "3 inches" (the apparatus will be described later) with what the pressures "ought to have been according to hypothesis." No mention was made in this paper of the nature of the hypothesis, so that Boyle must have made known the hypothesis to the Royal Society on September 11th, 1661.

Meanwhile Boyle's views of 1660 were not being universally accepted, and in 1661 Franciscus Linus, a professor in the Netherlands, in his book *De corporum inseparabilitate*, attacked Boyle's conclusions. Although he admitted that air had a "spring" as well as some weight, Linus denied that the air was capable of supporting a column of mercury 30 inches in height. To account for the properties of air and the action of the barometer, he proposed his "Funicular" hypothesis; the funiculus being an extremely thin substance, which, when dis-

tended, strongly attracted all bodies to which it was attached. The barometrical column was supported by the funiculus, and if the top of the barometer tube were closed by the finger the force due to the funiculus could be easily felt! This theory was severely criticised by Boyle in 1662 in *A Defence of the Doctrine touching the Spring and Weight of the Air . . . against the Objections of Franciscus Linus*, in which work occurred his famous account of experiments illustrating the truth of "the hypothesis that supposes the pressures and expansions to be in reciprocal proportion," the hypothesis now known as Boyle's Law. Boyle's word "expansion" corresponds to the modern word "volume." Boyle had little idea of the importance of the generalisation and the experiments were made quite incidentally in order to convince Linus that "the spring of the air is capable of doing far more than it is necessary for us to ascribe to it, to solve the phenomena of the Torricellian experiment."

The description of the first experiment can best be given in Boyle's own words: "We took then a long glass tube which by a dexterous hand and the help of a lamp was in such a manner crooked at the bottom that the part turned up was almost parallel to the rest of the tube, and the orifice of this shorter leg of the siphon (if I may so call the whole instrument) being hermetically sealed, the length of it was divided into inches (each of which was subdivided into eight parts) by a straight list of paper, which containing those divisions, was carefully pasted all along it. Then putting in as much quicksilver as served to fill the arch or bended part of the siphon, that the mercury standing in a level might reach in the one leg to the bottom of the divided paper and just to the same height or horizontal line in the other, we took care, by frequently inclining the tube, so that the air might freely pass from one leg into the other by the sides of the mercury (we took, I say, care), that the air at last included in the shorter cylinder should be of the same laxity with the rest of the air about it. This done, we began to pour quicksilver into the longer leg of the siphon which by its weight pressing up that in the shorter leg did by degrees strengthen the included air, and continuing this pouring in of quicksilver till the air in the shorter leg was by condensation reduced to take up but half the space it possessed (I say, possessed not filled) before, we cast our eyes upon the longer leg of the glass, on which was likewise pasted a list of paper carefully divided into inches and parts, and we observed not without delight and satisfaction that the quicksilver in that longer part of the tube was 29 inches higher than the other. Now that this observation does both very well agree with and confirm our hypothesis, will be easily discerned by him, that

takes notice what we teach. . . . The same air being brought to a degree of density about twice as great as that it had before, obtains a spring twice as great as formerly."

The tube described above was accidentally broken, but Boyle obtained another tube, similar in shape, but much larger. Owing to its size it was necessary to suspend it by strings on a staircase, and the lower end was placed in a wooden box to prevent loss of mercury. The mercury was poured in at the top according to the directions of the person observing the volume of the enclosed air, since it was easier to pour mercury in than to take it out after it had been poured in.

Boyle made twenty-five experiments between 1 and 4 atmospheres pressure and in each case he recorded the volume and total pressure and also calculated "what that pressure should be according to the hypothesis that supposes the pressures and expansions to be in reciprocal proportion." The observed and calculated values of the pressure agree in all cases within the limits of observational error. It was this table of results that Boyle had sent to the Royal Society in 1661.

A second series of experiments was then made for pressures less than one atmosphere. A thin glass tube was thrust down into a wider tube filled with mercury, until about an inch was left projecting above the surface. The open end of the glass tube was then sealed with sealing-wax. By raising the glass tube the "inch of air" could be dilated to an inch and a half, two inches, etc., up to 32 inches. The volumes were read directly, and the height of the mercury column noted so that from the barometric pressure, the actual pressure on the gas was computed; the pressures were also calculated from the volume according to the "hypothesis." In all the 19 results the calculated and observed pressures agreed within the limits of experimental error.

Boyle was aware of the qualitative effects of heat and cold upon air. He placed a piece of linen dipped in cold water over the air space of the bent tube used in the first set of experiments, and observed a slight shrinkage in the volume; he brought a candle flame near this part and found the air expanded. But no thermometric scale had yet been defined and even the constancy of the boiling point was as yet undiscovered. No doubt, partly on account of these difficulties, Boyle did not further inquire into the relation between volume and temperature, and the problem was next taken up by Amontons forty years later.

Hooke's Experiments

In connection with the refraction of light through the atmosphere, Hooke, in his *Micrographia*, gave an account of

some of his experiments on the relation between the pressure and volume of a constant mass of gas. The *Micrographia* was submitted to the Royal Society in 1664 and was published in 1665. According to Hooke, he first experimented upon this subject in the summer of 1660. These experiments were concerned with pressures less than atmospheric and his apparatus was the same as that used by Boyle. He took a small glass tube of uniform bore, "about the bigness of a swan's quill, and about four feet long." This tube, having been thrust down into a wider tube containing mercury, until only an inch was left protruding, had its open end cemented up. Hooke, having raised his tube until there was a mercury column of 2 inches in the narrow tube, then found that the air column had expanded by $\frac{1}{18}$th part of an inch. When the mercury column was 4 inches, the air column was $1\frac{1}{4}$ inches. These readings he continued until the mercury column was 27 inches, and, since the barometric height was 30 inches, this corresponded to a pressure of 3 inches on the enclosed air. Altogether he obtained 19 results, and this table but served as a sample of several others that he obtained. At this time he was ignorant of Townley's hypothesis, and the table was not intended to test or verify the law. Nevertheless, the results down to a pressure of about 6 inches are in quite good agreement with what we would expect from the law; but the last six readings show increasingly large deviations from the ideal.

About a year later he became aware of "Townley's hypothesis," and he tells us that on August 2, 1661, he repeated his experiments in order to test its accuracy. Not only did he test the law for pressures less than one atmosphere, but also for pressures up to two atmospheres, once more using Boyle's apparatus, in this case the "siphon" tube. He compressed 24 volumes of air to 12 volumes by a mercury column 29 inches long, the barometric pressure for that day also being 29 inches, and he also took intermediate readings. He then expanded 24 volumes of air to 1,152 volumes, and compared the value of the pressure corresponding to each volume, with the calculated value from the hypothesis. The results agreed within limits of experimental error, down to pressures of $\frac{5}{8}$ inch of mercury. From these figures Hooke thought it safe to conclude that "the Elater of the Air is reciprocal to its extension, or at least very near."

Whether Hooke verified the law before Boyle it is impossible to say. In the first place, certain doubt must be attached to Hooke's statement of 1664 that he verified the hypothesis on August 2, 1661, as other instances of Hooke making false claims are known. We do not know the date of Boyle's experiments, which were described to the Royal Society on September

11th, but they might have been performed before Hooke's. But, in any case, the credit must certainly go to Boyle. For Hooke was at this time an assistant to Boyle and obtained the news of Townley's hypothesis from him; and also no doubt he worked with Boyle's mercury and Boyle's glass tube; and, most important of all, Boyle's results were published two years before those of Hooke.

At first sight it seems odd that Hooke made no reference to Boyle's verification of the law; and that contemporary scientists did not make a full investigation into the rival claims of Boyle and Hooke and definitely clear up the question. But it must be realised that the importance of the law was not, and could not, at the time be fully appreciated. Both Hooke and Boyle brought in the accounts of their experiments as subsidiary points which had a bearing upon the more important matter then being dealt with. At this time the existence of other gases than air had only been dimly foreseen, and any practical application of the law to the theory of gases was, of course, out of the question. Had Hooke realised its future, he would certainly have put up a better claim for the discovery of the law than he did.

We may, after this survey of the position, without any injustice to Townley, Hooke, or others, support the popular title of the hypothesis—" Boyle's Law."

Work of Mariotte

In 1676 appeared Mariotte's book, *Discours de la Nature de l'Air*. He asserted that air had weight, and for a proof showed that if the cistern of a barometer is immersed in water to a depth of 3½ feet, the length of the mercury column increases by 3 inches. Since this increase is due to the weight of the water on the cistern, the previous barometrical column must have been due to the weight of the atmosphere. He then maintained that air can be condensed and expanded and has " la vertu de ressort," or " spring." Mariotte thought that if air was allowed to expand sufficiently it would arrive at its " extension naturelle." Air at the top of the atmosphere has its " extension naturelle," but near the surface of the earth it is compressed by the air on top of it. From which it follows, although he did not draw the conclusion, that Mariotte's law would only be an approximation. He mentioned that although the spring of most materials weakened with time, that of air was always the same.

Then followed the generalisation which has since been named " La Loi de Mariotte " and " Mariottesches Gesetz."

Mariotte gave the law the following form: " L'air se condense à proportion des poids dont il est chargé."

Mariotte gave an account of experiments to prove the law for pressures less than one atmosphere, using the same apparatus as Boyle. He gave figures for one or two simple cases, where the volume was doubled and increased by one-third. He said that he performed several other such experiments and they all supported his law. This he thought sufficient evidence for his law to be considered " une règle certaine " or " loi de la nature." As additional evidence he then performed experiments for pressures greater than one atmosphere, again using the same apparatus as Boyle, but, unlike Boyle, gave again only two simple numerical cases to support his conclusion. The rest of the book deals with the solution of certain problems depending on the new law, with the causes of the variations of the barometric pressure, and with the solution of air in liquids. Mariotte had a far clearer conception of the law and of its importance than did Boyle; and his book had a far greater beneficial effect on the progress of Physics on the Continent than did Boyle's publication.

Boyle's Law, or Mariotte's Law?

A great deal of confusion and misunderstanding still persists on the Continent concerning the discoverer of this law. In spite of Boyle's work having been published fourteen years before Mariotte's, many modern textbooks claim that the law was discovered by the two scientists " simultaneously " and independently; Jamin's *Cours de Physique* says that Boyle only worked with pressures greater than atmospheric; and Violle's *Cours de Physique*, 1884, informs us that Boyle did not formulate any clear law from his numerical results; while some histories wholly ignore Boyle in this connection and credit Mariotte with the discovery. Thus Maire, in his *Histoire des Sciences Mathématiques et Physiques*, 1884, gave an account of Boyle's works without mentioning the law, and ascribed its discovery to Mariotte. Similarly Libes, in his *Histoire Philosophique des Progrès de la Physique*, 1810, represented Mariotte as sole discoverer. On the other hand, it has been stated in English textbooks that Mariotte plagiarised Boyle's works, and reproduced many of Boyle's results, including the law, as his own; and a strong case for this view can certainly be put forward.

In the first place, Mariotte's book itself contains a large amount of intrinsic evidence supporting the accusation. A comparison of it with Boyle's writings reveals many similarities as well as giving a general impression of some connection.

Mariotte used the same apparatus as Boyle did, both for

pressures greater and less than atmospheric, but Mariotte placed the latter experiments first, and each used a barometer tube 40 inches long. Both used the same peculiar similes to describe the properties of air. Boyle likened the power of self-dilatation of compressed air to "the power . . . in a dry *spunge* compress'd"; and Mariotte said, "On peut comprendre . . . cette différence de condensation de l'air, par l'exemple de plusieurs *éponges*. . . ." Regarding the compressed state of the air near the earth's surface, Boyle said, "This notion may perhaps be somewhat further explained by conceiving the air near the earth . . . resembled to a fleece of *wool*"; while Mariotte wrote: "Pour expliquer en général la raréfaction et la condensation de l'air . . . on peut concevoir que l'air est quelque chose de semblable à du *coton*. . . ." Boyle spoke of air as having a "spring," while Mariotte ascribed to it a "vertu de ressort." Mariotte, like Boyle (at least in his defence against Linus), dismissed in a few sentences the effect of heat on the volume of the enclosed gas. Nor is this an isolated instance, for another similarity occurs between a paper on percussion read by Wren before the Royal Society and published by them, and Mariotte's book, *Traité de la Percussion, ou Choc des Corps*, published shortly afterwards.

That Boyle's work soon became known to foreign scientists is shown by James Bernoulli in his book, *De Gravitate Aetheris*, published at Amsterdam in 1683. He referred to Boyle's discovery of the law. Prof. P. G. Tait has said that Mariotte "must have been fully cognisant of Boyle's celebrated controversy with Linus."

It has been suggested that Mariotte did not claim to have actually discovered the results set out in his book, and that he was merely summing up the knowledge then available in the various branches of physics. But Mariotte's writing does not give this impression, for his style is authoritative and carries an air of finality and infallibility, and nowhere in his books does he mention either Boyle or Wren!

The possibility exists that Mariotte obtained the ideas for his work from Hooke and not Boyle. This is, however, improbable owing to the greater similarity of Mariotte's work with Boyle's than with Hooke's. Boyle's works were circulated on the Continent in Latin and would reach Mariotte more easily than the English *Micrographia*, or its German translation.

Prof. P. G. Tait, in the appendix to his *Properties of Matter*, has drawn attention to a remarkable passage written by Newton in the *Principia*: "Sed et veritas comprobata est a D. Wrenno coram Regia Societate per experimentum Pendulorum, quod etiam Clarissimus Mariottus Libro integro exponere mox dignatus est." This has been translated as follows, with A.

standing for D. Wrenno, and B. for Clarissimus Mariottus: " A. established the truth by means of a simple experiment, before the Royal Society; later B. thought it worth his while to write a whole book to prove the same point." The passage appears to be highly sarcastic at B.'s expense. The sarcasm is increased by the reference to " Clarissimus Mariottus," while in speaking of Wren, Huygens, etc., only the prefix " D." is added.

Tait's conclusions respecting Mariotte can be deduced from the following quotations from his *Properties of Matter*:

" He [Mariotte] seems to have been a splendidly successful and very early example of the highest class of what we now call Paper-Scientists.

" . . . Boyle, though perhaps he can scarcely be said to have been ' born great,' certainly ' achieved greatness '; the assumed parent of La Loi de Mariotte has as certainly had ' greatness thrust upon ' him."

POPULAR SCIENCE

HOW MUSCLES CONTRACT

By RONALD CAMPBELL MACFIE, M.A., M.B., C.M., LL.D.

"In the muscle nature has produced a machine so startling and at the same time so perfect, that the explanation of its mechanism could give satisfaction not only to the enquiring mind, but also promise a rich harvest to the technical progress of mankind."—Prof. Otto Meyerhof.

Almost all the energy in the world comes from the sun. The wind, the waves, the waterfalls, the rivers, oil, petrol, and coal have all the force of the sun in or behind them. Even the tides raised by the moon have sun-force lifting them, for the moon tugs at the sea with energy originally derived from the terrific tug it received when the sun wrenched and tore it out of the molten earth. It is true that our satellite is a falling body that falls round the earth under the pull of the earth's gravity; but it could not fall unless it had first been wrenched and heaved into the sky by the force of the sun, and its moving, falling force is consequent on that original wrench and heave, and is rightly to be considered solar, just as much as the flight and force of a flung cricket ball are rightly to be considered human.

Also the muscular energy of the human heart and limbs is really solar, for it is derived from the chemical energy in the sugar "glucose" manufactured by the digestive organs from the sugar and starch of plants. The sugar and starch of plants, again, as we know, are built up by the radiant energy of the sun, and retain that energy in chemical form. Human energy, accordingly, is derived from the chemical energy in glucose, derived from the chemical energy of plant sugar and starch—energy derived, in turn, from the radiant energy of the sun.

The digestive processes by which plant starch and sugar are converted into the sugar glucose are complicated and ingenious, and all the machinery of the body—heart, respiration, nerves, liver, pancreas, thyroid—plays a part in them; but here we will not attempt to describe them: we shall start with glucose ready-made.

Made in the stomach and intestines, glucose is carried by

the blood straight to the cells of the liver and by these talented cells is changed with the aid of "insulin" into a kindred substance called "glycogen," and in that form (still retaining sun-energy) is stored for future use. As the muscles require fuel and in proportion to their fuel requirements, the cells reconvert the glycogen into glucose again, and the glucose is carried to the muscles in the blood-stream and there, after reconversion into glycogen, is built up into the material of the muscle fibres. How the liver learns the requirements of the muscles is not known: it may draw its own conclusions from the chemical character of the blood reaching it, or it may have information conveyed to it in some way by its nerves. Anyhow, the liver transforms glucose into glycogen and doles it out again as glucose, according to the requirements of the muscles.

The energy, then, put to use in muscular contractions is the sun-energy of the glucose carried to the muscles in the blood and built up there into glycogen. That much is known for certain; but exactly how the chemical energy of the glycogen is converted into the mechanical movement of muscles, and into heat, is a very difficult question.

At one time it was believed that it was combined with oxygen (contributed to the blood by respiration) to form in the muscles an unstable explosive material called "inogen"; and the act of muscular contraction was supposed to be caused or initiated by a kind of explosion of the inogen. But a few years ago Sir Walter Fletcher and Prof. F. G. Hopkins brought forward a new theory which has now gained almost general acceptance.

According to this theory, the glycogen (or glucose) is not built up with oxygen into an unstable and explosive substance, but itself explodes in contact with the muscles. In the muscles its explosive energy is probably transformed into movement and rearrangement of the molecules of the muscle fibres of such a nature as to cause muscular contraction and to produce heat —much as the explosion of petrol-spray moves a piston and produces heat.

Muscular contraction is always followed by the appearance of lactic acid, and carbon dioxide, in the muscle; and these are products of the disruption of glycogen. They in themselves suggest an explosion of glycogen, and a disruption of glycogen is sufficient to explain their occurrence, and, accordingly, "inogen" would seem to be superfluous.

But there is a difficulty. Under ordinary conditions glycogen is as stable as cane sugar, and it does not explode. Why then does it explode in muscle? What breaks it up?

The explosion is certainly not of the nature of a combustion:

it is not due to oxidation, for muscular contraction takes place even in the absence of oxygen. What then sets the spark to the gunpowder?

The question has not yet been finally and satisfactorily answered, but it is possible that stimulation of muscle by nerve impulse, or by chemical or mechanical stimuli releases certain "enzymes" or "katalysts," which cause the disruption of glycogen. A few years ago some German workers discovered in muscle juice an "enzyme" containing phosphorus which had the property of breaking up carbohydrates like glycogen with the formation of lactic acid, and which they named accordingly "lactacidogen." They found, too, that if a sodium phosphate salt were given by the mouth to promote the formation of this "enzyme," capacity for muscular work was increased. Another German scientist, called Meyerhof, has extracted from muscle an enzyme which forms lactic acid from starch and glycogen at two-thirds the rate it is formed in muscles themselves. It is possible, therefore, as we have said, that the cause of the break-up of glycogen—the spark to the gunpowder—is a special "enzyme," and it is possible, too, that enzymes from the thyroid, pancreas, supra-renal, and pituitary glands play some part, direct or indirect, in the explosion.

However affected, the breaking up of glycogen into lactic acid liberates energy, and this energy, as we have already suggested, is the main agent in muscular contraction. It is possible, also, that the energy acts chemically through the lactic acid, for it is known that lactic acid applied to a muscle causes its contraction.

How exactly the lactic acid causes contraction of the muscles is not known, but Prof. A. V. Hill has made the interesting suggestion that the lactic acid acts by taking from sodium and potassium salts of protein, the sodium and potassium. The protein is altered in its electrical quality by the robbery, and instead of repelling as hitherto the other elements in the muscle, it attracts them and thus causes contraction of the muscle. (At one time it was suggested that lactic acid acted by increasing surface tension, but recent researches have shown this theory to be untenable.)

We must, then, regard glycogen with its store of sun-energy as the petrol of the body, whose explosion gives energy to the muscles, and we must give up the old idea of an explosive substance containing oxygen, and the still older idea that the energy of the body is derived, like the energy of a coal fire, from the energy of oxidation or combustion. It is true that the oxidation or combustion of glycogen would provide plenty of energy in the form of heat. One gram of it burned gives off

in heat enough energy to raise 5,332 tons to the height of one foot (about half as much as a gram of petrol), but the conversion or degradation of glycogen into lactic acid also produces energy—enough energy per gram to lift 1 cwt. 6,528 feet—and that conversion or "degradation" *not oxidation* is the source of muscular energy. A little more energy is also produced by the combination of some of the lactic acid, as it is formed, with sodium of the sodium bicarbonate salts that are present in the muscle to form sodium lactate.

Nevertheless, though the source of muscular energy and heat is not combustion of glycogen, but its conversion into lactic acid and sodium lactate, yet oxidation does play a very important part in the *maintenance* of muscular energy and temperature. Experiments on frogs' muscles prove that they will contract more effectively and for a longer period if they are kept in air or oxygen than if they are kept in nitrogen gas, and we all know that violent muscular exercise is associated with deeper and faster breathing.

What, then, is the part played by oxygen and oxidation in muscular contraction? It does not, as we have said, play a part in the actual explosion. What, then, does it do?

Sir Walter Fletcher showed that the main function of the oxygen carried by the blood to the muscles was to burn up an excess of lactic acid.

Lactic acid, as we have said, is probably the chemical energy used in muscular contraction; but if it be formed in too great quantities it becomes ineffective, and a condition of muscle fatigue or even muscle death follows. To some extent excess of lactic acid is neutralised by alkaline salts and proteins in the blood; but under conditions of considerable muscular effort, great quantities of glycogen are destroyed: great quantities of lactic acid form; and the body, by deeper and faster breathing, provides more oxygen and endeavours to burn up or change the lactic acid. In moderate exercise, the lactic acid is neutralised or oxidised as fast, or almost as fast, as it is formed, but in severe and violent exercise it is formed so fast that, in spite of greatly increased oxygen supply and oxidation, it accumulates, and impairs, or abolishes the contractile power of the muscle. A muscular man of average weight can maintain muscular activity till the lactic acid reaches a weight of about 3 ounces (90 grammes), but at, or about, that point his muscles fail.

Oxygen, then, takes no part in the actual explosion or degradation of the glycogen, but it plays a very important part in the control and removal of lactic acid, and renders possible muscular work otherwise impossible.

Part of the lactic acid is burned by the oxygen—burned

away into water and carbon dioxide—and with the energy so released the rest of the lactic acid is built up into glycogen again. Out of every 6 grammes of lactic acid removed, one is burned, and, with the energy of its burning, 5 grammes are built up into glycogen. A very economical arrangement!

All the lactic acid which accumulates in a contracting muscle and in the blood has to be removed by oxidation before the muscle reaches its full fitness, and so, whenever lactic acid accumulates there is really an oxygen debt which has to be paid off sooner or later. This debt is paid off during periods of rest. It may be as much as several litres of oxygen, but the heart and the lungs work away while the fatigued muscles are resting, and in from five to eighty minutes pay off all the debt; and not only pay off all the debt, but build up reserves of energy in the form of reconstructed glycogen. In eighty minutes at longest all the ashes, so to speak, have been cleared out of the grate, and new coal put in the coal-cellar.

Heat is, of course, produced during contraction, relaxation, and rest, and the heat produced during relaxation is entirely or almost entirely oxidative.

During violent exercise when lactic acid is formed in active muscles in great excess, some of the acid leaks into the blood and is carried to other parts of the body, and when local muscular exercise causes general fatigue it is possible that the general fatigue may be partly due to such leakage and dispersion of lactic acid. But the lactic acid of leakage, like the lactic acid formed in the active muscles, is quickly oxidised away. Indeed, in a sense it may be said that the fatigue of active muscles puts energy in the form of glycogen into passive muscles, for part of the lactic acid of leakage is built up into glycogen—distant lazy muscles get sugar from the fragments of the fatigued ones.

It is interesting to note that, under normal conditions, the muscles of the heart do not run up an oxygen debt. This is fortunate, and what might be expected, for if they did run up such a debt, every athlete taking violent exercise would run the risk of heart failure from excess of lactic acid. The heart muscles do not run into debt probably because they are abundantly supplied with blood coming straight from the lungs and rich in oxygen—so rich in oxygen that any lactic acid formed in the heart muscles is oxidised as quickly as produced.

This brief outline of the mechanism of muscular contraction will perhaps serve to show the fascinating character of modern research into its mystery.

The sun-energy of glycogen—energy originally imparted to it by the sun in some chlorophyll cell—is released in the muscle by an enzyme which degrades it into lactic acid, and the libe-

rated solar energy is changed in some still obscure way into the energy of muscular contraction. The contraction of a muscle in fact is the direct consequence of the leaping of an electron in an atom in the sun.

As the glycogen breaks down and liberates the trammelled energy, lactic acid is formed and accumulates to a greater or less extent, and the oxygen brought to the muscles by the blood burns up a sixth of it and converts the other five-sixths into glycogen again. Lactic acid—the same acid that is formed in sour milk—is thus revealed as one of the most interesting and important acids in the world. Every man running hard or doing hard muscular work of any kind is forming drams of lactic acid, and after his labours becomes partly a refuse furnace, and partly a starch manufactory, for part of the lactic acid is being burned away, and part, with the energy of the fire so produced, is being manufactured into glycogen.

LICHEN DYEING TO-DAY: THE REVIVAL OF AN ANCIENT INDUSTRY

By A. R. HORWOOD, F.L.S.

Few people, when they casually notice the lichens which grow on trees, giving their trunks a ragged hoary appearance, or those which hang in beard-like masses from the branches, or those that grow in patches on stones, often of a brilliant orange colour, realise that these lowly plants have considerable economic value. Their use in dyeing has long been known, however, and though commercially profitable aniline and synthetic dyes have largely replaced many of the native or foreign vegetable dye substances, lichen dyeing is still carried on as formerly in those parts of this country and abroad where homespuns are made by peasantry, and the wool used is dyed by the makers of tweeds and other cloths.

At the present time lichens are used for dyeing in England, the Hebrides and Western Isles, Shetlands, in Scotland, Ireland, the Isle of Man, Wales, France, Norway, Sweden, Germany, the United States, and Iceland.

Lichens are mainly now used for dyeing wool or yarns, rarely linen, stockings, for colouring egg shells for "Easter eggs," and for dyeing homespuns. The peculiar smell of Harris tweeds, which are dyed with lichens, is due to the mode of preparation of the dyes.

Besides being used as dye plants lichens are also used to form dye substances called Orchil and Cudbear, which are substantive dyes not requiring a mordant, obtained from lichens known as orseilles in the Canary Islands and elsewhere, where they grow abundantly on the rocks by the sea. These dyes are purple or red. Not being permanent they are made so by addition of alum or some other fixing agent.

Dye lichens are best gathered in autumn or winter, and after a shower of rain, especially the rock lichens, as they can be better detached then. But reindeer moss, which is a heath lichen, is gathered in May or June, and scrottyie, a common stone lichen, is gathered in August in the Shetlands. Lichens are first washed, then dried, and reduced to a fine powder. To one part of powdered lichen and one part of fresh quick-

lime (to ten parts of lichen) are added twenty-five parts of pure water. To 10 lb. of lichen ½ lb. of sal ammoniac is added when lime is used. For a few days the vessel is kept covered up. If some salt or saltpetre is added the brilliance of the colours is intensified. The Highlanders distinguish the tree lichens as "wood-raw," or "rags," whilst the stone lichens are called "stone raw." A metal scraper is used to scrape off the stone lichens. Rock lichens yield a better dye than tree-growing lichens of the same species.

Although the native homespun workers have used lichens from remote times, the use of these valuable but little-known dyes has been confined to these islands and the Scandinavian region generally. French dyers do not use the lichen plants, and in works on dyeing they are not included. Orchil and Cudbear, however, which are prepared from lichens, have been known generally and are used in large bulk, after being prepared by a lengthy process. Cudbear is prepared in Scotland from corcur and other lichens. Other lichens yielding cudbear when treated with ammonia dye a purple colour.

Common names exist for many of the different distinct lichens, but those species which can be distinguished by the lichenologist are unknown to the dyer, and as a rule he uses several species under a general name.

Some common rock lichens go under the name of scrottyie, or crottle, or crotal, and there are several of these, one dyeing yellow, being the stone crottle, and another is called dark crottle, giving a yellowish-brown dye. A tree lichen named philamort gives an orange dye.

The Lichen Colours and the Lichen Dyes

From the variety of colour displayed by all the groups of lichens from dull stone to bright orange with fruit organs (or apothecia) sometimes of a scarlet colour, one might suppose that lichens of a yellow colour would yield a yellow dye, but that is not so. These stone-coloured lichens yield brown dyes in some cases and the grey tree lichen may give a yellow-brown dye. The yellow wall lichen dyes wool a plum colour with a mordant. A grey wall lichen gives a red or crimson or scarlet dye.

Therefore, the colour of a lichen is no criterion as to the colour of the dye it will yield. Moreover, a particular lichen may yield dyes of different colours according to the manner in which it grows, or its habitat, or owing to a difference in climate, or altitude, proximity to the sea, or the reverse, age, and the time of gathering. A basin-shaped brown lichen growing on rocks yields a red dye, and cudbear lichens of a

grey colour afford purple dyes as noted. Hence the colour of a lichen bears no relation to the colour of the dyes it affords.

Scottish Methods used in Lichen Dyeing

Various methods for obtaining a dye from lichens are in vogue in different districts in Scotland and the West of Ireland, where the peasants collect them from trees and rocks and dye the Harris tweeds which have so rich an aroma. When they want to get a brown dye they collect the common crottle lichens. They gather them in July and August. The lichens are first dried in the sun for wool dyeing, and no preparation is necessary. All they do is to put the crottle in a bath of water, boil it, and let it cool. It is then boiled with the wool until the material is of the desired brown colour. These dyes are very fast, and no fixing agent or mordant is needed. The actual dyeing process takes several hours, as the wool does not quickly absorb the substances, an advantage gained in no other way except by the use of a mordant. The dye substance has no deleterious effect on the wool. The dyeing is accelerated by adding acetic acid, which brings out the colour.

A very similar process is followed in using one of the rock lichens to obtain a yellowish-brown dye. The lichen is boiled in water for a day. Into this the following day the wool is placed and boiled until the wool is of a rich yellow-brown colour. The colour is richer the longer the wool is left in the dye, but is sometimes uneven when the wool is steeped too long.

In the North of England the lungwort, a lichen known as rags, and stone rags is used to obtain a dark brown dye for wool, and requires no mordant. The substantive dyes are more permanent, but give a less brilliant colour, the latter being acquired by use of such a mordant as ammonia. If the wool has been dyed dark blue with indigo first, the addition of this lichen dye makes a permanent black dye.

In the Shetlands the process of dyeing is closely similar. There the common scrottyie is collected in August. The lichen is put in a vat, or iron vessel, and this is filled up with stale urine. It is boiled for two hours until the mixture has the consistency of a mucilage, and on cooling it is like a jelly at first, and then becomes thicker and of a dark rusty grey colour. A layer of this is put on a layer of cloth and boiled for twenty minutes in soft water containing a little alum. After being taken off the fire the cloth is washed in cold water, and the dyeing is then complete. The lichen dye may be used several times to dye cloth. When a red dye is required, the lichen known as corcur, or korkalett, is used in Scotland. It is collected from the rocks

on the moors in May and June. The lichens are steeped for three weeks in a vessel containing stale urine or ammonia, at a low temperature. The substance becomes thick and tough as dough, and is bluish-black. It is made into cakes of 12 ounces, wrapped in dock leaves, and hung up to dry in peat smoke. In a dry state its efficiency lasts for over a year or more. It is prepared for use by soaking in warm water until it is jelly-like, and the cloth is dyed in the same manner as above. The dyed cloth is light red. Yarn is dyed with this lichen as well as cloth. For dyeing purposes yarn is boiled in the mixture.

To obtain a claret dye the same lichen is used in the Highlands in the following manner. The lichen is pounded and mixed with ammonia or stale urine, and salt or seaweed added. This is kept for several weeks, being stirred from time to time, until it is like paste. It is then made up into balls with lime, and is kept thus for use as required. It is powdered and some alum added to it when required for dyeing.

Commercial Products

Orchil and cudbear are the commercial products from certain lichens. Formerly they were prepared from British material. They afford purple and red dyes of a fugitive nature, without the use of a mordant. These substances are obtained by the action of ammonia and oxygen upon the crushed lichens. In Scotland, from early times, the dye colour was obtained by the action of stale urine and slaked lime upon the lichen.

Orchil is used for dyeing wool by boiling it in a neutral or slightly acid solution. The acid used is usually sulphuric acid of 3 per cent. strength ; or else alum and tartar. Orchil gives an even dye, as it is slowly absorbed. Orchil is used to dye wool before indigo is used, and also gives a compound colour for wool and silk. Owing to its fugitive nature, orchil is rendered more fast by addition of tin as a mordant. Orchil gives a rich colour, the deeper the longer it is applied, but all the more fugitive, and thus requires tin to fix it.

Cudbear is prepared from native lichens in the same way as orchil is prepared. The corcur and another lichen yield cudbear. A purple colour when ammonia is used is also afforded by some other lichens, such as the ragged hoary lichen, blistered Umbilicaria, and others. The lichens are pounded and water and ammonia and slaked lime added, and kept at a low temperature for two or three weeks until the whole ferments.

Both orchil and cudbear can be obtained in the following manner. The lichens are carefully washed, dried and thoroughly cleaned. They are reduced to a pulp in water, until

fine or coarse as the case may be. Ammonia in some form is then added. The liquor is stirred so that it is thoroughly exposed to oxidation. As the colour may need toning or altering, potash or soda is added as required, whilst consistence is acquired by adding gypsum or lime. Heat is applied to cause the substance to ferment.

Current Lichen-dyeing

There are several good methods of dyeing with lichens which will produce the shades of brown, red, or terra-cotta or tan for cloth, now so fashionable for men's or women's clothes or cloth.

Using ordinary crottle lichens, take a quantity of wool, and have vats or vessels for immersing the stuff in of varying strength, containing different quantities of crottle, or crotal, to give a light or a dark shade as required. After putting the wool in the vessel it should be boiled for an hour. At first the colour will be light tan by dipping in a strong solution of crottle, but if this is boiled for half an hour the shade will be darker, and if left in for two hours, quite a dark brown colour is the result. It is a good plan to add a small quantity of oil of vitriol.

A red dye is obtained by placing layers of crottle and wool in a vat, in water, and then boiling until the colour desired is obtained, without using a mordant. The longer the wool is left in, the darker the shade will be. Terra-cotta shades can be obtained in this way quite easily.

The common yellow lichen on cowsheds, which acquires its colour from the presence of a greater amount of ammonia in the atmosphere in their vicinity, yields a pink dye, by use of a mordant consisting of 3 per cent. bichromate of potash. The lichen in this case is boiled for just over an hour.

A red purple dye for wool is obtained by using cudbear and logwood in equal parts after employing a mordant of chrome. To get a lighter colour one can use 8 lb. of cudbear and ½ lb. of logwood for 30 lb. of wool.

A yellow dye for linen is obtained from a grey heath lichen by using a mordant of alum, one part in four, and boiling for two hours. The lichen is added and the wool and dye boiled until the proper shade of yellow is obtained.

ESSAY

COAST EROSION IN EAST ANGLIA (J. Reid Moir, F.R.A.I.)

It has been truly said that any land surface, if exposed for a sufficiently extended period to the action of the sun's heat, frost, wind, and rain, will finally be disintegrated, washed away by rivers and streams, and deposited in the sea. This process of destruction is, of course, operating in a greater or lesser degree upon all the existing land surfaces of the earth; but its action is so gradual, and its results so seemingly small, that there is no need for us to trouble ourselves about it. It is, however, otherwise with another more rapid, and obvious form of land removal which is proceeding, at certain places, round the coasts of Britain, and, because of its magnitude, it is necessary that we should try to understand how and why this removal is taking place, in order that, if possible, measures should be taken to prevent it. We have all, no doubt, heard of what is generally called coast erosion, though those who live at inland resorts, and are thus not brought face to face with its baneful activities, do not look upon it with the apprehension as do others who dwell near the sea.

It is upon the east coast of England where, frequently, the cliffs are composed of beds of sand, gravel, and clay, that the land is now, and has been for a great period of time, disappearing into the sea. A study of the ancient records of this part of England shows that large and flourishing towns and vast areas of country have, sometimes with devastating suddenness, been destroyed, and even to-day, in spite of the ingenuity of man in erecting protections of various kinds, parts of the east coast are vanishing with melancholy rapidity. Six hundred years ago the harbour of the important town of Dunwich in Suffolk was totally destroyed, and four hundred of its houses carried away by the sea. Church after church suffered a like fate, and, recently, the last of these ancient edifices of Dunwich disappeared over the cliff. The fate of this place is, however, but one item in the long list of disasters which have occurred up and down the east coast, and the danger of further trouble is by no means past.

For many years I have been able to make a close study of this subject of coast erosion, and have sometimes witnessed

[Photo, Tansley, Sheringham.

VIEW OF THE RECENT GREAT SLIP OF CLIFF AT CROMER.

Showing how the fallen material extends from the cliffs across the beach.

immense falls of the cliffs, which, without warning, and with a thunderous roar, have swept across the beach into the sea. I have seen, also, the slow slipping down of even greater masses, bearing habitations and fertile land upon their surfaces. Recently, at Cromer, an area which I know intimately, there has been a disastrous fall of cliff, the débris of which now extends to almost half-tide mark. The accompanying photograph, taken immediately after this fall, gives a graphic idea of the manner in which the destruction of land is proceeding along the east coast. Now, it is usually supposed that the sea is solely responsible for coast erosion, but while this agent of destruction has not much to its credit in the matter, yet it can only be arraigned as an accessory after the fact. In order to understand this question, let us imagine a vertical cliff, about 100 feet in height, composed of alternating layers of sand, gravel, and clay. When such a cliff is exposed to atmospheric conditions, certain inevitable and destructive forces at once begin to operate upon it. In the first place, the wind, blowing upon the face of the cliff, begins to remove the smaller particles of the layers of sand and gravel, with the result that these deposits gradually crumble and fall, and accumulate in a sloping heap at the foot of the cliff. This process is aided considerably by the effects of frost, and of rain, and, before long, the sandy layers will be partially removed, and the intervening, and more resistant, strata of clay will stand out from the cliff face in the form of ledges. The next stage in the process of destruction is now reached when these ledges, unable to support their own weight, break off, and so increase the heap of fallen material at the foot of the cliff. Thus, by these means alone, an unbroken, and more or less vertical cliff, can very soon be partially destroyed. But, though this is the case, the destruction so produced is as nothing compared to that brought about by another agent, whose activities I now propose to describe. The water which falls, in the form of rain and snow, upon a land surface sinks through that surface until it reaches some impervious stratum, and then proceeds to run along the slope of this deposit in whatever direction it may lead. The result of this process can be seen very clearly in operation in cliffs formed of clay and sand, as springs, produced by water falling upon the land surface at the top of the cliff, issue, frequently, at considerable depths below the surface. Now, this underground water running along the top of the deeply buried impervious layer, acts as a lubricant, and causes great masses of cliff to slide downwards, and forwards, in the direction of least resistance—that is, towards the sea. Upon the east coast such happenings are of common occurrence, and these slips generally, for some unknown reason, take a

semicircular form, with the result that land some distance from the cliff face, is involved in the downward movement. There cannot be any doubt that these underground springs of water are the chief cause of the destruction of such cliffs as have been described, and it is very difficult to imagine any means by which this destruction can be prevented. Thus far we have seen how a vertical cliff may be altered and destroyed by certain natural processes, and it will be realised that in this *dénouement* the sea has taken no part whatever. In fact, any cliff of the kind under discussion, wherever situated, must be affected, inevitably, by these destructive forces, and, strange to say, when they have done their utmost the cliff is left protected from any further attacks of a similar nature. For, as the bluff disintegrates, a sloping bank, its slope conforming to the angle of rest of the material composing it, is formed against its face, and becoming, in the course of time, covered and held together by vegetation, remains in position indefinitely. When, therefore, the sea-cliffs of the east coast have passed through the primary stages of destruction, they usually present to the sea a sloping face, which, if left untouched, would prevent any further loss of land. In the ordinary course of events the sea does not reach the foot of the slope, but when a combination of exceptionally high tides and violent gales from the north-west takes place, the protecting slope is washed away—the cliff, in consequence, once more presents a vertical face which is immediately acted upon by the natural agents of disintegration, and the whole process of destruction is enacted again. At certain places, where a township exists, the municipality, in order to protect itself against the loss of valuable property, erects, at great expense, sea-walls and groynes, which, if intelligently built, and rigorously kept in order, do undoubtedly afford protection to the cliffs.

But such measures are quite beyond the means of most private individuals, and, during any period of storm, they may see extensive portions of their property disappear into the sea. In this way thousands of square miles of East Anglia have been lost, and every year which passes sees the grim total mounting steadily. Whether it is possible, without the expenditure of an overwhelming amount of money, such as would be involved in the construction of sea-walls, to put a stop to these destructive processes is a debatable question, but its urgency is glaringly apparent, and it must be remembered that, while man contemplates and considers, the forces of nature are proceeding relentlessly on their course, and working their will upon our fast receding coast-line.

NOTES

Obituary: Dr. Hideyo Noguchi

Dr. Hideyo Noguchi is said to have died of yellow fever at Accra, May 21st, 1928, during his investigation of that disease. Some of our readers may remember that Walter Myers of the Liverpool School of Tropical Medicine died of yellow fever at Para in 1901; and also that Professor Adrian Stokes is said to have died of the same disease last September. Dr. Noguchi thought that he had found a spirochæte, but we understand that there is much doubt on the matter. Dr. Noguchi was born in Japan in 1876, and was educated at Tokyo University. He had been connected with the Rockefeller Institute since 1914, and was the author of some important advances in Pathology.

Fishery Investigations

We have received an interesting report on the Flow of Water through the Straits of Dover as gauged by continuous current meter observations at the Varne Light Vessel (50° 56′ N.–1° 17′ E.). Part 1. Methods employed with a Preliminary Survey of the Results. By J. N. Carruthers, D.Sc., Fisheries Laboratory, Lowestoft. (Ministry of Agriculture and Fisheries. Fisheries Investigations. Ser. II, Vol. XI, No. 1. 1928.)

The apparatus used by Dr. Carruthers is the "Drift Indicator" designed by himself to show the water movements past a fixed position, such as a light vessel. In principle the "Drift Indicator" is a modification of the "Ekman Current Meter," but instead of the propeller it has four rotating cups and works for a longer period. The instrument takes up the direction of water flow by means of a vane and the current is measured by recording the number of turns made by these cups due to the passing flow of water. As the cups revolve a mechanism is set in motion which counts and records the revolutions and also indicates the direction of the current by means of a compass with a heavy magnetic needle. Shot are inserted into a hopper leading to a wheel or to a perforated ball which rotates on a spindle, and at each revolution of the wheel or ball one shot is discharged into a slot on one of the arms of the magnetic

needle where it rolls along a groove and falls into a box divided into sixteen radiating compartments. The magnet remains North and South; the instrument including the box moves in the direction of the current, the angle between the partition in which the shot has dropped and the axis of the needle indicating the direction of the current. The number of shot in each compartment shows the number of units of distance the current has flowed in a certain direction.

The Indicator has now been brought to such a high standard that it can be used continuously for many months, each record being reliable and usable.

The object of the present research was to use this "Drift Indicator" from the Varne Light Vessel situated in the middle of the Straits of Dover to secure an unbroken series of current meter records relating to the water exchange between the English Channel and the North Sea via the Straits of Dover, over a period of several years. The Varne Light Vessel being of wood is specially suitable, as current meters containing magnets could be used at quite shallow depths. The advantage of the present instrument is that it registers direct observations without the necessity of eliminating the periodic tidal water movements mathematically, is robust enough to keep in working order for several months without repair and to withstand all kinds of rough weather, and is simple enough to be used by ordinary seamen.

A long series of continuous records is now available. The chief aim in reporting on these was to arrive at a clear idea as to the relative importance of the customary E.N.E. going flow and the occasional W.S.W. going flow, the stream being prevalent but the E.N.E. flow being much the larger and overwhelmingly more important than its reversal. The results show that anything may happen with regard to the current in the Straits of Dover between a flow of 16·8 miles a day from Channel to North Sea and a flow of 11·9 miles a day from North Sea to Channel. The flow from Channel to North Sea is least in summer and gains strength in winter. Summer is the season of weakest flow, autumn greatest. The greatest reversals are in winter. It is estimated by the author that enough water comes into the North Sea annually by way of the Straits of Dover to form a layer 13¾ feet thick over the whole superficial extent of the North Sea, and that the North Sea would in the course of twenty years receive through the Straits of Dover as much water as it (the North Sea) at present holds.

The importance of this work in connection with the fisheries is obvious, although no application of the results obtained to individual fishery research problems has been made in this

report. Information such as the dispersal of young herring spawned in the eastern end of the Channel and of the distribution of plaice eggs, besides the transport of plankton, being cases in which a thorough knowledge of water movement is invaluable. As the author says, " It is hoped and believed that the known cases of reversal of usual flow of water from the English Channel to the North Sea will serve to explain features in the plaice and herring stock age composition some years hence," and " the results obtained are, it can be justly claimed, of great potential value in connection with certain problems of fishery research, and the necessary applications will be made."

Science and Practice

It is interesting to study the different ways in which wonderful discoveries are put into practice, and to note how often there is a long delay before the value of such discoveries are taken advantage of. In these notes mention has often been made of the wonderful discovery by Sir Ronald Ross of the transmission of malaria by the infected Anopheles mosquito.

America was the first nation to recognise the enormous value of his discovery, which led to the possibility of building the Panama Canal. The other wonderful example is the work of the disciple of Sir Ronald Ross, Sir Malcolm Watson, which has placed Malaya far ahead of any other British dominion or colony in the matter of malaria control work. The reason for the backwardness in making this scientific discovery of practical value has been the lack of co-operation and of general practical study in dealing with the destruction of the malaria-bearing mosquito.

We noticed recently in *The Times* mention of a memorandum which has just been issued by the Ross Institute and Hospital for Tropical Diseases, a copy of which has since been sent to us. The Memorandum announces the formation of a powerful body which is called the Ross Institute Industrial Anti-malarial Advisory Committee. The Ross Institute has acquired the whole-time services of Sir Malcolm Watson, and he has been appointed principal of the Malaria Control Department of the Institute. In order that the value of the experience of Sir Malcolm Watson may be put to the most useful purpose, the Advisory Committee is formed of the three Directors of the Institute, and Sir Malcolm Watson, and lay members representing the Indian Tea Association, the Rubber Growers' Association, the Ceylon Association, Malayan Tin and Rubber Companies, Nigerian Tin Companies, Indian Gold Mining Companies, Indian Coal Mining Companies, Indian Jute Mills,

Indian Cotton Mills, Indian Iron and Steel Works and Power Companies, the British Cotton Growing Association, Rhodesian Base Metals Companies, South African Mining and Farming interests, East and West African Industries, West Indian Companies, Shipping Companies, Power Companies, Railways, etc. This means that tropical industries will have their representatives, to whom they may bring all their troubles connected with malaria, and the central body, with branches all over the tropics, will give to these the scientific experience at their disposal.

Sir Malcolm Watson and the organiser, Major H. Lockwood Stevens, are being sent to India and Ceylon for several months, and are leaving in November, and it is the intention of the Institute to send Sir Malcolm Watson in an advisory capacity to the tropical parts of the Empire for several months each year. The foundation of this Advisory Committee is the outcome of recommendations put forward by Sir Ronald Ross, who has advocated this step for many years past, and it is generally recognised that the Empire has not benefited sufficiently by his discovery. It is felt by a growing number of people that the progress in Malaya for the past twenty-eight years abundantly proves the advisability of associating, in the future, research and active malaria-control as closely as possible.

The study of malaria in Malaya has shown how it carries with the physical features of the land and the species of anopheles inhabiting it. Even in a country so small as the Malay Peninsula, malaria was associated with various zones of land (*e.g.* salt water, mangrove swamps, fresh water coastal swamps, the ravines of coastal hills, the inland plains, and the inland hills), in certain definite ways, depending on whether or not the virgin jungle was intact or destroyed. That gave ten types of lane which had to be studied. In addition, rice fields of three types were discovered, giving a total of thirteen different types of land. Some twenty-one different species of Anopheles had to be classified in the laboratory, and studied in the fields. This research showed that probably only four or five were actually associated with the spread of malaria in nature on any considerable scale, and that expenditure on the control of others was unnecessary.

Finally, practical methods had to be devised for the control of malaria. These included the most widely different methods; in some places the strict preservation of jungle, in others elaborate engineering schemes, the application of larvicides, drainage, or flooding. In India, malaria is associated with other types of land, *e.g.* the tanks surrounding villages in the great plains, the rivers and hills of the Assam Valleys and

the wells in large cities. In other countries, other conditions associated with malaria have been discovered, each requiring special consideration.

At the Congress of the Far Eastern Association which was held in Calcutta in December 1927, it was unanimously carried that for towns, mines, plantations, large public works, and similar aggregations of people, the control of breeding places of the malaria-carrying species is a method which should be employed whatever other anti-malarial measures are put into force and wherever possible this control should be effected by permanent works which eliminate entirely the source of mosquito breeding.

Enormous sums of money have been saved to the Governments and industrial undertakings in Malaya by the control of malaria. It is certain that in each succeeding year there will be an ever-increasing demand for the control of malaria, both by the Governments and industrial undertakings in other countries. The expert staff to undertake this work is extremely limited, and the chief problem at the present moment is how most rapidly to increase it. Without trained research medical officers and expert engineers, the attempt to control malaria will be unduly expensive, and in many cases will end in failure, if not an actual aggravation of the disease. This was a lesson dearly bought in Malaya.

The First English Aeronaut.

The history of Aeronautics is always of interest. According to *The Times* of March 27, 1928, the first English aeronaut was James Saddler of Oxford, who died a hundred years ago, aged seventy-five. By that time his fame as a balloonist and his other achievements were largely forgotten. He was not only a notable Oxford worthy, but was also a many-sided engineer and inventor. Born at Oxford in 1753, his first thirty years of life are almost forgotten, except that he was a confectioner and was acquainted with Tom Warton, the Poet Laureate, who ascended in a balloon with him. The first ascent made was one at Paris in November 1783, by Pilâtre de Rosier. After 1785, however, he gave up ballooning and turned to chemistry, and became assistant to the Professor of Chemistry at Oxford in 1789, and then Chemist to the newly created Board of Naval Works at the Admiralty until 1807. He also tried to improve James Watt's invention of the steam-engine and made a pump for docks in Portsmouth dockyard. He also invented an improved air-pump, a new method of blasting for mining, and a furnace for heating shot, a new naval gun, and a new method of rifling the bore of muskets. Some of Sadler's guns were fitted on His Majesty's ships. His pension was suppressed

without compensation, and he fell upon hard times, the usual fate of world benefactors. In 1812 he crossed the Irish Channel from Dublin in a balloon. He eventually returned to Oxford and died there.

Dunces, Dogs, and Motor Horns (R. R.)

In the October number, 1927, I complained of the dog nuisance. Since then there have been many letters in the papers, but nothing appears to be done by our lawgivers. Yet, oddly enough, our lawgivers always like to have opportunities for taxing the public. Why, then, do they not take the opportunity to get a good result out of taxation, as well as to make some more money, by increasing the dog licence to at least one pound a year for one dog, three pounds a year for two dogs, six pounds a year for three dogs, and so on, with a reduction for professional dog breeding? We fancy that the tax would then prove very agreeable to our revenues. For all we know there must be some millions of dogs in this country.

We also complained of motor-horns and pointed out that they are not only a nuisance to residents but also are meant to scare away pedestrians in order to enable cars to rush at full speed everywhere. It is supposed that the horns prevent accidents; but really they probably increase the number of them, because they enable motorists to drive faster than would be otherwise possible. If cars had no horns, drivers would have to be more circumspect. Why are horns allowed at all? They are not required for carriages. Meantime the list of horrible accidents continues to maintain its level; and a man was killed just outside the address of this Quarterly in August. As I suggested before, some city or town should prohibit the use of motor-horns altogether within its limits, as an experimental measure. The public would probably welcome much more stringency in the granting of driving licences.

Mrs. W. G. Nott-Bower, Richmond Hill, is one of the numerous complainants in *The Times*, August 14, of the dog nuisance. One wonders why our politicians do not try to get rid of such nuisances on behalf of the public which supports them. The object of Parliament is, as we interpret it, to deal with all national troubles and nuisances as much as it can; but it is one defect in the party system, that our politicians seem to be much more interested in party quarrels than in useful work.

Notes and News

The Honours list published on the occasion of the King's Birthday contained the following names of interest here: *Baron*—Sir Alfred Mond. *Knights*—Dr. J. H. Jeans and

Capt. G. H. Wilkins (the Australian aviator, who recently flew across the Arctic Ocean). *Companion of Honour*—Prof. J. S. Haldane of the University of Birmingham. *G.B.E.*—Sir John Dewrance, past president of the Institution of Mechanical Engineers. *C.B.E.*—Dr. C. H. Lander, Director of Fuel Research ; Mr. H. E. Wimperis, Director of Scientific Research, Air Ministry. *O.B.E.*—Professor A. V. Bernard, Medical Officer of Health, Malta ; Mr. E. R. Sawer, Director of Department of Agriculture, Palestine, Dr. W. S. Tucker, Director of Acoustical Research, Air Defence Staff ; Mr. R. McK. Wood, principal scientific officer, Royal Aircraft Establishment, Farnborough. *M.B.E.*—Mr. W. R. Black, Ministry of Agriculture and Fisheries ; Dr. J. F. Corson and Mr. G. Maclean of the Medical and Sanitary Department, Tanganyika Territory.

Lord Melchett of Landford (Sir Alfred Mond) and Sir William S. McCormick have been elected Fellows of the Royal Society under the statute which permits persons to be elected who have rendered conspicuous service to the cause of science or whose election would be of notable benefit to the Society. In addition the following have been elected foreign members of the Society : Prof. Albert T. J. Brachet, rector of the University of Brussels and professor of anatomy and embryology ; Prof. David Hilbert of Göttingen, mathematician ; Prof. Paul Langevin of Paris, physicist ; Prof. Richard Pfeiffer of Breslau, bacteriologist ; Prof. Prandtl, professor of applied physics, mechanics, and thermodynamics at Göttingen ; Prof. Richard Willstätter of Munich, chemist.

Sir Ernest Rutherford has been elected an Associate of the Royal Academy of Belgium and an honorary Fellow of the Institute of Physics. In addition he has been awarded the Albert Medal of the Royal Society of Arts.

Sir Ronald Ross has been awarded the Harben Gold Medal of the Royal Institute of Public Health.

Sir William J. Pope has been elected Prime Warden of the Goldsmiths' Company.

The Council of the Royal Society of Edinburgh has ordered the Keith Prize for the period 1925–7 to be divided equally between Prof. T. J. Jehu and Mr. R. M. Craig for their joint work in the Geology of the Outer Hebrides ; the Neill Prize for the same period has been awarded to Prof. A. Robinson for his contributions to Comparative Anatomy and Embryology and the Bruce Prize for the period 1926–8 to Prof. H. U. Sverdrup of Bergen for his contributions to our knowledge of the meteorology, magnetism, and tides of the Arctic.

The Victoria medal of the Royal Geographical Society has been awarded to Mr. E. A. Reeves, the cartographer.

Dr. E. J. Holmyard, who on several occasions has contributed articles on the history of chemistry to SCIENCE PROGRESS, has received the degree of D.Litt. from the University of Bristol.

We have noted with regret the deaths of the following well-known men of science during the part quarter: Dr. Charles Chree; Prof. E. M. Crookshank, emeritus professor of bacteriology, King's College, London; Prof. T. B. Freas, chemist, Columbia University; Dr. H. F. Gadow, F.R.S., morphologist, Cambridge University; Prof. Léon Guignard, botanist and one time president of the Paris Academy of Sciences; Prof. L. Harrison, Australian zoologist; Dr. J. Horne, F.R.S., geologist; Dr. H. Noguchi, at Accra, of yellow fever, while investigating that disease; Prof. O. Nordenskjöld, Swedish explorer; Dr. F. M. Perkin, C.B.E., chemist; Prof. C. G. J. Petersen of Denmark, marine biologist; Mr. W. E. Plummer, director of Liverpool Observatory; Prof. A. Schönfliess, of Frankfurt-on-Main, mathematician; Prof. G. Schultz, of Munich, chemist; Prof. E. F. Smith of Philadelphia, chemist; Dr. J. A. Thomson of New Zealand, geologist; Sir John I. Thornycroft, marine engineer; Dr. W. A. Young, director of the Medical Research Institute, Gold Coast, of yellow fever, probably due to infection whilst making a post mortem examination of Dr. Noguchi, who was working with him.

Sir J. H. Jeans has consented to give the first Henry Herbert Wills Memorial Lecture at the University of Bristol on October 30. The Lecture has been founded by the Council of the University in commemoration of the donation of £200,000 given by the late Mr. H. H. Wills for the erection of the new Physics Department.

The portrait of Sir Charles Sherrington, painted by Mr. R. G. Eves, was presented to the Royal Society on June 21 last by Sir John Rose Bradford, on behalf of the subscribers.

The following is a list of the principal officers appointed to act during the session 1928–9 by the Societies named:

Institute of Physics: President, Sir Frank Dyson; *Honorary Secretary*, Prof. A. O. Rankine.

British Institute of Radiology: President, Dr. G. W. C. Kaye. *Honorary Secretaries*, Dr. S. Meville and Dr. G. Shearer.

Faraday Society: President, Prof. T. M. Lowry.

Linnean Society: President, Sir Sidney F. Harmer. *Honorary Secretaries*, Dr. G. P. Bidder (Zoology) and Mr. John Ramsbottom (Botany).

Institution of Electrical Engineers: President, Lt.-Col. K. Edgcumbe.

Royal Society of South Africa: President, Prof. W. A. Jolly, of the University of Cape Town. *Honorary Secretary*, Prof. A. Ogg.

The Royal Commissioners for the Exhibition of 1851 have awarded senior studentships to Dr. T. E. Allibone (physics) and Mr. L. S. B Leakey (anthropology) both of Cambridge University; to Dr. G. E. J. Temple (mathematical physics) Imperial College of Science; to Mr. B. Cavanagh (chemistry), Victoria University, Manchester, and to Mr. C. E. Wynn-Williams (physics), University College of North Wales, Bangor.

Senior Fellowships of the value of £700 per annum have been awarded by the Beit Memorial Trustees to Dr. A. S. Parkes of University College, London, and Dr. Honor Bridget Fell. Junior Fellowships (£400 per annum) have been given to Dr. J. H. Quastel, Mr. B. H. C. Matthews and Mr. D. R. McCullagh, all of the University of Cambridge, also to Dr. P. W. Clutterbuck and Dr. W. R. Aykroyd of the Lister Institute.

Fellowships of the Salters' Company have been awarded to Mr. W. K. Cameron and Mr. H. Diamond of University College, London, Mr. A. H. Loveless and Mr. H. Smith of the Imperial College of Science, and Mr. F. L. Gilbert of Nottingham and Cambridge.

The Fourth General Assembly of the International Research Council was held in Brussels on July 13. The two important items on the agenda were (1) questions arising out of the invitation to Germany, Austria, Hungary, and Bulgaria to join the Union, (2) consideration of the procedure to be adopted when the present Convention comes to an end in 1931.

The "enemy" countries were invited to join the Conference by a unanimous resolution passed in June 1926. So far, however, only Hungary has accepted. Bulgaria regrets that it is too poor to participate in the expense of international work, while Germany and Austria, showing little appreciation of the concession offered them, have referred the matter to the Verband der Deutschen Akademien. This body apparently declines to recommend acceptance of the invitation on the ground that the Council is an Association of governments, rather than of scientific bodies. The objection seems somewhat artificial.

The convention ends unless it be renewed in 1931, and a committee has been appointed to consider any changes which it may then be desirable to make in the Statutes.

A meeting of the International Astronomical Union was held at Leyden during the summer. Sir Frank Dyson was then elected president for the next meeting, which will be held in the United States in September 1932—a time chosen to enable the visitors to be present during the total eclipse of the sun, visible in New England and Eastern Canada, on August 31 in that year.

At the annual general meeting of the Institute of Physics, on May 15, it was stated that there had been a substantial increase in the membership of the Institute during the previous year, and that the *Journal of Scientific Instruments had become* firmly established. Dr. C. V. Drysdale, the editor, resigned (chiefly on account of ill-health) and the Secretary of the Institute, Mr. T. Martin, was appointed to be his successor. From January next the *Journal* will be distributed to Fellows of the Institute without charge, and to Associates at a much reduced subscription rate.

The meeting of the Royal Society on June 14 was remarkable for the fact that all the papers read at the meeting—seven in number—were due to Prof. A. V. Hill and his students. They dealt chiefly with experiments on various aspects of muscular activity—a subject which Prof. Hill dealt with in his usual interesting way in a recent number of SCIENCE PROGRESS.

The Department of Overseas Trade states that owing to the success of the last British Industries Fair it has been found necessary to provide an extra hall at the White City, giving 100,000 square feet of stand space in addition to the 250,000 square feet of stands occupied at the last Fair—roughly a 40 per cent. increase. In Birmingham the increase of space will bring the exhibition area to about 250,000 square feet, making a total area for both sections of the Fair of over 600,000 square feet. The Fair will begin as usual on the third Monday of February—the 18th in this case—and will end on the Friday of the following week—March 1. Public interest at White City was so great this year that it has been decided to open the next Fair to the public at four o'clock instead of five each evening.

The Thomas Gray Trust was recently established in memory of the late Thomas Gray, C.B., formerly Assistant Secretary to the Board of Trade, Marine Department, for "The advancement of the Science of Navigation and the educational interests of the British Mercantile Marine." It is administered by the Council of the Royal Society of Arts, which decided to offer a prize for Navigation of the value of £30 for competition between selected candidates from the three principal training establishments for boys intending to become officers in the Mercantile Marine, viz. the Nautical School, Pangbourne, the Training Ship, *Worcester*, off Greenhithe, and the Training Ship, *Conway*, at Birkenhead. The prize, which has been offered this year for the first time, has been awarded, upon the result of an Examination recently held, to Cadet-Captain John Herbert Chalk, of the Nautical College, Pangbourne.

John D. Rockefeller, Jr., has given $2,000,000 to the University of Paris for the construction and development of a

central building in the new students' quarter. A like sum would be very welcome in London, where it is desired to create a students' quarter in Bloomsbury around the new site for the University headquarters.

Preparations are being made in Germany for another flight across the North Pole. A giant airship, the LZ 127, now under construction at Friedrichshafen, is to be used. This airship is expected to have a cruising radius of 8,000 miles, and will work from a base right outside the Arctic Circle. It yet remains to be seen whether the scientific results of the Nobili expedition justify the risks run by explorers and rescuers alike.

The members of the British Association Great Barrier Reef Expedition left London for Brisbane on May 26. The main objects of the expedition are (1) to study the conditions underlying the formation of coral reefs and (2) to investigate the economic possibilities of the Reef. In addition the standard chemical and planktonic observations made in temperate seas will be carried on in order to obtain further information about the conditions prevailing in tropical waters. With these aims in view the party will camp on the Reef and take observations over a period of thirteen months. The estimated cost of the expedition is £10,000, of which about £2,000 remained to be collected at the time of departure.

Prof. Millikan and Dr. Cameron have made further observations of the properties of cosmic rays at Arrowhead and Gem Lakes in California. From the water absorption curves they conclude that at least three bands of different wave-lengths are present, the shortest being in the neighbourhood of 8×10^{-5} Å corresponding to a generating potential of 150 million volts. It is shown in the original paper (*Phys. Rev.* June 1928) that these wave-lengths are consistent with the theory that the radiation is due to the decrease in mass which takes place when the more abundant elements (oxygen, silicon, magnesium) are formed from hydrogen or helium. The Millikan rays may thus be giving us direct evidence of the building up of the elements from simpler materials which, somewhere and somehow, must be taking place.

It is reported that Senatore Marconi and M. Mathieu have succeeded in finding means of sending three messages simultaneously with the same beam-aerial and transmitting apparatus. Such multiplex working will of course produce a considerable decrease in the cost of communication, and may even provide a solution to the problem of Empire broadcasting since Morse signals and a broadcast programme could be radiated simultaneously from the same station.

On Tuesday, July 31, the Rt. Hon. the Earl of Balfour, K.C., O.M., F.R.S., as President of the Committee of the Privy

Council for Scientific and Industrial Research, held an informal reception at the Forest Products Research Laboratory recently erected at Princes Risborough, Buckinghamshire. The object of the reception was to bring to the notice of all interested the facilities existing in the new laboratories for scientific and technical investigations aiming at the conservation and efficient use of the timber-supplies of Great Britain, both home-grown and imported, and to enable visitors to see the important work already in progress. Some 140 guests attended, representative mainly of timber merchants and users. Representatives of Dominion Governments were also present.

The Laboratory, which was completed and equipped during the past year, is now fully in operation. It comprises sections for the study of Timber Seasoning, Timber Mechanics, Timber Physics, Wood Technology, Entomology, Wood Preservation, Wood-working and Timber Utilisation. Investigations on behalf of the Laboratory are also carried out at the Imperial Forestry Institute, Oxford (Wood Technology and Chemistry), the University of St. Andrews (Chemistry) and the Imperial College of Science and Technology South Kensington (Mycology).

The work of the Laboratory is under the immediate direction of Mr. R. S. Pearson, C.I.E., F.L.S., Director of Forest Products Research, and the general programme of work is supervised by the Forest Products Research Board, under the chairmanship of Principal Sir James C. Irvine, C.B.E., LL.D., D.Sc., F.R.S., of the University of St. Andrews.

A pamphlet dealing with the uses of home-grown timbers has just been issued by the Laboratory (in conjunction with other bodies interested in the subject). It gives a list of the trees commonly found in the British Isles, together with an account of the qualities of the timber and its suitability for various specific purposes. The pamphlet will be most helpful to all interested in the production and use of timber. It may be obtained from H.M. Stationery Office, price 1*s.* net.

We have received a copy of the *Abstracts of Papers on Agricultural Research in Great Britain and Northern Ireland* for the period Oct. 1926—Sept. 1927 issued from the Ministry of Agriculture and Fisheries, and apparently prepared in connection with the Imperial Conference last year. It gives a complete review (in the usual *Abstracts* form) of all the British research on Agriculture during the year, under suitable general headings, *e.g.* Soils, Grassland, Dairying, Agricultural Economics, etc. It is stated that a copy has been sent to all agricultural research workers, whose names appear in the "List of Agricultural Research Workers in the Empire." Other copies can be obtained for 1*s.* from H.M. Stationery Office. We do not know whether a corresponding volume is to be issued each

year, but if so, workers in agricultural science are very fortunate.

The Sixth Annual Report of the Safety in Mines Research Board, that for the year 1927, has just been published (H.M. Stationery Office, price 9*d.* net). As in previous years, the Report is mainly devoted to brief accounts of the progress of the numerous researches which are being carried out, either directly under the Board or Committees of the Board, or at Universities and other centres with the aid of funds supplied by the Board. The subjects of research included problems relating to coal-dust explosions, firedamp explosions, spontaneous combustion of coal (including the treatment of gob-fires), the safe use of electricity in coal-mines, mining explosives, safety-lamps, falls of ground, wire-ropes, the control of mine temperatures, and mine-rescue apparatus. Eleven reports were published by the Board during the year, and a list is given of other papers relating to the researches written by members of the staff and published in scientific journals, etc. At the instance of the Miners' Welfare Committee the Board propose to publish and to circulate to Miners' Institutes short summaries of the results of their researches expressed, so far as possible, in simple and non-scientific language.

The Report also embodies an account of the progress of the health inquiries carried out under the supervision of the Health Advisory Committee, notably the inquiries with regard to the injurious effects of certain rock dusts.

We have received a copy (vol. ii. No. 2) of the *Cambridge University Agricultural Society Magazine* (pp. 80 + plates and diagrams : W. Heffer, Cambridge. Price 2*s.* 6*d.*). Primarily intended for members of the Society, the magazine contains many articles of general agricultural interest and much sound common sense. It deserves a place on the magazine tables of all libraries and clubs in agricultural districts.

The Cambridge University Press makes the very interesting announcement that a new edition of Sir J. J. Thomson's *Conduction of Electricity through Gases* is to be published this autumn. In the new edition the name of G. P. Thomson will also appear on the title-page. Other notable books in preparation are Prof. Eddington's Gifford Lectures on *The Nature of the Physical World*; a mathematical treatise by R. H. Fowler dealing with statistical mechanics, and a two-volume collection of Sir Joseph Larmor's *Mathematical and Physical Papers.* On the biological side there will appear the third (and last) volume of Prof. F. O. Bower's treatise on *Ferns* and a collected edition of *The Scientific Papers of William Bateson.*

Messrs. Trost Brothers, of Victoria Street, have sent us a pamphlet describing the properties of Haveg, a new material

for use in the chemical industry. It is a phenol-formaldehyde condensation product of the Bakelite class containing an acid-resisting filler (usually asbestos) which can be selected to produce a variety of chemical and physical properties. The new material withstands the action of many acids and corrosive substances, has a low specific gravity (1·6), good mechanical strength, and can be used up to 130° C. (or 200° C, with a special grade). It is normally a good insulator, but data for the thermal and electrical conductivities are not given. It can be machined and coated over a metal base.

We have received a number of the *Technical Reprints* of papers prepared by members of the staff of the Bell Telephone Laboratories (463, West Street, New York). The most interesting is that entitled *Symposium on Television,* a group of five papers describing the apparatus and methods used in the experimental demonstration of television given by the Bell Company on April 7, 1927. This pamphlet gives an advanced account of the processes involved. Another paper by J. Mills, entitled *Through Electrical Eyes,* contains a more elementary exposition. It is likely that the reader of either of them will become sceptical as to the immediate prospects of television "broadcasts."

The British Instructional Films, Ltd. (46, Brewer St., W.1.), have sent us a catalogue of their educational films. The catalogue contains 162 different titles covering a wide range of educational matter, although, as might be expected, the longest lists occur under the headings Geography and Natural History. The films may be hired at the rate of 10*s.* per reel for three days (one day for despatch, one day for showing, and one day for return). Included in the list is a 12-reel film, illustrating the mechanism of a motor-car.

A new edition of the geological map of S. Australia has been printed and may be obtained from the office of the Geological Survey, Adelaide. It is drawn on the scale of 32 miles to an inch, and embodies all the new information which has accumulated since H. Y. L. Brown prepared the last important edition in 1899.

Bulletin No. 35 of the Council for Scientific and Industrial Research, Australia, contains an account of an investigation to determine the possibilities of kraft paper manufacture in Australia. Kraft paper is a strong brown wrapping paper, made from long-fibred pine pulp, which is rapidly displacing jute and manilla paper, and is even used as a substitute for woven jute bags in the cement trade. The experiments showed that a satisfactory pulp can be obtained from *Pinus insignis*, but that the wood at present available would only supply a mill making 20 tons of pulp per day. On this basis the lowest possible

c.i.f. selling price for pulp would be £15 16*s.* 9*d.* per ton, as against £12 8*s.* for imported pulp. In ten or twelve years' time far more timber will be available, and a 60-ton mill might be supplied. The price could then be lowered to £12 per ton.

Volume xx. of the *Collected Researches* of the members of the staff of the National Physical Laboratory is now obtainable from H.M. Stationery Office, price 18*s.* 6*d.* net. All the papers included in this volume bear in one way or another an Optics. There are fourteen papers by Mr. T. Smith reprinted from the *Transactions of the Optical Society*, eight by Mr. J. Guild reprinted from the same *Transactions* and from the *Journal of Scientific Instruments*, four by Dr. Walsh, and four by other members of the staff. Of these last papers, four appear in the *Proceedings of the Optical Convention*. The papers included in the *Collected Researches* are, of course, of an important character, but they are so readily accessible elsewhere, that it is doubtful whether the expense of republication on the elaborate scale adopted by the Laboratory is justified.

Circular of the Bureau of Standards, No. 346, is a reprint of a circular issued in 1919, and deals with the physical properties of aluminium and magnesium and their alloys. It contains over four hundred pages, of which seventy are devoted to the bibliography. It is interesting to note that while in 1925 the total output of aluminium was 213,000 tons (of which 9,000 tons came from Great Britain, 25,000 from Germany, and 93,000 from the United States), the new plant of the Aluminium Co. of Canada has a possible maximum output of 300,000 tons per year! In 1852, when the metal was a novelty, its price was $5.450 per lb.; by 1896 this had fallen to $0.295, and since then it has fluctuated near that figure, the minimum being $0.131 in 1911, while in 1926 it was $0.275.

The first of a series of Founders' Memorial Lectures was given at Girton College on March 3 by Sir J. J. Thomson. The lecture has been published by the Cambridge University Press (price, 2*s.* 6*d.*) under the title *Beyond the Electron.* It contains a simple account of the new concept of the electron which has resulted from the discovery that when passed through very thin sheets of metal (less than a millionth of an inch thick) electrons are diffracted in the same way as light. This result implies that an electron is accompanied by a train of waves which has complete control over its path. The wave-length of the waves is of the order of 10^{-9} cm. Mathematically these conclusions can be treated by the methods of de Broglie and Schrödinger; the physicist will find that thet reatment given by Sir J. J. Thomson leaves a clearer impression of the meaning of the subject.

In the July number of *Tropical Life* attention is called to

the use of the Nipah Palm for the production of sugar and spirit. It is claimed that Nipah, as a producer of sugar, will leave the maple behind, and with careful and scientific tapping, will even compete with beet-sugar. Nipah produces a good white sugar, and when market conditions are unfavourable, liquid fuel or motor spirit can be manufactured as an alternative. This new source of sugar production has in the Philippines many advantages over that of cane-sugar, as the tapping of the Nipah is a very simple matter. As regards alcohol, it is stated that a yield of 1,200 gallons of alcohol per acre per annum is a reasonable estimate, although it is claimed that as much as 1,440 gallons per acre per annum can be obtained. The Nipah Palm industry has been carried on in the Philippines and Borneo for many years successfully, and it is now being introduced into Malaya.

We have received from the Director of the British Mosquito-control Institute at Hayling Island (J. F. Marshall, Esq., M.A., F.E.S., F.L.S.) The First Annual Report (1927), and are glad that Mr. Marshall's work is being continued. English mosquitoes are often a considerable pest, in spite of the fact that the English climate is supposed to be too bad even for these insects.

One of the most remarkable medical discoveries made of late is that a diet of liver or of liver extract cures pernicious anæmia, as shown in America and England. This pernicious anæmia used to be a fatal complaint, and we understand that the discovery was made by Drs. G. R. Minot and W. P. Murphy of Boston, Massachusetts, about two years ago. As liver is not very palatable, extracts from it are being made in the United States, Germany, and Britain, and are now on the market, and are said to give about equally good results. A single dose, equivalent to nearly thirty ounces of entire liver, can now be given to patients (*Science*, p. xii, May 18, 1928).

Although years have now elapsed since dengue was proved to be a mosquito-borne disease, and also the methods of mosquito control were fully worked out, yet there has been a bad epidemic of this in Athens, the number of cases being estimated at between 160,000 and 200,000 by August 27. One wonders when the politicians who manage the world's affairs are going to show enough intelligence to act upon scientific knowledge already acquired, for the purpose of saving human health on a large scale. There used to be an excellent Antimalaria Society in Greece, but we do not know what has happened to it. M. Venizelos himself has been a victim, being the seventh member of the Cabinet to suffer. Perhaps this will stimulate reform.

ESSAY-REVIEWS

SCIENCE AND METAPHYSICS IN BIOLOGY. By J. H. Woodger, being a review of **Logic and Law in Biology**, by P. Chalmers Mitchell, C.B.E., D.Sc., F.R.S., etc. [Pp. 30.] (London : Macmillan & Co., 1927. Price 1*s*. net.)

In a survey of recent observations on anthropoid behaviour it is written : " Evidences of fatigue in the apes, especially in experimental situations which require novel adaptations of behaviour, have been noted by various observers. . . . Boutan states that mental work rapidly fatigues the gibbon and induces yawning and finally sleep, and Yerkes has made similar observations on the chimpanzee and other primates."[1] Probably most people can confirm this observation so far as some " other primates " are concerned. Mr. Punch, in fact, would doubtless call it " another glimpse of the obvious," and the evolutionary biologist will have no difficulty in connecting it with the remark of Mr. Bertrand Russell to the effect that " Most people would rather die than think. In fact, they frequently do." In other words, fighting is more popular than thinking, because we have not yet got far enough from our simian ancestors. The celebrated saying of Samuel Johnson is still appropriate : " Were Socrates and Charles the Twelfth of Sweden both present in any company, and Socrates to say, ' Follow me, and hear a lecture in philosophy ' ; and Charles, laying his hand on his sword, to say, ' Follow me, and dethrone the Czar,' a man would be ashamed to follow Socrates. Sir, the impression is universal. . . ." Even in intellectual matters the fighting spirit in which emotion and rhetoric are given precedence over logic is preferred to disinterested thinking. It is difficult to find any topic upon which men can unite. Everywhere is competition rather than co-operation. It is possible to take up an attitude of detachment, if that is one's preference, towards most questions, but there is one in regard to which this is not possible. Opinion, namely, is even divided about whether toleration is to be preferred to dogmatism. Some regard toleration as the height of enlightenment, and others as the height of feebleness and folly. But is it not one

[1] R. M. Yerkes and M. S. Child, " Anthropoid Behaviour," *Quart. Review of Biology*, vol. ii, p. 44.

of the glories of natural science that it confines itself to that realm of experience in which there is hope of finding common ground and agreement, and do we not feel that it should preserve an attitude of toleration rather than set up limits which will keep thought to one groove? Huxley has said somewhere that science commits suicide when it adopts a creed.

Dr. Chalmers Mitchell, in the above Huxley Memorial Lecture, writes (p. 30): "Philosophy, since she was judicially separated from science, although retaining the title 'lover of wisdom,' has made no positive addition to knowledge and has only been a spinner of vain words and barren ideas. How far the process will go I cannot guess. Personally, I doubt if the contained can ever comprehend the container, if the desire of man to comprehend the universe be more than a phase of anthropomorphic vanity. But hopefully we must pursue our hopeless task." Prof. A. N. Whitehead, on the other hand, writes: "To neglect philosophy when engaged in the re-formation of ideas is to assume the absolute correctness of the chance philosophic prejudices imbibed from a nurse or a schoolmaster or current modes of expression. It is to enact the part of those who thank Providence that they have been saved from the perplexities of religious inquiry by the happiness of birth in the true faith."[1] Now if both these authors mean the same by the word "philosophy" they clearly contradict one another. If Dr. Mitchell is right Prof. Whitehead is mistaken in looking for help among the spinners of vain words and barren ideas. If Prof. Whitehead is correct Dr. Mitchell has been misled by chance prejudices imbibed from some untrustworthy source. Another possibility is that these two authors do not mean the same by the ambiguous word "philosophy." The interests covered by the term are so numerous and varied that it is possible that some may be of importance for the re-formation of scientific ideas and others not. One of the most depressing features of the modern learned world is the absence of intercommunication between its branches. If there are aspects of philosophy which are of importance to science then it would seem desirable that science should keep in touch with them, and judicial separation is a state to be deplored rather than one for congratulation. Isolation leads to ignorance, and ignorance is the best culture-medium for misunderstanding, distrust, and intolerance—indeed, for every virus which separates men and wastes our attempts to think. Dr. Mitchell's view illustrates one very common misunderstanding in regard to philosophy, the belief, namely, that philosophy is trying to do the same job as natural science and doing it badly by an inferior method. Also the notion that

[1] *The Principle of Relativity.* Cambridge, 1922. P. 6.

philosophy is occupied with " vain words and barren ideas " betrays a misunderstanding of the different functions of science and philosophy and their historical relations.

Biological science has made us familiar with the notion of differentiation. We can legitimately extend it to political development and also to the evolution of thought. We might compare Greek thought with a fertilised ovum, with its " potencies," which has divided and differentiated to give us the highly complex texture of modern knowledge. But whereas in an organism differentiation and integration go hand in hand, this, unfortunately, is not the case with the development of thought, and here our analogy hopelessly breaks down. There is too much " self-differentiation " with no compensating " regulation." But it is just here that philosophy finds its niche. In one of its aspects it provides a critique of fundamental ideas which is invaluable as a corrective to incoordinated differentiation along one line. Modern physics is undergoing a process which we might liken to " dedifferentiation " in so far as it involves a process of unpicking of the closed system of concepts which it has developed during the last three centuries, and an unsparing critical scrutiny of all its fundamental notions. That system has been built upon the foundation of the common-sense ways of thinking of everyday life, a foundation which has been extraordinarily successful, but which is found to be inadequate in the spheres to which physics has now attained—so far are they removed from the conditions under which common-sense knowledge has been evolved. It is becoming recognised that common sense (in this meaning) is the greatest obstacle to clear thinking and it is here that philosophy comes into its own, because one of its tasks is to try to escape from the fetters placed upon thought by common sense and the egocentric predicament.

This brings us back to that mental work which is so rapidly fatiguing to gibbons and other primates. Everything is against it; the marvel is that it ever happens at all. " Nothing is more congenial," wrote William James, " from babyhood to the end of life, than to be able to assimilate the new to the old, to meet each threatening violator or burster of our well-known series of concepts, as it comes in, see through its unwontedness and ticket it off as an old friend in disguise." Not only are there these individual or subjective factors against fresh thinking, but also social ones—so-called " herd instincts," which keep thought in the wonted grooves. Is not the history of science filled with harrowing stories of the struggles of new ideas in the teeth of the opposition of tradition? Another obstacle to change is success. The success of the simple notions of traditional mechanics is astounding. They have

satisfied the requirements of physics for three centuries—no wonder they were regarded as keys to the very innermost secrets of nature—and so long as intellectual tools continue to receive fresh uses there is no stimulus to seek for new ones. Thus the four factors mentioned—natural laziness or simian reluctance, fondness of assimilating the new to the old, tradition and success—have contributed to keep our thought undeveloped. The periods of really intense intellectual ferment and tradition-shattering thinking have been extraordinarily few within the historical period. The thinking of Plato and Aristotle sufficed from Greek times to the Renaissance, and the thinking of Galileo and Descartes at the Renaissance has furnished natural science with a stock of fundamental notions that have needed little revision until recent times. Thus during most of the intervening times thinking has chiefly been a process of working out of the thoughts which issued from those two periods of successful overcoming of the limitations from which the primate intellect naturally suffers.

One consequence of this has been that people have taken it for granted that the ways of thinking thus evolved are the only *possible* ones—witness Kant with his rigidly fixed number of *a priori* categories. Two primary modes of thought emerged from the Renaissance: that employed in physical and that employed in mental science. Between these two stools biology has fallen—now looking for aid in one direction, and now in another—instead of attempting to plough a furrow for itself. There appears to be little doubt that this accounts, in part at least, for the celebrated antithesis between mechanism and vitalism. It hardly seems to occur to anybody[1] that there can be *any* other alternative, let alone several, and if any one ventures so much as to suggest such a thing he is invariably misunderstood and accused of harbouring secret prejudices in favour of one side or the other. Quite early in the last century it was discovered that, far from Euclid's geometry being the only possible one, it was possible for thought to rise above sensuous intuition and devise others. It then became a problem to determine, if possible, which was *the* geometry of physical space, provided you know what you mean by the latter expression—an embarrassing situation which had never presented itself before. The suggestion has been made that the same may be true of physics and that there may be more ways than one of interpreting physical facts if we had but the wit to discover them. Thus philosophy explores the realm of the possible, and natural science is a technique for determining which possibilities are actualised in the world of sense. The moral

[1] Dr. J. S. Haldane seems to be an honourable exception, but the present writer does not profess to understand his view nor to represent it here.

of all this should be fairly obvious for those who take a rather too naïve view of biological theory, and were it not for that "differentiation without integration" above referred to, these modern tendencies would be better understood. Consider, therefore, the process of differentiation in a little more detail. Thought begins with data and ends with interpretation. But data are bewilderingly and intolerably varied and complex and furnish endlessly diverse possibilities of interpretation. We are compelled to simplify, and common sense is the mode of simplification devised by the genius of primitive man. Science and philosophy are both differentiated out of this. If you put the emphasis on *sense* data and on analytical modes of interpretation, in purposely circumscribed realms of fact, you have science. If the emphasis is on broader and synthetic interpretations—especially on those which attempt to embrace both sense data and introspective data—you have philosophy. But there is another possible line for thought, namely, the study of the interpretative process itself, a study which was begun by Locke although not clearly differentiated from psychology until much later. It is this line which has given us the flourishing modern science of epistemology, and it is this to which Prof. Whitehead refers in the passage quoted above, when he says that you cannot neglect philosophy when engaged on the re-formation of ideas. It is probably speculative philosophy to which Dr. Mitchell refers in such uncomplimentary terms, because this is the only kind of philosophy which is at all popular and is therefore the kind commonly understood by the term. Moreover, the latter kind is very frequently confused with science in its own speculative moods. It is here that differentiation is so important, and has progressed much further in some sciences than in others. Biology is still embarrassed by the fact that biological thinkers have not yet clearly distinguished biological from related metaphysical problems. How is it possible to distinguish them? It is extremely difficult because, as M. Meyerson says: " L'homme fait de la métaphysique comme il respire, sans le vouloir et surtout sans s'en douter la plupart du temps." [1] But we can try.

In the first place the strong point of natural science has been its insistence upon verifiability. It is this demand which makes it possible for science to offer a body of doctrine upon which agreement is possible, so that it is a common possession resting upon brute fact. Here is a mark of difference, as well as an advantage, because metaphysical problems cannot be settled in this way. But if we confine scientific propositions to the verifiable we shall exclude from science all assertions about the remote past. Predictions are clearly in quite a different

[1] E. Meyerson: *De l'explication dans les sciences.* Paris, 1921. P. 6.

position because if they are wrong there is a possibility of their being found to be so, but this is not possible in regard to past events. Assertions about the past can obviously only be based upon what we know at present, that is to say, they represent the present as the consequence of hypothetical past events conceived on the basis of present events *known* to have such consequences. This, of course, involves the assumption that such events *did* happen in the past and this is a reasonable assumption if it explains the present state of affairs. But this cannot be verified because you cannot, without circularity, appeal to the data that the theory is attempting to explain to verify the theory as well. Facts are very accommodating, but logic will not allow you to do that with them. We argue that fossils are the remains of extinct animals, because, coupled with geological data, we know that the bones of animals existing to-day are capable of being preserved for long periods. This is a more reasonable hypothesis than supposing that they were embedded in the rocks for the purpose of pulling the legs of geologists, because at the present day such things are only done by school-boys at rare intervals, and there is no additional evidence that school-boys or equivalent malignant beings existed at the epochs in question. We give preference to the most probable hypothesis as judged from evidence available at the present day. Thus with suitable reservations it seems reasonable to admit such assertions to the rank of scientific hypotheses, more especially when there are positive empirical data to go upon. It is possible to make those reservations more precise from a consideration of the nature of inductive inference. Every observation or experiment which is employed in an inductive inference is made under certain conditions which can be divided into three groups: (1) the essential conditions, those which cannot be absent if the event in question is to occur; (2) the contingent conditions, which may be present or absent indifferently; and (3) those which have prevailed throughout the period during which human records have been kept and which are not under our control. Now it seems to follow from this that since we cannot tell whether instances of the third type of conditions are essential or contingent, no assertion about the past can be admitted as a scientific hypothesis if it involves the assumption of a departure, at a remote epoch, from conditions prevailing to-day, in order to make the hypothesis work. For the laws of nature as we know them to-day are valid only under the conditions prevailing to-day. No one knows what would happen if the laws of nature were different, and hence no scientific assertions can be made about hypothetical epochs in which they are supposed to have been different.

Dr. Chalmers Mitchell devotes his lecture to an account of

what he calls Huxley's " logical discipline " and to some of the consequences of the neglect of it by modern biologists. He summarises these principles as follows : " Accurate observation, clear statement, a logical scrutiny of generalisations so that they should not imply and should not seem to imply an iota of necessity ; an open mind for new facts, but a cross-examination of the evidence for them the more ruthless in proportion to their apparent contradiction of widely based generalisations ; and, above all, a profession of ignorance in preference to the propounding or acceptance of ' causal ' principles which could not be put to the test of experiment." If Huxley was more cautious than his contemporaries and so avoided making assertions which later investigations have contradicted it was because he was an epistemologist as well as a biologist and learnt his discipline from Locke and Hume. Dr. Mitchell gives four chief examples of " the evils in thought due to transgression of Huxley's canons " and to the extension of one generalisation or another " beyond the sphere from which it was derived." First he describes what he regards as the illegitimate inferences made by the late William Bateson in his celebrated address at Melbourne. This is condemned as the result of a desire to press cytological and genetical generalisations beyond their scope " to a kind of miraculous preformation as definite as that discarded by Wolff." We shall return to this later. The second example is furnished by the attempt to employ the concept of hormones in the service of a Lamarckian interpretation of evolution—an attempt which seems to be nothing more than pangenesis in modern dress. It involves assumptions which have not the remotest chance of verification at present, and is an example of the type of theory which has been far too common in biological speculation. The third example is " orthogenesis." Dr. Mitchell writes : " Osborn has spoken of orthogenesis as being due to an unknown law of hereditary predisposition, although he candidly admits that it may lead to extinction as well as to the elaboration of adaptive characters. It is a kind of biological Calvinism ; some stocks are vessels of wrath, predestined to damnation, others vessels of grace, predestined to such high achievements as, say, the alleged Nordic civilisation of the United States of North America. But it is not science." This seems to be a perfectly just criticism. An appeal to an " unknown law of hereditary predisposition " is perfectly useless for the purposes of science. It is a metaphysical not a scientific use of a concept. The last object of Dr. Mitchell's wrath is " emergent evolution," or at least the use made of it by Prof. Lloyd Morgan. Dr. Mitchell expresses the opinion that this concept is helpful " towards a materialistic interpretation of the phenomena of life in so far as it gives a

chemical and physical leg over difficult stiles." He accuses Prof. Lloyd Morgan of invoking it as "a justification of his view that the whole sweep of evolutionary advance may be attributed to Spiritual Agency." He even seems to suggest that Prof. Morgan is not sincere in his beliefs, when he speaks of "making due allowance for the troubles of a Gifford lecturer, anxious to keep in the open air of science and yet to temper its keen wind to the supposed theological opinions of my fellow-countrymen." Here Dr. Mitchell does not appear to have been so happy in his criticism. The Gifford Lectures were not founded for lecturers in natural science but, according to Lord Gifford's will, for "Promoting, Advancing, Teaching and Diffusing the study of Natural Theology in the widest sense of that term," and they were to be delivered "without reference to or reliance upon any supposed special exceptional or so-called miraculous revelation." The first three of Dr. Mitchell's examples were scientific doctrines and could therefore be criticised from the standpoint of science, but the fourth does not profess to be a scientific doctrine but a metaphysical one. Consequently, in discussing it, we have definitely crossed the border between natural science and metaphysics, and it is beside the point to apply the same canons of criticism as we should if it offered itself as a contribution to biological theory. Dr. Mitchell evidently has a strong predilection for what he calls materialistic interpretations, and a strong dislike for theology, and consequently it is not surprising that he should feel little sympathy for Gifford lectures and the opinions of Prof. Lloyd Morgan, whose inclinations lie in another direction. We are here in a region remote from natural science because its demand for verification cannot be fulfilled and no discussion in the scientific sense is possible.

But apart from this there seem to be directions in which Dr. Mitchell himself departs from the very principles he advocates, and falls into the same error which he wishes to expose in his first three examples. As a minor instance there is the statement on p. 27: "Apart from all questions of theory, evolution, descent with modification, has been established as the mode in which plants, animals, and human beings have appeared on this earth." This is presumably a slip because "apart from all questions of theory" is hardly compatible with Huxley's demand that there should be no "iota of necessity." You cannot have your cake and eat it. Finally, Dr. Mitchell departs from his logical principles as much as the authors he quotes in giving scientific assent to the doctrine of an original abiogenesis. He admits that this is a departure, but says: "The category of scientific faith must be reserved for speculations without a shadow of proof; it is an

honourable estate, and there is no merit in belief based on proof." Now it is difficult to harmonise this admission with Dr. Mitchell's strictures on Prof. Lloyd Morgan. Might not the latter also claim that *his* faith was an honourable estate? Dr. Mitchell gives no explanation of how it is that faith is only honourable when it coincides with his own particular outlook. There must always be an element of faith in all empirical generalisations because proof in regard to them never means demonstrative certainty. What, then, are the criteria of merit in faith? The pragmatist's test would be satisfied with a faith which could move mountains, but what can we *do* with Dr. Mitchell's belief? If it is urged that it furnishes an inspiration to biochemists to try to repeat the performance in their own laboratories, then it would have so much pragmatic justification, but it is difficult to take seriously the suggestion that biochemists are such feeble-minded people that they have no other spur or interest in their work than is furnished by this. To say that such beliefs are necessary for the prosecution of research is to imply that no biochemist is capable of synthesising any compound unless he believes that it has synthesised itself in the past, which is manifestly absurd.

Dr. Mitchell says that: "We are still far from having a causal explanation of the evolution of living matter from the inorganic." But he has insisted that Huxley's canons require "a profession of ignorance in preference to the propounding or acceptance of 'causal' principles which could not be put to the test of experiment." What he means by saying that we have not yet got a causal explanation of the evolution of living matter from the inorganic is, presumably, that no one has yet succeeded in bringing about such a state of affairs that it will happen to-day, and that consequently we have no grounds at present for saying that, since it happens to-day under such and such conditions, and since there is reason to believe that such conditions were realised in the past, it is therefore reasonable to suppose that such was the mode of origin of living things. This is all that one can mean by a causal explanation of such an event. But it is just the absence of this information which distinguishes this theory from the other theories about the remote past which have already been referred to, and it is the absence of this information which places this theory in just the same position as the theories of orthogenesis and the hormone theory of evolution which Dr. Mitchell criticises. We are no longer arguing from the present to the past, but are condemned to ask that the past shall have been sufficiently different from the present to allow us to assert of it what our present knowledge does not permit. But when once this licence is admitted anyone is free to suppose whatever he chooses, and

there is no way of deciding between one opinion and another. We have left the realm of science and entered one which Dr. Mitchell condemns. He has himself said that where biology has departed from the principles he enunciated at the beginning "it has been barren of additions to knowledge, fertile only in vain words." If this is true of the hormone theory of evolution it is equally true of abiogenesis ; and if abiogenesis is excused on the ground that it encourages research the hormone theory can claim precisely the same defence.

The first generalisation which this doctrine of abiogenesis contradicts is that which asserts that every living thing arises from some other living thing. If we try to project this back into the past we contradict the geological theories which require that the earth was once molten and at a high temperature. If we suppose that organisms existed at such temperatures we contradict another well-established biological generalisation. If we suppose organisms to have existed then which *could* survive high temperatures, then we can make no scientific assertions about them. We are driven then either to abiogenesis or to supposing that organisms came to our planet from elsewhere. For neither of these suppositions is there any support in our experience. But just as it is open to anyone to suppose that germs came to our planet from elsewhere and may still be doing so, it is also open to anyone to suppose that abiogenesis occurred once and is *still going on undetected.* The latter alternative was adopted by Prof. Shafer in his British Association Address in 1912. All that can be said of this supposition is : If you assume that abiogenesis has been going on throughout the period of the earth's history during which it has been capable of supporting living things, then at once you make mincemeat of all our phylogenetic theories, and this is a consequence which naturally will not trouble a physiologist. This assumption, then, is one which may encourage its supporters with the hope of a future verification, but preserves its plausibility only at the cost of forgetting one half of biological science—a thing which is very easy for a biochemist to do. The only other alternative left is to postulate a unique occurrence under conditions when the laws of nature were sufficiently different from what they are now to allow that to happen which we want to suppose *did* happen and this, as Dr. Mitchell says of Osborn's orthogenesis, is not science. It is basing your scientific beliefs on metaphysical ones. Science *discovers* biogenesis ; metaphysics *wants* abiogenesis, therefore it is assumed that abiogenesis must have happened.

What makes abiogenesis seem scientific is the fact, not that it is in harmony with what we know about organisms as such, but that human thought and skill have successfully combined

to synthesise organic compounds not otherwise occurring in nature apart from living things, although what is thereby synthesised does not admit of the predicate " living." The next step is smoothed over in thought by the incautious use of the concepts " protoplasm " or " living matter." These, from the point of view of the problem in question, are question-begging terms. They are names for genuine abstractions, otherwise they would not have survived. But, nevertheless, what we find in nature are not bits of protoplasm but individual living things. We can, if it is useful to do so, abstract from all the diversity of things in a cell, and from all the diverse kinds of cells, and think of a common stuff from which they are all made. But we know there is no such stuff. There is every reason to believe that in a given cell there is enormous complexity of different stuffs organised in a hierarchy of formed bodies. If the usual interpretation of the chromosome theory of heredity is true, even a single chromosome will contain an enormous number of different and highly specific entities. And yet in spite of all this it is commonly stated that the " protoplasm " of animals and plants is *identical!* The nearest approach we ever make to protoplasm is in the mixture obtained by grinding up cells with sand in a mortar, in which all organisation above the chemical level is destroyed. There does not appear to be any insuperable difficulty in the way of anyone some day concocting such a mixture, but it can hardly be expected to support the predicate " living " any more than the ground-up mass it imitates. In other words, whoever sets out to synthesise " life " (to use a popular expression) will have to give his mixture an organisation above the chemical level of integration. He will have to synthesise an organism, not protoplasm. Moreover, if he has to exercise thought to accomplish this, and if this is to explain an original abiogenesis, he will also, if he is to carry out the analogy fairly and consistently, have to postulate the intervention of thought in the original abiogenesis—a requirement with which Dr. Mitchell would probably decline to comply, since it could easily be pressed into the service of theology.

But all these difficulties pale into significance if we look at the arguments used by those who attempt to carry out the speculation in detail. We find that they all involve, if they are to work at all, putting into the original hypothetical chemical compound all the properties of living things as we know them now. The obvious difficulty is to get your organism to start at all. When once this is done, and the thing has got the known properties of organisms, then you can appeal to any of the current theories of evolution, according to taste, to do the rest. But in order to do this, you either have to endow chemical

compounds with all manner of mystical properties, which they are not known to possess, or you have to talk vaguely of such abstractions as protoplasm. All these difficulties appear to rest on the fact that such a process as evolution violates all our traditional ways of thinking, and calls loudly for a revision of our fundamental ideas. This can be most easily understood from the following considerations. Two ova from two different species develop in the same pond into quite different organisms. To " account for " this we telescope these differences back into the original starting point, *i.e.* into the undivided ovum, in the form of " potencies " conceived in one way or another. No difficulty arises about the origin of such things because we can always appeal to a previous generation from which they were derived. This is the recourse to which we are driven by the difficulties we experience in conceiving, with our present notions, how manifest complexity can arise from apparent simplicity. Now when we get back to such a beginning as is contemplated by the doctrine of abiogenesis this device is denied us, unless we are prepared to endow atoms and molecules with such potencies. This is the difficulty which Bateson presented in his Melbourne Address. Bateson was one of the most penetrating and critical thinkers biology has so far possessed, and a perusal of his books shows him always able to grasp the essentials of biological problems, to state them clearly, and leave them so rather than to obscure difficulties by camouflaging them in speculations which flatter what Dr. Mitchell calls our " anthropomorphic vanity," but which make " no positive addition to knowledge." We can therefore either accept Bateson's preformation and the premises on which it rests, or we can regard the Batesonian preformation as a *reductio ad absurdum* of those premises, but we cannot keep the premises and reject the preformation. In just the same way we can regard Hume's scepticism as a refutation of the atomistic psychological assumptions upon which it all hangs, or we can accept those assumptions and with them the scepticism. But we cannot keep the assumptions and escape the scepticism.

The above arguments are not offered as a refutation of abiogenesis. That theory may very well be true. But the considerations here brought forward, and many others not mentioned, do suggest that the implications of the doctrine have not been properly appreciated by biologists, and that a re-formation of our fundamental ideas is as much needed in biological as in physical science. Indeed, a mathematical physicist has already pointed this out to our shame, and has suggested directions in which we may look for light if we are prepared to put our biological pride in our pockets. Prof.

Whitehead, speaking of the rise of evolutionary notions during the last century, writes :

" By a blindness which is almost judicial as being a penalty affixed to hasty, superficial thinking, many religious thinkers opposed the new doctrine ; although, in truth, a thoroughgoing evolutionary philosophy is inconsistent with materialism. The aboriginal stuff, or material from which a materialistic philosophy starts, is incapable of evolution. This material is in itself the ultimate substance. Evolution, on the materialistic theory, is reduced to the rôle of being another word for the description of the changes of the external relations between portions of matter. There is nothing to evolve, because one set of external relations is as good as any other set of external relations. There can merely be change, purposeless and unprogressive. But the whole point of the modern doctrine is the evolution of the complex organisms from antecedent states of less complex organisms. The doctrine thus cries aloud for a conception of organism as fundamental for nature."[1]

Thus the scientific thinkers who accepted evolution understood its implications as little as the religious thinkers who opposed it. They have persisted in trying to pour the new wine into the old bottles. Far from modern biology having any " conception of organism " its aim seems to be to try to get as far away from it as possible and to substitute such abstractions as protoplasm or " living matter." Thought *can* only work with abstractions, but the question is : have we yet discovered what are the *right kind* of abstractions for biological thinking ? Speaking of the history of the natural sciences, Dr. Broad has said :

" They flounder about in the dark till some man of genius sees what are the really fundamental factors and the really fundamental structures of the region of phenomena under investigation. In mechanics the keystone is the notion of acceleration ; in chemistry it is the theory of elements and compounds and the conservation of mass ; in economics, perhaps, it is the notion of marginal utility. Sciences where no such discovery has yet been made, such, *e.g.* as psychology and biology, are almost at a prescientific level ; their inductions carry no great conviction to anyone trained in the more advanced sciences."[2]

Is anything in the nature of a renaissance in biological thinking possible ? It would seem at first sight that Mr. J.

[1] *Science and the Modern World*. Cambridge, 1927. P. 134.
[2] C. D. Broad : "The Relation between Induction and Probability," *Mind*, vol. xxix, p. 45.

Needham, in a recent article,[1] was arguing that any change in scientific biology was impossible. But Mr. Needham appears to use the term " science " in a very restricted, and " philosophy " in a very wide, sense. By science he seems to mean simply the activity of investigation itself, and by philosophy the theoretical interpretations reached by thought reflection on the facts which are the outcome of investigation. But Mr. Needham is one of those thinkers who believe in thought-tight divisions, and he wishes to interpret the speculations of Prof. Lloyd Morgan and of Prof. Whitehead, in his later books,[2] in favour of an unchanging biological methodology but with a biological philosophy which would otherwise flatly contradict it. The thinking of the investigator, he seems to suggest, is eternally condemned to run in one unalterable groove. Only to the philosopher is it granted to dwell in another sphere and understand something of their subtle connection. In his former writings Mr. Needham was content to exclude the organism from philosophy and to endure the absence of all connection between the latter and science. What he says in his recent article about the investigator is extremely good and profoundly true, but he has made the situation appear more simple than it is. He passes too lightly from the proposition that the traditional methodology is successfully applicable to every sphere of natural science (which no one in his senses would deny) to the much more debatable suggestion that it is the *only possible* one. Many people have been puzzled by the apparent contradiction between Dr. J. S. Haldane's " methodological vitalism," as it has been called, and his actual practice. But they do not appear to have noticed that he has been guided by his theoretical views to approach the study of respiration in a way which led to discoveries which his predecessors had missed. This is clear from a passage at the close of chapter i, p. 14, of his *Respiration.* Instances could be given from other investigators who have been guided in a similar way although they have not so frankly faced the issue as Dr. Haldane has done. It cannot, therefore, be claimed that what we may shortly call an " organic " approach to biological problems, as opposed to a *purely* analytical one, is entirely without heuristic merit. Mr. Needham seems to suggest that the relation between science and philosophy is a purely " asymmetrical " one. Philosophy, in his sense, is to work with the data obtained by science, but the latter is to remain uninfluenced by what philosophy may do with them. It has certainly not been so in the past because the only philosophy which science

[1] " Organicism in Biology," *Journ. Philos. Studies*, vol. iii, p. 37.

[2] It should be noted that Profs. Whitehead and Lloyd Morgan diverge on many fundamental points, especially on theory of knowledge and the relation of mind to nature.

has taken any heed of has been its own, namely, the traditional materialism, and this has naturally kept its methodology in one groove. But if that philosophy is now beginning to crack up, even in the eyes of physicists, it is not for speculative reasons but because it is beginning to fail methodologically. That this is so seems clear from the following passage in a book by Prof. P. W. Bridgman, which is written, be it noted, from a purely empirical standpoint. As a lesson in disinterested thinking and in the method of overcoming " chance philosophical prejudices " it is a book which every biologist would do well to read.

" It is difficult to conceive anything more scientifically bigoted than to postulate that all possible experience conforms to the same type as that with which we are already familiar, and therefore to demand that explanation use only elements familiar in every-day experience. Such an attitude bespeaks an unimaginativeness, a mental obtuseness and obstinacy, which might be expected to have exhausted their pragmatic justification at a lower plane of mental activity.

" Although it will probably be fairly easy to give intellectual assent to the strictures of the last paragraph, I believe many will discover in themselves a longing for mechanical explanation which has all the tenacity of original sin. The discovery of such a desire need not occasion any particular alarm, because it is easy to see how the demand for this sort of explanation has had its origin in the enormous preponderance of the mechanical in our physical experience. But nevertheless, just as the old monks struggled to subdue the flesh, so must the physicist struggle to subdue this sometimes nearly irresistible, but perfectly unjustifiable, desire."[1]

What superb intellectual detachment! What an inspiration for every biologist who claims to have any vestige of mental elasticity!! But if Mr. Needham is correct Prof. Bridgman is doing no less than bidding every investigator in natural science to murder the goose that lays the golden eggs. But Mr. Needham appears to be bidding science to adopt a creed, and that, according to Huxley, is to commit suicide. Dean Inge also asserts that science *must* be monistic. But we look in vain for any justification for the *must* in these authors' pages. Surely science, in the wide sense, is at liberty to be just as pluralistic as it suits its convenience to be. If Mr. Needham would approach the problem more from the epistemological and empirical standpoint, as Prof. Bridgman does, he would, perhaps, see it in a different light. The best attitude for science seems to be not to tie itself up with any one constructive or metaphysical scheme. The supposed necessity for monism in

[1] *The Logic of Modern Physics.* New York, 1927. P. 46.

science may rest on nothing more than a psychological kink, like our fondness for dyadic relations. And if by taking thought we can remove such limitations so much the better. We have noted the contradictions into which Dr. Mitchell has been led by his materialistic monism—denying, as he does, to others, speculations of the same logical type as those in which he indulges himself. Huxley's discipline is easy to prescribe but difficult to follow. What, then, are we to do in the midst of all these conflicts and inconsistencies? This brings us back to our starting point—to our simian ancestry, our preference for fighting rather than thinking, for competition rather than co-operation, for dogmatism rather than tolerance. All these characteristics appear to rest on our persistent adherence to absolutism and mutually exclusive interpretations. Far from welcoming many alternative interpretations, we want this or that one to be *the* interpretation. Dr. Mitchell is not satisfied that materialism has furnished us with a fruitful methodology; he wants to believe that the whole world consists of, and can be exhaustively interpreted as, a collection of little bits of stuff pushing each other about—a notion derived, as Prof. Bridgman says, from everyday experience. It is one to be used intelligently but not to be adhered to with all the tenacity of original sin. Every such notion has its "field," but there are many fields, even in what is called nature, and to suppose that we already possess one mode of thinking which covers them all, and to scorn all contrary opinions, seems to be the height of "anthropomorphic vanity." When we consider how brief has been the history of human thought, and how much briefer still the life of natural science; when we remember that the latter is only one part of the whole field of intellectual inquiry; when, further, we reflect that our experience has been limited to only one part of the vast scale of magnitudes, and only one part of the universe during a brief period of its existence; when we recall, too, that during that period our thought has been dominated by but few fundamental notions derived, for the most part, from the still more limited field of daily life; then, it seems to be the height of folly to suppose that any one of our constructive schemes, however comprehensive and carefully elaborated, or however well rooted in our experience, can be even approximately true and exhaustive of what we know, still less that it entitles us to suppose that there are no further surprises in store for us.

The most hopeful suggestion seems to be, then, that there are many "fields," and that whilst we are indebted to a few men of genius for finding the right concepts for some of them, it would be better in biology not to persist in stretching our old ideas beyond their original fields until they become so thin as

barely to conceal the ignorance they cover. It is rarely a question : can a given concept be used in a particular field, but, are we justified in asserting it to be the *only* one that can be profitably used? Thus if we change our attitude towards scientific concepts, we see that there is need, not for competition among them, but co-operation, room for mutual supplementation but not for mutual exclusion. We shall be free then to demand " that *reason* should be used," as Whitehead says, and that the " ape and tiger " be allowed to die.

FUNDAMENTALS. By Allan Ferguson, M.A., D.Sc. Being a review of : (1) **Science and Philosophy,** by the late Bernard Bosanquet. (London : Allen & Unwin, 1927. Price 16*s*. net.) (2) **The Logic of Modern Physics,** by P. W. Bridgman. (New York : The Macmillan Company, 1927. Price 10*s*. 6*d*. net.) (3) **The Anatomy of Science,** by G. N. Lewis. (Oxford University Press, 1926. Price 14*s*. net.)

We live in a queer, but thoroughly interesting world nowadays. Facts, for the most part dealing with, or to be interpreted in terms of, concepts whose scale of magnitude is far below that to which the notions of Newtonian mechanics apply, are accumulating at such a rate that the most unmetaphysical amongst us is driven to theorise. Very far distant seems the time when Bunsen could assert that " one chemical fact properly established was worth more than all the theories one could invent," or when Röntgen, on his way to an after-lunch nap in his private room, could remark genially as he passed through his laboratory, " Ich gehe theoretisch arbeiten." It is impossible to handle the vast mass of facts which pours from our laboratories at an ever-increasing pace without some form of theory which shall serve to correlate them, and as the range of the facts widens, it becomes increasingly difficult to fit them into one consistent scheme. Naïve notions of space and time which have served us well enough in the past have to be thrown into the melting-pot, and the revolutionary statement that " von Stund an sollen Raum für sich und Zeit für sich völlig zu Schatten herabsinken, und nur noch eine Art Union der beiden soll Selbständigkeit bewahren," has now become all but a platitude.

It is curious to notice how consistently the nineteenth-century man of science confused the perceptual and the conceptual world, giving to the conceptual world a reality corresponding to the world of his perceptions, and so introducing into a descriptive scheme such as that of the Newtonian mechanics an element of necessity which led to a confused meaning of the term " law," and to a feeling that the Newtonian scheme corresponded to an external reality in such a way that, minor modifications apart, all newly dis-

covered facts would prove to be part and parcel of that scheme.

Truth to tell, in spite of the fact that discontinuities such as the Victorian physicist would regard with sour distrust are now an integral part of the scheme of things, there still exists a feeling that the Newtonian scheme *does* correspond to something carrying with it a notion of necessity which is absent from, say, the Ptolemaic or Keplerian systems of the heavens; and this notion is fostered by a definition of force as a *cause* of change of motion. Yet, what are the facts?

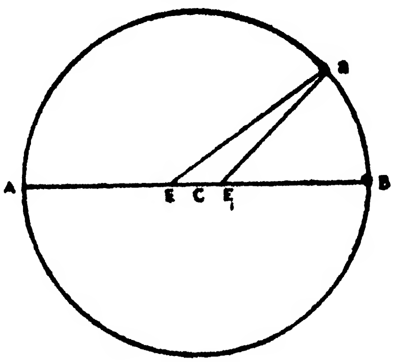
We live in a world of changing sense-impressions, which impressions we must correlate as best we may, if we are to pick our way through our lives with any comfort to ourselves or others. And in place of this perceptual world, which, without stopping to discuss the meaning of the term "real," we shall call the world of reality, we introduce a conceptual world the changes in which, subsumed under as simple a formula as possible, shall imitate as closely as may be the changing impressions of the world of perception.

It is but natural that the group of sense-impressions which are included in the term "astronomical" should be among those for which men earliest endeavoured to find a formula, nor is it surprising that, built on the scale on which we are, the mechanical notions derived from a study of astronomical and molar magnitudes should be given a reality which they are far from possessing and should be projected and extrapolated down to atomic and sub-atomic regions. The surprising thing is, not that these notions should fail us in this region, but that they should have enjoyed the remarkable success which has fallen to their lot. *A priori*, there seems to be no reason to suppose that they should have any validity at all in these realms so far removed from the region of everyday dimensions, where an hour is a reasonable unit of time, and a yard a convenient unit of length.

It is not unprofitable to study in some detail the various conceptual schemes which have from time to time been proposed to represent the facts of astronomy, and specially to note their relation to the comprehensive scheme propounded by Newton.

Hipparchus, then, reduced his astronomical sense-impressions to order by constructing a conceptual world in which the planets move in circles around an eccentrically placed earth. Ptolemy employed the idea—known indeed to Apollonius and to Hipparchus—of the epicycle, where the *conceptual* planet moves in a circle, the epicycle, the centre of the epicycle moving in another circle, the deferent. At (or near) the centre of the deferent the earth is fixed. Ptolemy placed the earth (E)

eccentrically at a distance CE from the centre of the deferent, and introduced another point E_1, the equant, determined by the fact that ECE_1 is a straight line, and EC is equal to E_1C. The motion of the centre of the epicycle round the deferent was then regulated so that, as seen from the equant, the apparent motion of the centre of the epicycle should be uniform. With this conceptual system he was able to describe the motions of the planets in such a way that the description agreed very closely with the facts of perception. Indeed, it is worth noting that the simple eccentric system of Hipparchus can represent the perceptual facts, as far as the motion of the



sun is concerned with such accuracy that the error will never exceed one minute of arc.

Using the heliocentric doctrine enunciated by Copernicus, but rejecting his apparatus of epicycles, Kepler described the motion of the planet Mars round the sun in the simple laws which bear his name.

(1) *The planet describes an ellipse, of which the sun is in one focus.*

(2) *The radius vector*—the line joining sun and planet—*sweeps out equal areas in equal times.* To these, considering the relations between the motions of the different planets, he added his third law, that

(3) *The squares of the periodic times of any two planets are proportional to the cubes of their distances from the sun.*

Now all these systems are admittedly descriptive, but the Newtonian system is supposed to go a step further, and to give a reason—a cause—for Kepler's laws. A detailed analysis shows that this conclusion is unwarranted, and that the Newtonian scheme differs from that of Ptolemy or of Copernicus, where we—

> " Gird the Sphere
> With Centric and Eccentric scribbled o'er,
> Cycle and Epicycle, Orb in Orb,"

only in that it subsumes a wider range of phenomena under a simple formula. For in what consists the process? If we imagine a conceptual world of but two particles, we assume that, whatever the velocities of these particles may be, in whatever complex manner their motions may change, the ratio of their mutual accelerations remains constant and independent of their relative positions and velocities. To this ratio we give a name. We call it the mass-ratio of the particles, defining this mass-ratio in such a manner that—

$$\frac{M_Q}{M_P} = \frac{\text{Mass of Q}}{\text{Mass of P}} = \frac{\text{Acceleration of P due to Q}}{\text{Acceleration of Q due to P}} = \frac{f_P}{f_Q}.$$

Writing this in the form

$$M_Q f_Q = M_P f_P,$$

We *name* and *define* the product $M_Q f_Q$, as the force exerted *by* P *on* Q.

If, then, we build up a conceptual universe in which the mutual interactions of the particles may be described in terms of forces defined as we have just defined them, we find that we may resume a vast range of phenomena, celestial and terrestrial, in the shorthand statement that every particle in our conceptual universe attracts every other particle with a force proportional to the mass of each and inversely as the square of the distance between them.

But we must emphasise again and yet again the importance of the *descriptive* nature of this statement. It is a statement of higher generality than the statement made by Kepler, including Kepler's laws as one aspect of a wider solution, but it no more proposes a final " why " than does Kepler's statement. If we care to answer the question, " Why do the planets move in ellipses? " and so forth, by stating that the law of gravitation provides the reason, no great harm is done so long as we recognise that such a statement is mere conventional shorthand. But considerable harm may be, and has been done by permitting such questions to import into the descriptions of our conceptual world an element of necessity which is non-existent, and which inevitably leads to a confusion between law in the scientific sense, and law in the legal sense of the word. Moreover, such explanations, in terms of a " Because " answering to a " Why," can never be final, or even approach finality. If we answer the " Why " of Kepler's Laws with a " Because " in terms of the law of gravitation, we are immediately faced with the question of the reason for the existence of the inverse square law. Newton himself was fully alive to the difficulty, and his much-quoted and much-

misunderstood phrase " non fingo hypotheses " is, when read with its context, part of a perfectly clear statement of the fact that he is unable to give any reasons for the existence of the law of gravitation. In the penultimate paragraph of the *Principia* he states :

" Rationem vero harum gravitatis proprietatum ex phænomenis nondum potui deducere, et hypotheses non fingo. Quicquid enim ex phænomenis non deducitur, *hypothesis* vocanda est ; et hypotheses seu metaphysicæ, seu physicæ, seu qualitatum occultarum, seu mechanicæ, in *philosophia experimentali* locum non habent."

It is, indeed, a remarkable fact that this conceptual system, called into being to correlate man's sense-perceptions of data of molar magnitudes, should bear an extrapolation to atomic dimensions, and that notions of smoothness, roughness, perfect elasticity and the like, applied to conceptual atoms, whose motions are discussed in terms of Newtonian mechanics, should lead to results which are in good accordance with quantitative data. True, we find that, as Sir J. H. Jeans has remarked, " The old laws are not, so to speak, fine-grained enough to supply the whole truth with regard to small-scale phenomena," but the matter for surprise is, not that the Newtonian description should break down at some points in this realm of small-scale phenomena, but that it should have been found to have any validity at all therein. Kirchhoff, who certainly did not anticipate that new discoveries were likely to upset fundamental conceptions, characteristically remarked, when told by Sir Arthur Schuster of the effect of light on selenium, " I am surprised that so curious a phenomenon should have remained undiscovered till now." We may equally feel surprise that quantum ideas, which are now leading principles in dealing with mechanisms of atomic magnitude, should so long have remained unsought, and, indeed, down to a certain stage unnecessary. Prof. Bridgman has some pertinent remarks to make, *à propos* of the Lorentz field equations, concerning the risks of extrapolation processes.

" . . . If one contrasts the scale of the supposed dimensions of the electron with the smallest dimensions on which we can make independent experimental verification of these equations, he must admit that there is an enormous chance for change in the type of equation beyond the limit that we can reach by direct experiment, and the chances of guessing the correct extension of the equation to small dimensions are correspondingly almost vanishingly small. . . . In spite, however, of the apparently enormous chances against it, this programme of extending the field equations to small dimensions and following out the consequences was exactly the programme which Lorentz

set himself. . . . It seems that, regarded as a complete description of physical behaviour on a small scale, the equations must be judged false, because they contain no suggestion of quantum phenomena.

"Even if we have to recognise that the equations are false, there can be no question that they correspond to an important part of reality, and that they have been of the greatest service to physics. What is the significance of the success that they have attained? It is to be noticed that all the phenomena to which the Lorentz equations have been successfully applied, although not large-scale phenomena in the ordinary sense of the word, are nevertheless phenomena involving the co-operation of a number of atoms and that the equations unquestionably fail when applied to phenomena involving single electrons. It appears from our best present evidence that on a small scale the behaviour of nature is governed by quantum principles, and is therefore quite different from large-scale behaviour, which . . . is governed by the Maxwell equations. There must, of course, be a transition zone in which the character of phenomena changes from quantum to Maxwell. Now any programme like that of Lorentz is almost inevitably bound to begin to give correct results when we get up as far as the transition zone, for the simple reason that the relations of Maxwell have been put into the equations and are always there ready to appear as soon as the quantum relations begin to give way. The physical significance of the success of the Lorentz programme seems to be that the transition from Maxwell to quantum phenomena constitutes a large part of the programme of the immediate future.

"All this scepticism about the classical work of Lorentz is likely to be rather irritating or depressing, particularly if one attempts to imagine what other course could have been adopted. Indeed, it does seem that we find ourselves in a real quandary; Lorentz was practically forced, because of the character of the mathematical tools at his command, to take the course that he did, in spite of any recognition of the physical meaninglessness of the mathematical operations. . . . Conventional mathematics does not correspond to the physical reality, . . . it recognises no difference between the physically big and the physically little and the corresponding change in the operational meaning of the symbols. It begins by being a most useful servant when dealing with phenomena of the ordinary scale of magnitude, but ends by dragging us by the scruff of the neck willy-nilly into the inside of the electron, where it forces us to repeat meaningless gibberish.

"What we should like to be able to do is easy to see. The

things that go into our equations must have independent physical meaning, and the character of our mathematical formulation should change to keep pace with the change in the physical operations which give meaning to the terms. For example, electrical density has a meaning for large-scale phenomena, but means nothing on a small scale. Our ultimate electric unit is the electron ; when we get down to this scale of magnitude, our mathematics ought to be making statements about the relative behaviour of discrete electrons, and not mention so much as by implication the density at points inside an electron. But this sort of thing we apparently cannot yet do ; the proper mathematical language has not been developed."

Without taking every phrase in this quotation literally—it is obvious that the statement that mathematics recognises no difference between the physically big and the physically little requires a certain amount of qualification—it is so suggestive in its outlook that we make no apology for quoting these pages practically *in toto*.

The necessity for keeping in mind the dangers attendant on a confusion of the perceptual and conceptual worlds is emphasised by the recent discussions which have arisen concerning the reality of atoms. *Brownian Movements and Molecular Reality*, the confession of faith of Prof. Ostwald, and, unless we do him an injustice, the statement by Prof. Eddington in his delightful book on *Stars and Atoms*, that the mark of a ray-track on a photographic plate affords the same evidence for the perceptual existence of individual atoms as does a thumb-print for the perceptual existence of a thumb ; all these go to show that the problem of the real existence of particles of atomic dimensions is one which will at least repay analysis.

We do not propose to enter on the thorny path of a strict discussion of the meaning of a " real " existence. It is not necessary. Nor need we consider the possibility of the existence of the " Dinge an sich." It is sufficient to say that in the perceptual experiences of normal human beings we have impressions of what we call " external objects." These impressions are a complex mixture of immediate and of past stored-up sense-impressions, and the sum-total of these impressions in any particular instance—which *is* the external object—has received from Prof. Lloyd Morgan the name of " construct." The desk at which I now write is such a construct, my impression of it depending, not alone on immediate impressions, which change with every change in the angle of view or in the conditions of light or shade, but on a synthesis of these with certain past sense-impressions. The question, then, before us is, is

the atom real in the sense—whatever it may be—in which a pen, or a table, is real? There is a tendency to assume that quantitative work on Brownian movements, or on ray-tracks, affords a demonstration of atomic reality which heretofore was missing, and it must be emphasised that the atom is just as much, or as little, of a perceptual existence after all this work as it was before it.

The whole apparatus of molecules, atoms, and electrons constitutes a concept called into being to correlate certain perceptual facts—the facts of quantitative chemical combination, the decay of torsional oscillations of an horizontal disk suspended in a gas, the movements of the mercury in a constant pressure air thermometer, the movements of a fluorescent spot in a vacuum-tube, and the like. With all these phenomena before him, Ostwald was content to adopt a sceptical attitude to the atomic theory. How is the evidence for the *perceptual* existence of an atom strengthened by the knowledge that, if we look down the tube of a microscope, we shall see a number of dancing particles whose movements can be predicted in terms of kinetic theory? The perceptual facts are the maze of dancing particles, a line drawn across a photographic plate, or, as in Millikan's apparatus, an oil-drop observed through a short-focus telescope with a scale in the eye-piece. We have, in fact, under our concept of atoms and electrons, subsumed a certain additional number of important experimental facts, and thereby further emphasised the convenience of the conception.

We are far from asserting that a concept may never become a perceptual reality. Ekaluminium and ekasilicon were concepts described by Mendeléef, and later came into the perceptual scheme of things as the elements gallium and germanium respectively. The planet Neptune affords another instance which has become classical. But the atom stands on a different footing, and, *on the scale on which we have constructed our conceptual world*, the atom is, and, in virtue of the properties assigned to light-waves, always will be, far beyond the sphere of our sense of perception.

We have discussed in such detail these important fundamental notions that we have left but little space in which to develop or barely to recount other important topics dealt with in the works under review.

The late Dr. Bosanquet's book is a posthumous collection of his occasional writings garnered from various sources, and written between the years 1886 and 1919. They are collected under three main headings, (*a*) Logic and Metaphysics, (*b*) Ethical, Social and Political, (*c*) Æsthetics. Of Dr. Bosanquet it can with literal truth be said that he touched nothing which

he did not adorn, and whether he is discoursing on Science and Philosophy, Contradiction and Reality, Ladies and Gentlemen, the Idea of Decadence, or the Nature of Æsthetic Emotion—to quote a few of the titles of the twenty-four essays which make up the book—he is always stimulating and thought-compelling. True, he belonged to the class of professed metaphysicians, that

"Gens ratione ferox, mentem pasta chimæris,"

and the impatient man of science may find some of his more philosophical discussions tending perilously near to logomachies, but even there the subtleties of his argument compel close attention, while in his ethical essays the charm, alike of diction and of thought, is complete.

Here and there, swimming rare in the vast whirlpool, one may find statements to which one takes a certain amount of exception. The essay on the idea of decadence, read before the students of the University College of North Wales more than twenty years ago, provides a case in point. Dr. Bosanquet develops the thesis that the decadence of the post-classical age not merely permitted, but actually produced, in its relaxation of spiritual tension, the revival of the epigram, and he quotes in illustration lines whose haunting charm is often in the ears of one who has read them understandingly:

"They told me, Heracleitus, they told me you were dead,
They brought me bitter news to hear, and bitter tears to shed;
I wept, as I remembered how often you and I
Had tired the sun with talking, and sent him down the sky.

"And now that thou art lying, my dear old Carian guest,
A handful of grey ashes, long, long ago at rest,
Still are thy pleasant voices, thy nightingales, awake,
For Death, he taketh all away, but them he cannot take."

Johnson Cory's translation is a perfect poem, and, in the admirable way in which, full of sentiment, it avoids all trace of sentimentality, the translation illustrates equally the "relaxation of spiritual tension," and the debt which we owe to one aspect at least of the art of a decadent age. But is it Callimachus? We doubt it, and the translation given by the Vice-Provost of Eton in a recent number of *The National Review* exhibits an austerity of phrase which is not that of a decadent age. We hasten to add that we are far from attempting to combat the main thesis of Dr. Bosanquet's essay—we would merely hint that this particular illustration does not carry conviction.

The world is the poorer by Dr. Bosanquet's death, and we owe a debt of gratitude to the pious care which has gathered together these fragments of a richly endowed mind.

Professor Bridgman's essay does not consider in any great detail the epistemological problems beloved of Dr. Bosanquet. In fact, he frankly adopts a naïve attitude to metaphysical problems. "We shall accept," he says, "as significant our common-sense judgment that there is a world external to us, and shall limit as far as possible our inquiry to the behaviour and interpretation of this external world." Within these limits he discusses learnedly and acutely of the relative character of knowledge, of explanations and mechanisms, of models and constructs, of the concepts of space, time, causality, identity, velocity, force and mass, energy, thermodynamics, relativity, and of electrical and quantum concepts.

He has introduced us to a novel and interesting test of the significance, or otherwise, of the many fundamental questions which are now vexing the mind of the natural philosopher. There may, or may not, exist in nature bodies with properties expressed by certain concepts. Whether these concepts have anything corresponding to themselves in nature is a matter for experiment to discover. For example, "the concept of length is . . . fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined . . . the concept is synonymous with the corresponding set of operations." This "operational" attitude towards a concept leads to an interesting definition of the significance of a question. If operations can be found by means of which an answer may be given to the question, the question has significance. Otherwise, it is meaningless. "This matter of meaningless questions," continues Prof. Bridgman, "is a very subtle thing which may poison much more of our thought than that dealing with purely physical phenomena. I believe that many of the questions asked about social and philosophical subjects will be found to be meaningless when examined from the point of view of operations. . . . We shall no longer permit ourselves to use as tools in our thinking concepts of which we cannot give an adequate account in terms of operations. In some respects thinking becomes simpler, because certain old generalisations and idealisations become incapable of use. . . . In other respects, however, thinking becomes much more difficult, because the operational implications of a concept are often very involved." Prof. Bridgman has doubts concerning the social virtues of those who indulge in operational thinking, looking forward to a measure of unpopularity for the earnest seeker who holds up an argument by demanding the meaning of apparently the simplest terms. This unpopularity is not novel; Socrates experienced it, and we foresee, in operational circles at least,

a revival of the defensive move initiated by Poinsot. " Are we agreed," said he to a disputatious friend, " on the meaning of the word time ? Very well , let us talk about it. Are we not agreed ? Very well , let us talk of something else." But Prof. Bridgman points out a possibility which offers encouragement to the operational thinker inasmuch as, with greater skill in operational methods of thought, much of our present conversation may become unnecessary. A very desirable end.

Prof. Bridgman's work is thoroughly stimulating and suggestive, and he is never more suggestive than when he is dealing with the extrapolation of large-scale ideas to small-scale phenomena. One further quotation will serve to summarise his attitude : " The invention of new concepts is certainly not an easy thing, and is something which physics has always deliberately and perhaps justifiably shirked, as shown by the persistent attempts to carry the notions of mechanics down into the finest structure of things. This shirking has not had bad results, but on the contrary good results, as long as physics has been primarily concerned with phenomena near the range of ordinary experience, but I believe that, as we get farther and farther away from ordinary experience, the invention of new concepts will become an increasing necessity."

One may be tempted to ask what Prof. Bridgman means by ordinary experience ? Small-scale *experience* is denied to us—we may interpret certain ordinary scale phenomena in terms of small-scale concepts, but the limits of our existence are such that our experience, as far as *scale* goes, is, of necessity, ordinary. Possibly Prof. Bridgman is referring to operations carried out at temperatures and pressures outside the common range. However that may be, it remains true that the widening of our experience is daily forcing on us the necessity for the invention of new concepts.

Prof. Lewis's book consists of a series of eight addresses delivered at Yale College as one course of the Silliman Memorial Lectures. The author discourses easily and genially, with a fine sense of scholarship and a wealth of apt illustration which at times imparts a delusive sense of ease to a difficult problem, of Numbers, Space and Geometry, Time and Motion, Matter in Motion, Light and the Quantum, Probability and Entropy, the Non-mathematical Sciences, and Body and Mind. Each lecture is in itself a complete unit, though through the different essays runs a connecting thread of thought, which outlines, in the author's own words, a " satisfying little philosophy, which I venture to believe is shared by a number of men of science, although it differs as widely from the materialism traditionally

attributed to scientists as it does from the thinly disguised theology of classical metaphysics." Where all is interesting, it were invidious to select; but if we were to choose one essay as more fascinating than another it would be that on Probability and Entropy, where the author conducts us by easy stages from the miracle of the one-gram weight rising " spontaneously " through a height of ten centimetres once in a number of centuries to reckon which a million is all too small a unit, to the daily miracle of the dance of the gamboge particles in Brownian movement. And if, as captious critics must, we were to hunt for a phrase to which we should take exception, we should find it in the last essay in the book, where the author, adopting the Johnsonian solution of the eternal problem of free will, says, " It is a paradox which comes almost daily to our attention and never more than at the present time, when the doctrine of determinism is very widely taught, and yet we have an uncommonly keen sense of individual responsibility, which is one of the main ideas underlying our whole social structure."

But surely a sense—in ourselves or concerning others—of individual responsibility is inherent in every system of determinism, and is, *ipso facto*, ruled out from a free-will system. No determinist for a moment assumes that in our volitions we are not free to choose—the fundamental question is, " What determines our choice ? " Free will, if it means anything, means that our volitions are uncaused, and in the moment in which we take up such an attitude all ideas of moral responsibility go by the board.

What will be the end of it all ? Criticism of certain concepts of the Newtonian scheme has led to the theory of Relatitivity; quantum notions have been introduced by the failure of our small-scale concepts to represent accurately certain aspects of our perceptual universe; and at the moment we find it convenient to explain certain sense-impressions in terms of mutually contradictory conceptual schemes. Are we to be content with mechanical models—one model devised to illustrate one set of phenomena, a second to illustrate another set, and possibly a third, or even a fourth ? Or are we better advised to scrap all models, and to consider a phenomenon explained when we have described its progress by means of a differential equation ? One fascinating possibility begins to emerge from the welter of systems—almost daily appearing and dissolving—which are employed to describe the sequence of our perceptions, that the whole apparatus of atoms, electrons, and ether may disappear and be replaced by a system which shall describe the full routine of our perceptions in terms of the geometry of space.

REVIEWS

MATHEMATICS

Numerische Infinitesimalrechnung. By Dr. Martin Lindow. [Pp. viii + 176, with 17 figures.] (Berlin and Bonn: Ferd. Dümmlers Verlagsbuchhandlung, 1928. Price M.15 stitched, M.18 bound.)

In the applications of mathematics to data obtained from experiment or observation the functions determined by these data are only known by their numerical values for a discrete set of values of the argument. It is frequently necessary to differentiate or integrate the functions so defined. Even should analytical expressions be available, to obtain numerical values is often a matter which sorely taxes the powers of the computer. An analogous problem is the determination of numerical values of functions defined by ordinary differential equations.

The object of this book is to show how the foregoing problems may be approached by means of the calculus of finite differences and by interpolation formulæ based thereon. A uniform notation for both differences and sums has been used throughout which makes the formulæ obtained readily intelligible. The full chain of reasoning has been presented in deriving methods of interpolation, differentiation, and integration, but the author has kept in mind that his end is essentially a practical one and plenty of numerical illustrations, mostly based on astronomical data, are given. A very valuable feature of the book is the admirable set of tables which present the interpolation formulæ of Newton, Stirling, Bessel, and Gauss, each accompanied by interpolation coefficients up to the sixth difference, together with tables of the formulæ of practical differentiation and integration and the necessary numerical coefficients. This book should certainly be in the hands of all who are interested in the numerical problems arising from tabular material.

L. M. M.-T.

L'Évolution des Idées géométriques dans la Pensée grecque: Point, Ligne, Surface. Par Federigo Enriques. Traduit sur la troisième Édition italienne par Maurice Solovine. [Pp. viii + 45.] (Paris: Gauthier-Villars et Cie, 1927. Price 12 fcs.)

Prof. Enriques traces in this tract the gradual refinement of geometry, from the measuring practice of early Egyptian civilisation to the logical treatise of Euclid.

After reviewing the material handled on to the Greeks and the course of their own discoveries of new geometrical facts, he examines the history of the removal of obstacles to abstract rational *proof*. The more important of these were the notion of a point having size (held in the Pythagorean School, side by side with an atomic theory of the universe), and the difficulties of incommensurables and of infinity. The Greek way of dealing with these led to Aristotle's definitions, axioms, and postulates, which formed the essential foundations of Euclid's work. Finally, the author gives an interesting survey of the nature of the foundation required for the ordering

of any deductive science according to the refinements of Modern Logic, and of the effect of the use of intuition in such a foundation.

The book gives a clear picture of the relative places of the works of the Greek Schools, of Euclid and of Archimedes; as well as an account of the logical criticisms of the last century, leading to the broader fields and more facile methods of modern geometry. It is the French translation of the first of a series of twenty-two reports on *Questions relating to Elementary Mathematics*, drawn up by various authors under the editorship of Prof. Enriques.

Those who study mathematics in the Universities to-day have ample opportunity for losing connection with elementary questions in a cloud of higher developments—even though they acquire great technical skill. The series (all of which are to be translated and published in French) is designed to counteract this tendency and, more particularly, to give those who are to teach mathematics a clear grasp of the historical point of view, which sees the germ of higher researches in elementary problems.

We hope that the present investigation may be of use in clearing away the geometrical confusion that is common among students and teachers in England.

F. B.

PHYSICS

Vorlesungen über Theoretische Physik. Von Prof. Dr. H. A. Lorentz. Band II, Kinetische Probleme. Band III, Aethertheorien und Aethermodette. [Pp. vi + 136 and 78, with 23 and 19 diagrams, respectively.] (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1928. Price, brosch. M.17, geb. M.18.)

The book before us is the second of the series on mathematical physics written by students of the late Prof. H. A. Lorentz and based directly on the lectures which he delivered at the University of Leiden. It contains the second and third volumes of these lectures. The second volume, on problems in kinetic theory, is contributed by Dr. E. C. Bruins and Dr. J. Rendler. It is mainly concerned with the hydrodynamic and kinetic treatment of viscosity, and a great deal of attention is devoted to Knudsen's investigations with rarefied gases. It also deals with the kinetic theory problems associated with the emission of electrons by hot bodies and with contact differences of potential. The final chapter of this volume includes an interesting section on the kinetic energy of the electrons in an electric current, and is very typical of the lucidity for which Lorentz was famous. The third volume, on models and theories of the ether, is contributed by Dr. H. Bremekamp. In it are described, from the viewpoint of the mathematical physicist, the important researches which have been devised to test the various hypotheses concerning the structure of the ether. The models of the ether are described, for the same reason, no doubt, that Lorentz described them in later years, in order that they may enable us to visualise the phenomena more easily, and, perhaps, to lead to further research. Very complete lists of references are given.

L. F. B.

Intermediate Electricity and Magnetism. By R. A. Houstoun, M.A., D.Sc. [Pp. x + 170, with 155 diagrams.] (London: Longmans, Green & Co., 1928. Price 4*s.* 6*d.* net.)

A good intermediate textbook is always of interest to all engaged in teaching physics, and Dr. Houstoun's latest book is certainly very good. It does not, however, possess the charm and distinction which characterise his intermediate textbook of light, although it is as clearly written and as well supplied with diagrams and examples. The chapter on cathode rays, X-

rays, and radioactivity might with advantage be enlarged, and we hope that in a future edition Dr. Houstoun will omit the description of the experiment which is wrongly supposed to illustrate the production of mechanical pressure by cathode rays. L. F. B.

New Worlds for Old. By R. G. Lunnon, M.A., M.Sc. [Pp. v + 106.] (London: Methuen & Co., 1928. Price 2*s*. 6*d*. net.)

In this book the author attempts to give a thoughtful person with no previous knowledge of physics a review of the results of modern research. His language is restrained and concise, and he gives an adequate account of all the important phenomena which are likely to interest such a person. L. F. B.

The Rise of Modern Physics: A Popular Sketch. By Henry Crew, Ph.D. [Pp. xvi + 356, with 38 illustrations.] (London: Baillière, Tindall & Cox, 1928. Price 22*s*. 6*d*. net.)

This work by the Fayerweather Professor of Physics in Northwestern University, Illinois, is based upon a course of lectures delivered by the author to undergraduates with no specialised knowledge of science. Hence it is intended for the general reader, as well as for the professed student of physics.

The field covered is a wide one, for the book begins with the earliest known Eastern modes of measuring mass, time, and angle, and ends with Bohr's theory of the atomic spectrum of hydrogen. The first hundred pages serve to summarise the Greek, Arabian, and Mediæval European contributions to the stock of mechanical and physical facts and ideas. The modern age of experiment, beginning with Galileo, is then traced out with increasing detail, and in the later chapters we follow the growth of the several branches of physics through the period when these were developing along independent lines. The closing chapter shows the convergence of these distinct lines of advance in the modern science of spectroscopy. No attempt is made to deal with Relativity: the word does not even appear in the index.

We can recommend Prof. Crew's treatise as a wise selection of what is essential from the mass of facts confronting the historian of physics. The material has been summarised without loss of clearness, and the style has the freshness and vitality which we associate with the American college textbook.

There are several slips in the text, besides the more obvious printer's errors. The diameters of the successive "Newton's Rings" (p. 161) are proportional, not, as stated, to the successive natural numbers, but to the square roots of these numbers. "Glazehook" on p. 131 (footnote) should read "Glazebrook," and the correct title of Heath's work referred to on p. 24 (footnote) is *A History of Greek Mathematics*. Other passages which might be modified in future editions occur on p. 31, where the plane of the ecliptic is described as "the plane of the sun's motion about the earth as we now say," and on p. 95, where the human eye is classed with the periscope among "well known modern examples" of the *camera obscura*. Again, it appears rather a bold claim to make for Hipparchus, the discoverer of precession, that he "showed that the motion of the earth is essentially that of a boy's spinning top" (p. 32). It is unlikely that Hipparchus advanced beyond the stage of explaining the fact of precession as due to a rotation of the sphere of stars about the pole of the ecliptic.

The book is generously illustrated with twenty-four full-page plates, most of them portraits of noted physicists; and there are explanatory figures in the text. There is a brief bibliography and an adequate index. It is to be feared, however, that the high price of the work may restrict its circulation among those for whom it is intended. A. Armitage.

Wien-Harms Handbuch der Experimentalphysik. Band XIX. Dispersion und Absorption, von GEORGE JAFFÉ. Medien mit veränderlichem Brechungsindex und Lichtzerstreuung, von RICHARD GANS. [Pp. viii + 430, with 77 illustrations.] (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1928. Price: brosch. M. 39, geb. M. 41.)

WE so often find in our textbooks on light that the learned and lengthy discussions of the theory of dispersion are quite unaccompanied by any satisfactory description of the experimental side of the subject. It is therefore a pleasure to read an important work in which the experimental side is given the attention it deserves. Prof. George Jaffé, of Giessen, opens his treatment of dispersion and absorption with an interesting historical introduction which is written in a very attractive style. Then, within the bounds of some fifty pages, he concisely reviews the more important theories of dispersion, and discusses the various formulæ to which they lead; after which he proceeds to describe the investigation and the detailed results of the dispersion of gases, liquids, and solids. His subject-matter is very up-to-date; for example, he describes Wetterblad's prism method, for measuring the dispersion of gases in the infra-red region, which was published as an Upsala dissertation of 1924. It is also very complete; thus, in his treatment of the dispersion of gases he devotes sections to all the important methods of investigation, prism methods, interference methods, differential interferometer methods, and grating methods. Moreover, some attempt is made to indicate the relative value of the various methods. Experimental results are discussed at length in separate sections, the discussion of cases of anomalous dispersion being particularly clear and complete. A chapter is devoted to a discussion of the theoretical significance of the experimental results, and this includes sections on the number of dispersion electrons and on natural frequencies. The treatment of absorption is equally complete.

Prof. Gans, of Königsberg, is responsible for the two remaining portions of the book. The first of these deals with media of variable refractive index. It includes sections on astronomical refraction and Schmidt's theory of the sun's disc. The second portion deals with the scattering of light by small particles, and includes a description of the Pulfrich photometer for measuring the turbidity of a medium.

Volume XIX is therefore another important addition to our valuable books of reference. The printing and illustrations are of the same high standard that we have commended in the reviews of the earlier volumes of this Handbuch. It has been mentioned that in this volume some attempt is made to compare the value of different experimental methods, and we feel that a more critical attitude of the various contributors to the Handbuch might render it even more valuable than it is at present.

L. F. B.

CHEMISTRY.

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. MELLOR, D.Sc., F.R.S., Vol. VIII, N. & P. [Pp. x + 1110, illustrated.] (London: Longmans Green & Co., 1928. Price 63s. net.)

"THE fixation of nitrogen is vital to the progress of civilised humanity, and unless we can class it among the certainties to come, the great Caucasian race will cease to be foremost in the world and will be squeezed out of existence by races to whom wheaten bread is not the staff of life."

When Sir William Crookes startled the world with these words in 1898 they acted as the catalyst to a series of scientific and industrial achievements of a magnitude almost without parallel in the history of applied science. The importance of nitrogen in the life cycles of animals and plants was indeed known, but that the world was in danger of actual nitrogen starvation within

a measurable period was a new and alarming conception, and it was soon recognised that the solution of the problem of nitrogen fixation on a large scale was one of vital importance to humanity.

As a result of a generation of research in university laboratories and chemical works chemists have once more come to the rescue of mankind and by their efforts have achieved a victory the stupendousness of which is hardly known to, and certainly not realised by, most of the world's inhabitants.

Inevitably these investigations have led to a renewed interest in the chemistry of nitrogen, and for the large number of chemists engaged in these investigations Dr. Mellor's latest volume will be more than welcome. One could wish that even more space had been devoted to the discussion of the actual problem of nitrogen fixation, but that is perhaps asking too much, especially as the literature references given are very full, though somewhat surprisingly there appears to be no mention of the important Government Report on Nitrogen Fixation, published just after the Armistice.

Among minor omissions may be noted the absence of any reference to the industrial importance of sodamide for the commercial synthesis of Indigo, and on page 360, in discussing the photosynthetic work of Baly and Heilbron, there is no mention of the serious criticisms made by Snow and Stone.

The remaining third of the book deals very fully with the chemistry of Phosphorus and enables one to realise the vast amount of work that has been done on this strange and ubiquitous element.

It is scarcely necessary to add that Volume VIII is fully "up to standard" and on the principle that "a good wine needs no bush" it is quite unnecessary for the reviewer to do more than state that Volume VIII is now published and leave the chemists and librarians to do the rest!

F. A. Mason.

BOTANY

Truck Crop Plants. By H. A. Jones and J. J. Rosa. [Pp. xiv + 537.] (London: McGraw-Hill Publishing Co., 1928. Price 25*s*. net.)

Truck-farming, or the growing of vegetables on a large scale at a more or less great distance from a market, is a modern phase of vegetable production of such importance that it merits special divisions in certain of the American universities. Vegetable growing is passing out of the domain of horticulture into that of field crops, though connection is still maintained in the marketing. In this age of specialisation the old-time market gardening has ramified into several branches, the chief being seed growing, vegetable forcing, home gardening, canning and pickling crops, market gardening proper, and truck farming, but several types of production are still often carried out on the same farm.

A good deal of scattered field research has been done on truck crops, but a broad survey of the literature was needed, and an endeavour is made to supply this in the present text-book which is not intended primarily for elementary students, cultural details being therefore omitted. The basis of discussion is the plant itself, and any appropriate branch of scientific knowledge is applied to explain cultural practices and the response of the plant. Owing to the rapid development of truck farming information as to the economics of manuring the crops is still fragmentary and little attempt is here made to deal with it. On the other hand, the importance of insect and plant pests is fully realised, their control being even more urgent than the need for manuring, and particular attention is devoted to them.

Truck-farming, with its distant markets, involves a very close correlation between adaptation of the crops to soil and climate and to the expected time of marketing, the latter of which is all-important from the point of view of the prices realised, and attention is concentrated on one or a very

few crops on each farm. Shipment is usually in car lots, and the development of truck-farming has proceeded alongside the improvement of transport systems and the installation of refrigerating cars. Competition between truck crops supply and local competition has to be carefully considered. As the crops are perishable, large fluctuations of over- and under-supply occur, as crops cannot be held over, if in excess, against a time of dearth, as is the case with wheat and similar crops. The future of truck-farming probably lies in production at lower costs rather than in the possibility of higher selling prices. Hence the importance of the study of the plant and its needs, and of the best methods of handling and manuring soils, as increased production will depend more on intensive cultivation and less upon occupying new areas of virgin soil in America.

Truck crops are widely distributed among the natural orders, only one or two genera in each order supplying the major crops. On the other hand, one genus, as *Brassica*, may supply many species of the utmost economic importance. Of the twenty-seven described, twenty are of importance in England. The general method of treatment of each crop is prefaced by a description dealing with taxonomy and history, followed by an account of the plant characteristics, with emphasis upon that part of the plant valued as a vegetable, as stem in asparagus, tuber in sweet potato, and fruiting spike in Zea mais. Methods of cultivation, harvesting, and marketing are then dealt with, together with special information relating to the individual crop, as canning of asparagus and bolting of brassica. Finally, for each crop the main insect, fungus and bacterial pests are indicated, with an outline of the methods of control that may effectively be adopted. A number of clear illustrations emphasise the salient points in crop development, and sufficient tables are provided to clarify various points with regard to manuring and marketing without overburdening the reader with masses of detailed statistics.

WINIFRED E. BRENCHLEY.

The Garden Interests of Madeira. By M. C. GRABHAM, M.D., LL.D. [Pp. xii + 100, with three plates.] (London: W. Clowes & Sons. Price 5*s*. net.)

THE greater portion of this little work consists of an enumeration of the more striking flowers of the Madeiran Flora dealt with under their respective families. This is preceded by an introductory section in which the chief features of the floral pageant are dealt with in their seasonal succession. These two sections contain a considerable diversity of information, miscellaneous in character but refreshing in its inconsequence.

There is no attempt at narrative and it is hardly a book of reference, for the reader will rarely find similar information respecting the different species, but numerous points are commented upon which should prove of interest both to the casual visitor and the inhabitant.

We may note that several introduced plants are cited as pests amongst which the Asparagus Fern, four species of *Oxalis*, *Eupatoriam adenophorum*, *Allium fragrans*, and *Solanum auriculatum* call for especial comment. The tendency for the introduced Gorse to oust the Broom is also noteworthy.

E. J. S.

ZOOLOGY

Animal Biology. By J. B. S. HALDANE and JULIAN HUXLEY. [Pp. xvi + 344, with 122 illustrations.] (Oxford: at the Clarendon Press, 1927. Price 10*s*. net, cheap ed. 6*s*. 6*d*. net.)

THIS book is not addressed to the professed biologist, but to the general public. Consequently it should be criticised from the standpoint of the average educated reader. The most that can be asked of a book of this kind

is that the author should give his defenceless readers the maximum of unadulterated biological facts and the minimum of his personal predilections. Failure to comply with this requirement is one of the chief dangers of popular science, and Messrs. Haldane and Huxley seem, on the whole, to have avoided it to a greater extent than is commonly the case. Another danger of popular science arises out of the difficulty of telling the truth in popular language as science progresses and so gets further removed from common-sense experience. The general reader, especially if his taste has been vitiated by persistent newspaper reading, prefers cut-and-dried assertions to carefully weighed and properly qualified alternatives. But with care it is still possible in biology to say much that is both interesting and important without being misleading and without palming off personal metaphysical prejudices upon the public in the name of science. A third danger, and one which is closely allied to the first, depends on the fact that so many authors find it necessary not only to offer their wares as intrinsically interesting and as sufficiently worth having for their own sake, but also as immensely useful and important in some practical sphere or other. One consequence of this is that the purely utilitarian aspect of science is over-emphasised in the popular mind, and science becomes associated with various forms of propaganda. But it is not easy to see why intellectual values should not be as intrinsically meritorious as, say, æsthetic ones, and why those who pursue the first should not be as satisfied with them from that point of view as those who pursue the second. Practical applications of science usually have their negative as well as their positive side, and if science is to claim credit for the latter it also saddles itself with the responsibility for the former. The dangers are particularly great in too hasty applications of biology to human affairs on account of the uncertainty of inductions in the non-mathematical sciences, and the great differences of opinion which exist on every topic. Moreover, if the man in the street is taught to value science primarily for its practical applications he may come to neglect those values which most distinguish men from the beasts, because they do not happen to provide him with rapid means of transport, telephones, surgery, tinned food, high explosives, and other ministrations to his material needs.

Messrs. Haldane and Huxley are of opinion that "only by a judicious combination of eugenics and good education can we get the best out of humanity; and biology is basic for both." But some people would point out that ancient Greece, in some directions at least, got the best out of humanity before either eugenics or biology was dreamt of. Experts never seem to be able to see these general sociological problems from any other angle but their own. Consequently, while everyone will welcome any help offered by positive additions to biological knowledge, sociologists and educationists would be well within their rights to emphasise the fact that they are primarily concerned with those characteristics of man which do not come within the scope of biological science, and that it is *their* business to assess the importance of the auxiliary sciences. The average biologist is apt to confuse psychology with the physiology of the nervous system, and eugenics is a good example of a topic upon which expert opinion is divided even as regards physical qualities. This is clear from a recent critical article by Prof. Raymond Pearl [*Verhandlungen des V. Internationalen Kongresses für Vererbungswissenschaft*, Berlin, 1927. Bd. I, Leipzig, 1928, p. 261].

Messrs. Haldane and Huxley apparently take the view that thought has been an essential factor in human evolution, and this seems to be the only consistent thing to do if you believe in eugenics. If you prefer to speak of "laryngeal habits" instead of thoughts you enjoy the advantages of what Whitehead calls "homogeneous thinking about nature," but it then becomes difficult to make sense, or at least common sense (to which a popular book must address itself), of the proposition that the exercise of laryngeal habits

will lead to the increasing perfection of the human race. On the other hand to carry out a programme of psycho-biology is exceedingly difficult. It usually ends in a hopeless confusion of psychological and neurological terminology, and a mixture of interpretation which is neither good neurology nor good psychology. The authors of *Animal Biology* appear to use the terms brain and mind almost as synonyms, with a preference for the former. On p. 143 they speak of a part of the brain as being "concerned in putting sensations together and interpreting them." The extremely ambiguous term "sensation" is still responsible for a great deal of confusion in neurological literature. By using it first in a neurological and then in a psychological sense great metaphysical feats can easily be accomplished. In the present case, if by this term is meant an afferent neural process then doubtless there is a "putting together" in the brain, but what does it mean to say that this organ "interprets" them? Can "sensations," in *this* sense, be said to be interpreted at all? It would be better to omit the word "interpret" from neurological nomenclature. On the other hand, if by "sensation" is meant *what is sensed* then we are talking psychology and there is primarily no "putting together." When a medical student looks through a microscope for the first time what he sees is a confused blur of colour which he has first to *analyse* if he is ever to learn anything. He does not see "sensations" and proceed to stick them together. If you are talking psychology you must begin with what he sees, not with what you suppose to be going on in his brain. Most physiologists are constitutionally incapable of understanding this elementary point. But such confusions are not likely to disturb the general reader. Every novel he reads is a psycho-biological mixture and therefore he will feel most at home with a book which ignores such difficulties.

Messrs. Haldane and Huxley have succeeded in compressing a great deal of good and fresh information into their three hundred and forty pages, and the pictures are copious and extremely good. There seems to be some unnecessary repetition. We are told about thyroids on p. 162, and in the succeeding section much the same information is given on p. 190. How anti-diphtheria serum is prepared is told on p. 166, and again on p. 189 in the following section. It would appear that the two authors have written these two sections separately. Also on p. 204 it is explained that palæontological data do not give information about the method of evolution, and this is repeated on p. 253. The authors describe the usual theories on this problem and decide in favour of natural selection. They point out that the facts which are explained by the other theories can also be explained by the one they favour. It seems strange that people can still differ violently about this question when it appears, from the nature of the case, to be one which can never be decisively settled. Each side can claim to explain what the others explain since the course of the explanation depends on which of several alternative assumptions you choose to start from. From the nature of the case it is very difficult to form an estimate of the relative probability of these alternatives. For the guidance of the general reader it might be pointed out that the doctrine of natural selection is not, and does not as such profess to be, an explanation of the origin of species, although it is associated in the public mind with the title of Darwin's book. It explains why certain variations, having appeared, did not survive. The Darwinian theory appeals, like orthogenesis, to the occurrence of spontaneous inheritable variations. But the Darwinians prefer to regard their appearance as "accidental" and the supporters of orthogenesis prefer to say that they appeared according to an immanent law. The former have to make large drafts on time in order to get enough accidents in the right direction—a difficulty which does not confront orthogenesis. The Lamarckians can also take shelter behind time in order to escape the criticism that they have not yet succeeded in producing convincing experimental evidence of their contentions. They also have

to appeal to the "capacity" of the organism to do just what they require of of it. Why *should* the lining of a sea-gull's stomach alter so as to become thicker and more like that of seed-eating birds when it is fed on corn? Why *should* the muscle of a dog's bladder thicken and become striated when the quantity of fluid it has to deal with in a given time is artificially increased? It would be very difficult to explain these facts and the familiar facts of regeneration on *any* theory. As Messrs. Haldane and Huxley point out, "regeneration is a primitive property of life." The traditional theories of evolution are far too simple. All scientific theories begin by being simple and end by being complicated, and the fact that this has not happened in the case of the theories of evolution is perhaps a bad sign. They can claim the merit of having provoked a great deal of valuable research, even although the results have not always been in accordance with what was expected. The wisest attitude to take, from the scientific point of view, seems to be that of Johannsen, when he says: "In Wirklichkeit ist das Evolutionsproblem eigentlich eine ganz offene Frage." [W. Johannsen: "Experimentelle Grundlagen der Deszendenzlehre," in *Kultur der Gegenwart*, Teil III, Abt. iv, Bd. I, p. 659.]

The usual care of the Oxford University Press has been plenteously bestowed upon this book, and their skill has combined with the learning of the authors to make it a very attractive volume. J. H. W.

Brachiopod Morphology and Genera (Recent and Tertiary). By J. Allan Thompson, M.A., D.Sc., F.G.S. [Pp. vi + 338, with 103 figures and 2 plates.] (Wellington, N.Z.: Manual No. 7, New Zealand Board of Sciences and Art, 1927. Price 17*s*. Obtainable from the Government Printer, or The High Commissioner for New Zealand, The Strand, London.)

This manual will be welcomed by all students of the Brachiopoda. The author's attitude to his subject and the method which he has applied to the systematic account of the Recent and Tertiary Brachiopoda are indicated by these words in the Preface: "The interpretation of the distinctive characters requires that morphology shall be treated, wherever possible, by the method of morphogeny—the determination of the sequence of forms resulting in the existing structures—and that classification shall be based as far as possible on morphogenetic considerations. Towards this goal Beecher and Buckman have blazed a clear trail, and in this manual the attempt has been made to carry it a few steps further."

The systematic account of Recent and Tertiary genera is comprehensive and includes descriptions of a number of new genera. This account is preceded by a chapter in which the author proposes a new Major Classification. The Brachiopoda are divided into two sub-classes based primarily on the genesis of the pedicle in living forms. The sub-class Gastrocaulia, approximately synonymous with the Inarticulata of Huxley, includes the Orders Atremata (Obolacea, Lingulacea, Siphonotretacea) and Neotremata, comprising the superfamilies Paterinacea (Paterinidæ, Schuchert), Acrotretacea, Discinacea, and Cranacea. The pedicle develops within the protegulum from the ventral mantle lobe, and the mantle lobes develop without revolution. The author regards the succession of hemiperipheral by holoperipheral growth in the ventral valve as a significant index of the affinities of the Paterinacea, yet in *Micromitra* there is mixoperipheral growth and a symbolothyrid foramen; according to the author's amended diagnosis these are ordinal characters of the Atremata. The sub-class Pygocaulia, approximately equivalent to the Articulata, includes in addition to the Protremata and Telotremata, the new order Palæotremata, comprising the Rustellacea and Kutorginacea. In the Pygocaulia the pedicle develops from the caudal segment of the

embryo, and is never enclosed within the shell; the mantle lobes revolve in developing.

The first part of the volume, comprising over one hundred pages, is devoted to a general account of brachiopod anatomy, development (embryonic and post-embryonic), shell structure and morphology, and recapitulates at some length the work of Beecher, Schuchert, S. S. Buckman, and other authors. For the description of shell morphology the author adopts the terminology used by Buckman and devotes a section to the description of this author's method of calcining the test to reveal internal characters. There is a chapter on the range in time and distribution of the Australian Tertiary Brachiopoda by Irene Crespin and Frederick Chapman, and a very comprehensive bibliography of Recent and Tertiary Brachiopoda. Illustrations in the text, for the most part reproductions of figures in other memoirs, are numerous and good.

John Weir.

The Brain from Ape to Man. A Contribution to the Study of the Evolution and Development of the Human Brain. By Frederick Tilney, Ph.D., M.D. With Chapters on the Reconstruction of the Grey Matter in the Primate Brain Stem, by H. A. Riley, A.M., M.D. Two vols. [Pp. xxvii + 1120, with 557 illustrations, of which 35 are coloured.] (New York: Paul B. Hoebner. Price $25.00; London: H. K. Lewis & Co. Price £5 5*s*. net.)

These two volumes, weighty in more senses than one, represent many years of work on the part of one of the most outstanding neurologists. In recent years the United States has been the scene of a very curious and atavistic outbreak of obscurantism on the part of a group of people known for some inexplicable reason as "Fundamentalists," and the method of presenting the facts and the style of writing in the present volumes have been influenced by a desire to combat the position adopted by such persons. This is in no way detrimental to the book, but the attitude may appear strange to one not aware of the reason underlying it.

Much work has been done on the primate brain, both from the specific and comparative points of view, and particularly in the matter of the development of the cerebrum, its convolutions, and the localisation in the neo-pallial cortex thereof of the motor and sensory areas. The present work, in addition to the convolution pattern, follows a less frequented path in attacking the brain-stem and its various nuclei. The animals chosen for study have been arranged in four groups as follows: The lower Primates, including *Lemur mongoz, Tarsier spectrum, Callithrix jacchus* (the Marmoset), *Mycetes seniculus* (the Red Howling Monkey); the intermediate Primates, including *Papio cynocephalus* (the Baboon), *Macacus rhesus, Hylobates hoolock* (the Gibbon); the higher Anthropoids, including *Simia satyrus, Troglodytes niger*, and *Troglodytes gorilla*; and lastly *Homo*. In the first three groups the work on each species is divided into two chapters, one dealing with the behaviour of the animal and the structure of the brain, and the other, written by Prof. Riley, on the reconstruction of the grey matter in the brain-stem. Each section also terminates with a reasoned "comparative summary of structures having evolutional significance in the brain-stem." In the case of *Homo*, the behaviour portion has been expanded into a long, interesting chapter dealing with the origin of and trends of behaviour in primitive and modern man. There is also added a chapter on the brains of primitive man, wherein we find, among a vast deal of other information, that the author deduces from the prominence of the inferior frontal convolution in *Pithecanthropus* "that he added one supreme advantage to the motor equipment of animal life. *He had learned to speak*—to communicate in verbal language." Lastly, the fifth part of the work is devoted to the evolutional modifications of the

primate cerebrum culminating in the human brain, a useful bibliography for further reading, and an extensive index that gives ready access to any information desired.

Each animal is dealt with similarly and the corresponding structure treated in order and in a uniform manner, so that even without the index or excellent summaries it is easy to make comparisons. The volumes contain an enormous amount of information regarding the structure of the brain-stems of the various animals, illustrated by a wonderful series of photographs and drawings, so that one can follow the details in a manner not usually possible in works of this sort. In addition to these strictly morphological chapters the functional activities and behaviour dependent on the different structures are indicated and discussed in a truly fascinating manner.

The books appear very free from slips, but on p. 870 "restorationr" should read "restorations," and on p. 84, in discussing the position of Tarsiurs, it is stated that the "Royal" Zoological Society of London expressed its opinion on this subject. What is really meant is that in a discussion at a meeting of the society there was general agreement. "Royal" is erroneously, sometimes, but not always, prefixed to Zoological Society in the list of references.

It is interesting to note that the author definitely removes the Gibbon from the higher anthropoids, and also places man in a separate family determined by five "hominid" characters. These are: (1) the human brain; (2) the human foot; (3) the human hand; (4) the erect posture with bipedal locomotion; and (5) a terrestrial mode of life; but are not the last four all covered by or absolutely dependent upon (4)? Man is not an ape, but the apes are related Primates, and "They have become most effectively arborealised, have ascended well up into the trees, where doubtless they will remain, quite as unconcerned in the origin of man as they are innocent of participation in it."

For the book as a whole there is nothing but praise; it is well printed and beautifully illustrated. The technical portions are lucid and the more general matter written in an interesting and gripping manner. It will be welcomed by anatomists, zoologists, neurologists, psychologists and indeed by all interested in the immediate ancestry and relationships of man. It will long prove a veritable mine of information. We congratulate Prof. Tilney and his co-worker most heartily on this large and important work, and shall look forward eagerly to the other parts he promises that will complete a survey of the vertebrate brain.

C. H. O'D.

The Skate *Raja erinacea* Mitchell: A Laboratory Manual. By Charles W. Creaser, Ph.D. [Pp. x + 57, with 2 text-figures.] (New York: The Macmillan Co., 1927. Price 4*s*. 6*d*. net.)

This text is intended as a laboratory guide for junior classes with no previous knowledge of dissection. The preface puts forward the claim of the skate as a superior form for an introduction to a primitive type, but does admit that its body form is so highly specialised that it is necessary to introduce demonstration material to illustrate a generalised form—an obvious drawback. The directions given, in part, apply to the author's own laboratory routine and provision of material. The two text-figures are poor reproductions of figures in Parker's Zootomy, and strangely enough there is no reference to either Rand or Rand and Ulrich's papers on the species chosen. One useful feature that might be copied with advantage by other texts, particularly where the species supplied to a laboratory are liable to vary, is a key to the common skates and rays of the Atlantic Coast of North America.

There are certain curious statements that are liable to mislead junior students, *e.g.*, it is stated that "The embryonic first afferent artery of the

first arch, the spiracle, is not persistent in the adult skate. . . ." Apart from the fact that the spiracle is not an arch but a cleft, it has an afferent vessel in the adult, namely, the vessel termed by the author the "hyoidean artery." The "fourth cranial"—it does not say what, but means nerve—is stated to arise from the mesencephalon, and the fifth from the medulla, thus leaving the metencephalon without a nerve.

We cannot see that there will be much use for this text, at any rate in universities, and it will certainly not replace the account of the skate in Parker's Zootomy.

C. H. O'D.

Sheep Production. By LEVI JACKSON HORLACHER, B.S.A., M.S. [Pp. x + 418, with 137 text-figures and a number of tables.] (New York: McGraw-Hill Book Co., 1927. Price $4.00 net.)

THE author's aim is to provide a guide to the judging, feeding, breeding, management and marketing of sheep and wool more particularly for the use of classes in sheep husbandry in Agricultural Colleges. He has certainly been successful. The book is divided into four sections, dealing with: I the history and development of the industry, distribution, anatomy, judging, feeding and breeding; II a description of the various breeds; III the management of the flocks; and IV a glossary of the terms used in connection with sheep and wool. As might be expected, the part dealing with anatomy is brief, of little use to the comparative anatomist, but adequate to provide a satisfactory basis for the other parts of the book.

The University of Kentucky has always taken a foremost place in the sheep husbandry in the United States, and, as the author is a member of the staff there, it is only natural and wise that the information and experience accumulated there have been largely drawn upon. The first section of the book covers a wide field and gives a short account of the laws of genetics in order that breeders may understand and utilise them. It also contains some informative historical data. The second section describes concisely the more important breeds of sheep and illustrates them by numerous excellent photographs of prize animals. The third section deals with the practical aspects of sheep management, mutton production, and wool production and sale, and is written in a thoroughly straightforward and lucid manner by one who is well acquainted with his subject. The glossary in part four is very useful.

The only criticism we feel called upon to make is that while some references are given as footnotes it would have added to the usefulness of the book to those who wish to go further into certain points if a literature list had been provided either at the end of the whole work or at the end of the various chapters.

The book will be more particularly useful to agricultural students in North America since it is principally concerned with conditions there. It is, however, by no means narrow in its outlook, and anyone concerned with sheep, student or breeder, cannot afford to neglect it wherever he may be. The more general parts and the accounts of the breeds are also of interest to the general zoologist, and it forms a handy book of reference on many points usually omitted from the ordinary zoological textbooks.

C. H. O'D.

Animal Life of the Carlsbad Cavern. By VERNON BAILEY. [Pp. xiii + 195, with 67 figures on plates.] (Baltimore: The Williams and Wilkins Co. Price 13*s*. 6*d*. net.)

IN the arid foothills of the Guadalupe Mountains in New Mexico a dozen or so miles from the border of Texas are a number of canyons and caverns in the limestones of the Pecos River Valley. This region, with its characteristic vegetation of mesquite, ocotilla, sotol, yuccas and cacti, was, in the days

of Coronado in 1540, inhabited by the warlike Apache Indians, who were not finally driven from their cave strongholds until a little more than fifty years ago. Largest and most spectacular of these caverns is the one that takes its name from the town of Carlsbad, twenty-six miles to the north-east of it. The lowest point of this cave is 700 feet below the entrance; the largest room is 450 feet wide and its ceiling 250 feet high. This passes on and gives rise to other rooms and passages whose total length is many miles and it takes days even to cover the explored and known ramifications. Thus it forms the most extensive cave system in North America. It has long been known as the home of vast flocks of bats whose guano was dug out as a commercial undertaking and not until well over a hundred thousand tons had been removed did it cease to pay for working.

Mr. Bailey of the United States Biological Survey has spent a great deal of time in and around the cave, and in the present volume gives a brief account of the cave and a more extended one of the plants and mammals in it and the adjacent terrain. In spite of the arid nature of the surroundings they are wonderfully rich in animal life, as is evidenced by the fact that over fifty species of mammals are recorded therefrom. The life of the cavern is taken in its widest sense, for we have animals that live in the utter darkness of its recesses, those that live in the dim twilight near its entrance, those that habitually utilise the cavern mouths, those that resort to the caves in times of storm and stress, and those that are not strictly speaking cave dwellers or frequenters, but live in the parts immediately adjacent and so could not be missed by visitors to the caves.

The author is to be congratulated on the production of a book that can be read by layman and scientist alike with considerable pleasure and profit. It forms the third monograph issued by the American Society of Mammalogists, a series that promises to become indispensable to all students of the ecology of mammals. It is a very interesting book, and the printing and illustrations are worthy of praise as well as the subject-matter.

C. H. O'D.

Birds of the Ocean. By W. B. ALEXANDER, M.A.(Camb.). [Pp. xxiii + 428. With an Appendix and 88 illustrations from photographs.] (New York and London: G. P. Putnam's Sons.)

THIS is a handbook for the identification of the various species of sea birds which the ocean traveller is likely to meet from the North to the South Pole. The author, who has travelled widely in all the seas, has kept a carefully compiled notebook and has embodied his data in this very convenient little volume, which can be carried in the ordinary coat pocket. He admits the difficulty in deciding which species should be included and which omitted and has done wisely in describing all the species of the families dealt with, whether they are likely to be seen at sea or not. After describing an adult specimen and giving the range and some notes about the different species in the families which he has selected he gives an account of the North Atlantic and North Pacific Oceans, Tropical Seas and the Southern Oceans, and under each mentions the various species likely to be encountered out on the open sea and along the various coasts.

At the end of the book, in an appendix, he gives a systematic list of the birds and a good index.

There are some 88 plates of illustrations which the author must have been to great pains to select from the large number sent to him from various sources. They all admirably illustrate the text and assist the reader in his identification of the various species. They are about as good as could possibly be secured. The semi-diagrammatic sketches showing the colouring of the underparts of the birds on the wing will be found most useful, as this is the only portion of the birds visible, very often, from board ship.

The author gratefully acknowledges the assistance he has received from various ornithologists and institutions in this country and America in compiling this admirable little handbook. It is of a convenient size, the printing is good, and the type conveniently large without making the volume too bulky, a mistake only too frequently made in compiling handbooks for travellers.

I can find no error of omission or commission, and can strongly recommend the ocean-traveller not to forget to include it in his kitbag. Whether interested in birds or not, the spotting of the different species seen from day to day will add to the interest of the voyage and help to relieve the tedium inseparable from long sea journeys.

R. E. DRAKE-BROCKMAN.

Economic Biology for Students of Social Science. By PHILIPPA C. ESDAILE, D.Sc. Part I. [Pp. xv + 175, with 150 illustrations.] (London: University of London Press, 1927. Price 7*s*. 6*d*. net.)

DR. ESDAILE, as she justly claims in the preface, has filled a real want with this book. It is devoted to the "minor horrors of peace" and deals with them in a very comprehensive and competent manner. The present volume is Part I, and is devoted to "Harmful and Useful Animals," and is, therefore, not entirely confined to "horrors." Part II deals with "Animal and Vegetable Products." The subject-matter of Part I is arranged according to the system of classification of the animal kingdom; seven of the fourteen chapters are devoted to insects and the remainder to protozoa, porifera, platyhelmia, arachnida, and one to "the general care of the person and the house." The examples are described with the aid of ample figures for identification and full details of life-histories are given as well as advice regarding the control of pests. There is an ample index and a glossary. The book may be warmly recommended to those to whom it is addressed: "health visitors, sanitary inspectors, nurses, and institutional managers, as well as those who take a general interest in household matters."

J. H. W.

The Biology of Insects. By G. H. CARPENTER, D.Sc., Keeper of the Manchester Museum, University of Manchester. [Pp. xv + 473, with 16 plates and 88 text-figures.] (London: Sidgwick & Jackson, Ltd., 1928. Price 16*s*. net.)

THE Biology of Insects is the latest addition to a valuable series of volumes, which is being brought out by Messrs. Sidgwick and Jackson under the general editorship of Prof. J. Arthur Thomson, dealing with the biology of various groups of animals and plants.

In books on entomology two branches—structure and systematics—are usually so much emphasised that one is given the impression that entomology must be dull. This idea is so far-reaching that even among some zoologists there is a tendency, not without some justification, to regard entomologists as mere collectors and systematists. This volume should do a great deal to alter this idea.

Dr. Carpenter, having in his preface stated that the structural features of insects will only be described so far as is necessary for the proper understanding of function and behaviour and that systematics will be discussed only in relation to problems of ecology and evolution, goes on to deal in fourteen chapters with the structure, feeding and breathing, movement, sensation and reaction, behaviour, reproduction, reproduction and heredity, growth, family life, social life, adaptations to haunts and seasons, classification, evolution, insects and other organisms, and finally insects and man. Throughout the book one is struck by the wealth of information that has been accumulated and the readable manner in which it is presented.

The chapters on adaptations to haunts and seasons, insects and other organisms and insects and man are particularly interesting and well thought out. Though there is no original matter, the writer brings his subject quite up to date and affords, by means of abundant citations, the opportunity to follow up any particular line that may be required. There is a valuable list of references and an adequate index at the end of the volume.

One minor criticism seems necessary. When dealing with galls formed on plants, Dr. Carpenter continually refers to "gall-flies," meaning "gall-wasps." It seems a pity that, in such a book as this meant for general readers, a popular misnomer is perpetuated and unwittingly endorsed with authority. One notices that the cockroach is never referred to as the "black-beetle." Otherwise the book is well written and illustrated by means of numerous text-figures and plates.

H. F. B.

Text Book of General Zoology. By W. C. Curtis and M. J. Guthrie. [Pp. 585, with 308 illustrations.] (New York: John Wiley & Sons, 1927. Price 18*s.* 6*d.*)

The authors state, in the preface, that the essential feature of this book is that a limited number of animals are selected to illustrate certain biological principles and only incidentally as representative of particular phyla. That simple statement demands careful consideration. It shows that the authors are heretics so far as the old school of Zoology is concerned. It shows that they will approve no longer the system of teaching all zoology on a systematic basis, as has been done in the past. They have rebelled, and many zoologists in England and America are rebelling also. It is necessary, therefore, in reviewing their book, to inquire into this vexed question concerning the teaching of zoology.

The issue is whether morphology should remain the chief subject in teaching zoology or whether it should fall back and be treated as co-equal with the newer branches, such as genetics, embryology, cytology and histology, ecology, comparative physiology, etc. The problem has excited much discussion and is far from settled. It is of considerable importance in so much as it involves a complete change in the system of teaching, with consequent alteration of the whole curriculum. Many zoologists are in favour of retaining the old method, which is obviously the course of least resistance. Moreover, it is more easily adapted to the limitations of the teaching staff of any given institution. At present the other branches of zoology are fitted in to the morphological course wherever opportunity offers in connection with a suitable example. This method enables a teacher who is, for example, well acquainted with genetics, but ignorant of embryology, to treat of the former at considerable length and to virtually ignore the latter. On the other hand, if morphology was relegated to a place of equality with the other branches, the course in Zoology would have to provide equally for them all. This method would ensure that students were well grounded in all the chief branches, but it would also make a heavier demand on the teaching staff, especially when small. Many colleges, owing to insufficient staff, could not meet this demand at present.

Another advantage of the old method is the ease with which evolution can be taught with morphology. This consideration, perhaps more than any other, has bolstered up the old system. But it is fast declining now that genetics, etc., ecology, are of prime importance to the theory of evolution, and animal embryology has become a very specialized subject.

There can be no doubt, however, that the newer branches of zoology have attained dimensions which warrant their elevation to a position of equality with pure morphology. Moreover, they are advancing more rapidly. Added to this the demand for men qualified in any one of these branches is

at least as great as for systematists, and is increasing very rapidly during the last five years. Sooner or later, the change must come when comparative physiology and ecology, genetics and cytology, embryology and histology, will be given as much attention as morphology in the zoological curriculum. When that is attained one branch will have to be chosen with which the course can be begun, and to which the others can be linked up. Both habit and expediency will indicate that this subject should be morphology.

The book under review marks a step in this direction, although we doubt if many institutions are ready at present to adopt it. It is well written and contains many new illustrations, instead of the old ones which have been borrowed over and over again. We welcome it, as such, and recommend it to zoologists.

F. W. R. B.

Vertebrate Embryology: A Text Book for Colleges and Universities. By WALDO SHUMWAY, Ph.D., Associate Professor of Zoology, University of Illinois. [Pp. 314, with 195 illustrations.] (New York: John Wiley & Sons, 1927. Price 18*s*. 6*d*.)

THIS book deals with the comparative embryology of Vertebrates, and is intended for the use of university students who are taking a practical course at the same time. In consequence four types have been chosen, which are suitable also for laboratory work: Amphioxus, the Frog, the Chick and Man (supplemented in places by the pig). The book is divided into four sections. The first and largest section deals with fertilisation, segmentation, germ-layer formation and the embryonic membranes. The second deals with organogenesis. The third is a descriptive account of sections of embryos, with numerous illustrations. This section is good, and we regret that more of the book has not been devoted to it. The fourth section is devoted to microscopic technique and is useful from the student's point of view. On the whole, the book is clearly written and well illustrated. The terms used are in many cases not those commonly employed in this country, and the illustrations are sometimes too diagrammatic. We think the book is fair, but do not feel as enthusiastic about it as the author appears to do from his preface.

F. W. R. B.

The Biology of the Frog. By SAMUEL J. HOLMES, Ph.D., Professor of Zoology in the University of California. Fourth Revised Edition. [Pp. 386, with frontispiece and 112 illustrations.] (New York: Macmillan Co., 1927.)

NEARLY a quarter of a century ago this book made its appearance. Since then it has been widely used as a practical textbook on the frog. The new edition brings the book up to date, without damaging in any way its unity, spontaneity, or balance. It is a convenient, clear, and well-balanced book, well written and well illustrated. It deals in a simple, but remarkably thorough manner, with the anatomy and elementary physiology of the frog. It is, in the reviewer's opinion, rendered especially valuable by the chapter, comprising about a tenth of the whole, on the habits and natural history of the frog. Too often students of zoology study the structure and function of an animal without gaining any insight into the intimate affairs of its every-day life. They know it only as a subject for dissection or experiment in the laboratory, and not as one of the wild creatures of the countryside. This deficiency has been met by Prof. Holmes's intriguing account of the field natural history of the leopard frog (*R. pipiens*) of North America. Another chapter is devoted to the development and embryology. Excellent bibliographies appended to each chapter render the book useful to those who employ the frog for research purposes.

F. W. R. B.

Ciliary Movement. By J. GRAY, M.A. [Pp. 162, with 105 figs.] (Cambridge: at the University Press, 1928. Price 10s. 6d.)

THE publication of this book has been awaited with interest by many biologists. The subject of ciliary movement, although of obvious importance for vertebrate as well as invertebrate physiology, has been treated very inadequately up to the present time. The lack of a monograph on the subject has been felt for some time, and consequently the appearance of the present volume is opportune. The author has conducted an extensive series of researches into the subject and has embodied his results in a number of papers which have appeared in various journals, but chiefly in the *Proc. Roy. Soc., B.*, during the last eight years. The results are included in this monograph.

The book deals with cilia from the physiological, rather than the morphological, point of view. The text is divided into an Introduction and seven other chapters dealing with " The Movement of a Cilium," " Ciliary Currents," " The Force Exerted and the Work done by Cilia," " The Relation of Ciliary Movement to Ionic Equilibria," " The Metabolism of Ciliated Cells and the Nature of the Ciliary Cycle," "Metachronal Rhythm and Ciliary Control," and " The Distribution of Cilia and the Functions which they Perform." The titles outline the contents, which is presented in a clear and concise manner. The illustrations, chiefly diagrammatic, are well chosen and plentiful. Useful bibliographies are attached to each chapter and are included in an extensive bibliography at the end of the book. The book is intended for the serious student, and special care is taken to indicate where further research is required most urgently. It is essentially stimulating and should do much to encourage research in the subject. It enhances the series of manuals on Comparative Physiology of which it forms a part.

F. W. R. B.

Hunting Under the Microscope. By SIR ARTHUR E. SHIPLEY, G.B.E., F.R.S., Master of Christ's College, Cambridge. Edited by C. F. A. PANTIN, M.A. [Pp. 184, with 34 figs.] (London: Ernest Benn, 1928. Price 8s. 6d.)

IT is a difficult and delicate task to review a posthumous work a short time after the author's death. The book is the man's monument. It would be desecration to abuse it, but to praise it is like writing an epitaph. The book in question merits the praise, although there is an editor on whom we could have heaped blame, were it called for.

Hunting Under the Microscope is a charming book. It is written with the grace and wit which characterised its author and which have endeared his writings to a large and growing circle of readers. The book consists of a series of essays on subjects ranging from " Suspended Animation " to " Sir Ronald Ross and the Malaria Problem," but chiefly on micro-organisms such as " The Volvocinæ," " Water Fleas," etc. These articles contain a wealth of matter presented in a very entertaining and readable manner. Sir Arthur Shipley had that rare gift of imparting the glamour of the fairy-tale to scientific facts and theories. He possessed, also, a wonderful facility for driving home his point with one or two short, forceful phrases. For instance, when explaining the origin of body and germ plasm as illustrated in Volvox, he writes in this book: " The nutritive cells die after a time: they form a mortal body—something to make a corpse of." Can anything more be said ? Could anyone who read that ever forget his meaning ?

The book is written chiefly for the amateur, to encourage him to explore through the microscope the world of things that live in his water-butt. But the closing chapter, being a review of Sir Ronald Ross's *Memoirs*, points the path to greater things. Shipley shows in that chapter the simple steps which led to the conquest of malaria.

Mr. Pantin is to be complimented on the editing of the book. We recommend it to anyone who is interested, or who wishes to be interested, in the living world around him.

F. W. R. B.

MISCELLANEOUS

Harrison of Ightham. By Sir E. R. Harrison. [Pp. ii + 395, with plates.] (London: Oxford University Press, 1928. Price 15*s*.)

This book is made up principally from Benjamin Harrison's notebooks and the letters which he received and sent. Harrison was born at Ightham in Kent in 1837, and during the many years that he lived and laboured there was known as an ardent archæologist and an authority on the flint implements which he discovered in the village and the surrounding country.

Despite the fact that for the greater part of his life he had to keep a country general shop to maintain himself, his wife and children, the many disadvantages of lack of education (he left school when he was fourteen), the lack of money, books, and time for his archæological field-work, he was able to contribute much that was of scientific value to his era.

He will be principally remembered amongst archæologists for the position which he took up on the vexed question of the Eoliths. Following Sir Joseph Prestwich he asserted that the eolithic implements which he had found in the tertiary gravels around Ightham were of human manufacture. This was disputed at the time and has ever since been a question of much controversy.

The eolithic implements were exhibited at the Anthropological Institute in 1891, at the Royal Society's rooms in 1895, and at the meeting of the British Association for the Advancement of Science held at Dover in 1899.

In 1895 the Geological Society made Harrison a monetary grant, which was followed in 1899 by an annuity from the Royal Society, and later by a grant from the Crown which was subsequently increased, and in this fashion he was able, during the later years of his life, to devote himself exclusively to archæology. He died in 1921, aged eighty-four.

The book is admirably got up; the type excellent, the paper thick and opaque, the illustrations clear and of good quality; altogether, both in subject matter and form, a book worthy of the best traditions of the Oxford University Press.

L. J. P. G.

The Stone Age. By E. G. James, Ph.D., F.S.A. [Pp. viii + 202, with 38 illustrations.] (London: Sheldon Press, 1927. Price 3*s*. 6*d*. net.)

In writing popular summaries of what is known of man's earlier history the great danger is romancing. The writer may think that the narrative will be far more attractive if he fills in the gaps and rounds off the story. It may be permissible to do that if the reader is able to distinguish between the parts which are based on facts and those which are purely speculative, but no sharp division—if any division at all—is usually made between the fruits of empiricism and imagination. Of the many elementary accounts of man's prehistoric past which have been published within recent years this book appears to be the one which has kept most within the bounds of the known. Though greatly simplified, its form is similar to that of the manuals dealing with the subject which have been written by several recognised authorities. The writer evidently derived his material not from those manuals, but from the original researches, and he has himself contributed to them and visited the most important sites in Western Europe. He says that he endeavoured to concentrate on evidence rather than interpretation, and the story should

be none the less interesting to the general reader on that account. The entire Stone Age is dealt with. No previous knowledge of the subject is assumed and the book is well adapted for use in schools, since it outlines both methods and results. There are numerous line figures, three charts showing the succession of various periods, and select bibliographies appended to each chapter.

β_2.

Frequency Curves and Correlation. By W. PALIN ELDERTON. Second edition. [Pp. vii + 239.] (London: Charles and Edwin Layton, 1927. Price 15s. net.)

THE appearance of a second edition (1927) after an interval of twenty-one years gives the reader an opportunity of surveying the progress in methods and ideas which has been accomplished at least in one corner of statistical science. Mr. Elderton's book was from the first of a very special character. It was written specially for actuaries who were unfamiliar with the general development of statistical theory, by a distinguished actuary who had made himself familiar with the new and promising work of Prof. K. Pearson. It was special, therefore, not only in the class of readers for which it was intended, but in the source from which the methods were drawn. It would have seemed captious in 1906 to criticise the first edition, on the ground that this source had been drawn upon uncritically, and that in the wholesale importation of methods developed in a different branch of the subject, there was likely to be much unsuitable to actuarial needs, or even fundamentally wrong in itself. Yet at the present time such a criticism cannot be avoided.

It has been known since 1921 that the method of moments is only efficient, in fitting curves of the Pearsonian system, in the immediate neighbourhood of the normal distribution. Several of the distributions chosen as examples in the first edition deviated extremely from the normal form, and must have been very badly fitted. Yet, in the new edition the word "efficiency" does not appear in the index, and the same examples are used in the text.

The only example in the original chapter on Goodness of Fit was unfortunate in that, instead of illustrating the comparison between a series of observed frequencies and the corresponding series of theoretical expectations, the comparison is made between two series of percentages, based upon unspecified numbers of observations. This unlucky example is retained, save for a numerical correction. It gives a value of P exceeding 0·99, thus making the fit "too good to be true."

In the absence of any improvement in the efficiency of the methods of fitting, it would be too much to expect that the treatment of Goodness of Fit would have been adequately overhauled, for it has been demonstrated that with inefficient fitting the test of goodness of fit itself, as is but natural, becomes invalid. An addition of about a page has, however, been made to the chapter, in order to discuss the effect of the number of degrees of freedom absorbed in the process of fitting. The plea is put forward (p. 194) that "a departure from the ordinary application of the formulæ is likely to lead to confusion." The number of degrees of freedom appears now to be taken into account even by the staunchest original opponents of the new theory, and we must doubt if Mr. Elderton has succeeded in keeping abreast of the "ordinary" application of the formulæ. In quoting the high values found for the goodness of fit of his own examples of fitting, without mentioning that by more recent tests the fits are in some cases, if not in all, extremely unsatisfactory, the author is open to grave criticism.

A valuable change has been introduced in the numbering of the Types of the Pearsonian system of curves. In the original edition Type VII was omitted, and its number was given to the normal curve of error; the incon-

sistency with Pearson's usage thus caused has produced an immense amount of confusion. In the new edition the normal curve is unnumbered, and space is given for Type VII, but with only a dummy example.

It is much to be regretted that the author did not find it possible to carry out a thorough and critical revision of the whole work, for the method of presenting fully worked examples is excellent, and in the modern growth of interest in mathematical statistics, there is room for a book on the Pearsonian system of curves.

R. A. FISHER.

Man Rises to Parnassus. By HENRY FAIRFIELD OSBORN. [Pp. xix + 217, with frontispiece and 83 figures.] (Princeton University Press, 1927. Price 11*s*. 6*d*. net.)

IT has become the custom for the publisher to utilise the paper cover of a bound technical book by printing on it a preface of his own which sets forth the peculiar merits of the new work. On this wrapper it is said previous investigations have been "directed mainly to the anatomical characters of fossil man, but here the author chiefly stresses not these anatomical characters *per se*, but their bearing on our knowledge of the development of the mind and spirit of man." Numerous accounts have been written within recent years summarising what is at present known of man's prehistoric past, and Prof. Osborn's *Men of the Old Stone Age* is one of the best known. *Man Rises to Parnassus* purports to be a sequel to such manuals since, in the author's own words, "these lectures concern chiefly the higher faculties of man." They are a reply to those Fundamentalists who have been attacking recently all who profess to believe in human evolution. The reader is thus led to anticipate that some original deductions have been drawn from the researches of prehistoric archæologists.

A prologue, entitled "The Greeks foresee the gradual rise of man," discusses those theories of human origins which may be found in classical writings, and particularly in the plays of Æschylus. The discoveries of Mr. Reid Moir are described in some detail, and Prof. Osborn declares himself a believer in Eoliths, after having personally examined the evidence. The Piltdown remains are also fully discussed, though not from any new point of view. A closer approach to the information from which mental characteristics may be inferred is made when dealing with Upper Palæolithic times, but little is said of the intentional burials of the Moustierian epoch. It is supposed that "the cave-dwelling period . . . was of incalculable influence in fostering the spiritual, artistic, and imaginative side of Stone Age life," but there is no evidence that a spelean existence of long duration had any such effect on Neanderthal man. The remarkable artistic relics of Aurignacian times are so mature that it is hardly possible to believe that they were not preceded by cruder attempts which have not yet been discovered. The mental characters of which they bear witness must be supposed to have been essentially associated with racial peculiarities. Interesting accounts are given of the author's visits to some of the more inaccessible mural paintings and etchings preserved in French caves. Though no attempt is made to outline the whole story, the later lectures deal with various topics relating to late prehistoric times, such as the chronological systems of De Geer and Montelius, the Campignian flint industry, the arrival of "our ancestors" in Scandinavia, and the megalithic monuments of Brittany. Equally removed from psychological questions are the speculations concerning the hypothesis that man originated in Central Asia. A plea is made for eugenic control which would ensure the future progress of humanity. This compilation of facts and theories will doubtless be read with pleasure by both the specialist and the general reader, but the present writer must confess to being quite unable to appreciate why the book should be supposed to be concerned chiefly with the higher faculties of man.

β_2.

Man a Machine. In Answer to a Romantical and Unscientific Treatise written by Sig. Eugenio Rignano and entitled "Man not a Machine." By JOSEPH NEEDHAM, *Psyche Miniatures*, No 12 (pp. 111). (London: Kegan Paul, Trench, Trübner & Co., 1927 Price 2s. 6d. net.)

HERE we have presented once more the old problem which has ever puzzled men's brains and of which under the guise of Free Will or Determinism, Maimonides writes, "Know ye that with regard to the discussions of this problem the measure thereof is longer than the earth and broader than the sea." The author is led to present the problem anew from the fact that this century finds us with a growing number of adherents to the view that "mechanism anywhere outside physics and chemistry ought to be abandoned", the immediate stimulus to the work, however, was the appearance of the treatise by Rignano mentioned in the title.

Brief glances at the historical aspect of the question are scattered throughout the volume; they are well chosen and provide delightful reading, but the little-known treatise published by la Mettrie in Leyden in 1748, from which the title of the present volume is taken, is described in more detail with interesting biographical comments on its writer.

The author is evidently an ardent scientist who will allow and encourage mystics and philosophers to dream their dreams and present possible alternative truths, so long as they do not fall into the error of describing them as scientific truths. He presses forward the view, commonly accepted in theory and transgressed too often in practice, that science can deal only with what it can observe and measure or weigh, and that all scientific knowledge must be based on observational methods. He glories in the achievements of science, and provides a number of examples culled from his intimate knowledge of modern biological or biochemical research illustrating the mechanistic theory of life, many of which will be found illuminating to the reader who is without a specialised knowledge of the work which is going forward in those fields.

This little volume may be heartily recommended to anyone who likes to be set thinking.

B. 10.

A Survey of the Social Structure of England and Wales as Illustrated by Statistics. By A. M. CARR-SAUNDERS and D. CARADOG JONES [Pp. xi + 246.] (London: Oxford University Press, 1927. Price 10s. net.)

THIS relatively small volume of 246 pages contains twenty chapters, the first five of which give information concerning the Population of the Country with regard to age, sex, marriage, housing, distribution, classification by industry and classification by occupation. There follow chapters on industrial status and social class, on occupational and other associations, on the national income and national wealth, on education, entrance into occupations, state and voluntary provision against misfortune, compulsory and voluntary transfer from rich to poor, and poverty. Three further chapters deal, in forty-four pages only, with crime, inborn qualities, and the recruitment of the population. On reading of so ambitious a scheme carried out in so small a compass, one can only look for a bare summary of facts, collected in a readily accessible form, which may prove of considerable value as a reference volume to the student in economics, the social worker, the ———, and perhaps a wider public. After examination of the contents it does seem to us that the book has this definite use, though we read in the introduction that it is not primarily intended to fulfil this function.

It is undoubtedly of value to have statistics obtainable from the census, augmented by statistics connected with a view to the investigation of special

problems concerning social welfare published in one volume, and thus far we congratulate the authors and commend their scheme. We venture the opinion, however, that certain sections, notably the chapters on crime, inborn qualities, and recruitment of the population, had better have been omitted or much more fully discussed. Much work has been done on these subjects, and so slender a treatment of them may serve to mislead the uninformed seeker after information: one is tempted to wonder whether the authors are fully aware of the complexity of these problems; have they ever themselves attempted to collect information or analyse it with a view to testing the relative strengths of nature and nurture? Have they studied any of the elaborate researches which have been carried out on that point? Have they read any of the earlier work that has been done on the question of the selective birth-rate, or even the results of correlating the size of family with social status? Do they know of the determination made some years back of the proportion of the population which provides the members constituting the next generation? Have they ever worked out pedigrees of disease or anomaly and grasped the significance from their point of view of the transmission of defect by members of affected stocks who are not themselves possessed of a patent defect but carry it in a latent form? Surely, had they studied these things more intimately they would have taken their opportunity of stressing more forcibly the overwhelming importance of good parentage.

These chapters contain certain tables which are of interest, but when one recalls the many contributing factors one fears lest too much significance shall be attached to simple percentages such as are here provided.

β_{10}.

Common Principles in Psychology and Physiology. By John T. McCurdy. [Pp. xvii + 284.] (Cambridge: at the University Press, 1928. Price 15*s*. net.)

We read on the cover of this volume "This book is an attempt to bridge the gap between body and mind, between structure and function. The author's claim is that the chasm between Psychology and Physiology is an artificial one which has resulted from a materialistic bias in the study of Physiology; but if one assumes that bodily processes follow functional and not physico-chemical laws then it may be shown that these laws are common to both Psychology and Physiology and structure becomes merely a medium of expression for function." Now all will agree that the chasm between Psychology and Physiology is an artificial one, many will quarrel with the view that the error lies in the materialistic bias of the Physiologists.

It is a surprising fact that there has never been an adequate effort to obtain any measure of the association between mental and bodily states, though Darwin's work on the Expression of the Emotion might readily have led to such investigation; the single example we can recall to mind at the moment, though there are doubtless others, is that of Davy, who describes in 1863 how he measured the effect of mental effort on his body temperature.

It is of interest to recall in this connection the sayings of Spinoza on the interdependence of mind and body. "Every manifestation of either is but a manifestation of the other seen under a different aspect. Man's mind is the idea of man's body, the consciousness of bodily states. Now bodily states are the result not only of the body itself but also of all things affecting the body; hence the human mind takes cognizance not only of the human body but also of the external world in so far as it affects the human body. Its capacity for varied perceptions is in proportion to the body's capacity for receiving impressions." Prof. McCurdy, on the other hand, writes: "The principles which I detect and hope to demonstrate as common to mental behaviour of various types and to the physiology of the nervous

system are immaterial agencies. . . . I propose to assume that immaterial agencies, which I call patterns, do guide and in that sense control the physico-chemical processes involved in all living." His patterns "represent properties and relationships, and are pure abstractions which, however, have some kind of existence apart from the material phenomena which they control." Science will assuredly rear its head at such statements in the preface and may perhaps set out to criticise in a hostile spirit; but the book is a delightful volume, full of the most arresting observations and deductions, and he who sets out to criticise will surely be content on a first reading merely to enjoy. If it be necessary to make a decision on the worth of the suggestions and the validity of the arguments more than a single reading will be needful. The author makes an appeal for more recognition and consideration of the part played by the exercise of function in the evolution of specialised structure; this section of the work raises far-reaching questions of philosophy and metaphysics, and is amongst the most interesting parts of the volume.

It is perhaps hardly fair to quarrel with statements made in the last chapter which are admittedly only tentative suggestions on the part of the author in a field which is not his own, but we must express some disagreement with the following: "If a society becomes too homogeneous it dies. Thus the city-state of ancient Greece lost in adaptability as it gained in intellectual and artistic glory; so it died. . . . Specialisation means sterility, so the 'upper' classes are bound to be less fecund than the lower ones. . . . If the group is to be viewed as an organism, the upper classes are the head and the lower the body, so more stupid people function as the generative organs—and the eye cannot say to the hand, I have no need of thee. . . . Ardent reformers would have us dispense with the feeble-minded, not merely with the idiots and imbeciles . . . but also with those who do not attain mental age of more than nine or ten years. Yet the stupid peasant woman is the mother of the race, and always will be. Eliminate her and an over-specialised, moribund race will result. The folly of modern society lies not in allowing stupid people to breed, but in putting into their hands responsibilities for which they are unfit."

Surely these statements are full of error, and we hope that some strong eugenist will convince the author of the fact. All we can do here is to express warm appreciation and indebtedness to Prof. MacCurdy for the interest and pleasure derived from a first reading of his work.

β_{10}.

Histological Technique: A Guide for use in a Laboratory Course in Histology. By B. F. KINGSBURY, Ph.D., M.D., and O. A. JOHANNSEN, Ph.D. [Pp. 142, with 16 figures.] (New York: John Wiley & Sons, 1927. Price 11s.)

An Introduction to the Technique of Section-cutting. From the notes of the late MR. PETER JAMIESON, edited by FRANCES M. BALLANTYNE, M.A. [Pp. 80, with 11 figures.] (Edinburgh: E. & S. Livingstone, 1928. Price 3s.)

NEITHER of these books, as their titles imply, aims at supplanting the larger textbooks of Microscopical Technique. The former is suitable for teachers and research workers who are called upon to prepare microscopical sections, but who do not need the more complicated and specialised techniques required for exact cytological, histological, or embryological research. The book is also to be recommended for the use of senior students. It contains a clear and concise account of the principles of sectioning and staining, together with particulars of most of the better-known methods. The authors are to be congratulated on producing a book which should be useful to many biologists and will supply a need that has been felt by many.

The second book is more elementary and aims at giving a simple account of a standard method of fixing, cutting, and staining sections, as well as brief descriptions of a very few optional fixatives and stains. It contains many useful hints for the beginner. This book can be thoroughly recommended to students and others who are wrestling with the intricacies of microscopical technique for the first time.

F. W. R. B.

Turbines à Vapeur. By Professor G. Belluzzo, translated from Italian to French by J. Chevrier. [Vol. I, pp. 367, with 260 illustrations and 2 charts. Price 60 francs. Vol. II, pp. 596, with 490 illustrations. Price 80 francs.] (Paris: Gauthier-Villars & Cie., 1927.)

The translation of an Italian technical book into French is considerably easier than into English as no numerical alterations have to be made due to different units being used. The present work, from a second edition in Italian, is apparently a third edition in French. It is obvious from this that the work has lost nothing of its value in the course of translation. A translator of a foreign book rarely earns the credit due for the labour involved, as the author's name is always associated with the text. M. Chevrier is to be congratulated on his share of the work.

The first of the two volumes deals chiefly with the theory of steam-flow and opens with a chapter on the properties of steam. This can only be described as disappointing to English engineers, as it ignores the work of Callendar, whose results are based on most careful experiments.

Whilst giving a clear statement of the earlier nozzle tests of Christlein, Briling, etc., the author had probably written the text too early to include much of the investigations of nozzle-flow carried out in this country by the Institution of Mechanical Engineers. As the date of the original text is not given, it is impossible to fix exactly the date of publication, but from certain references it can be inferred that the original is not more than five years old.

One cannot help being struck by the implicit faith the author has in mathematical deductions. This procedure would hardly be acceptable to British students, who prefer a physical explanation of a phenomenon before deriving an equation to represent it.

Throughout the text is a useful series of worked examples explaining the application of the theory. An example of this type would have assisted in the explanation of the question of undercooling of steam, a phenomenon which the author apparently accepts as a fact. Here, he gives a physical explanation, but it is not in agreement with that usually adopted in this country.

The second volume is mainly descriptive and is clearly set out. It covers both complete machines and details, and one can judge easily that the author has a wide experience of actual designing. The reviewer feels that he cannot help taking exception to the statement that the Ljungström turbines are no longer built on account of their high cost. This is not true in England, where this type has been manufactured since 1913, the reviewer having designed some fifty of these sets. The second volume in particular should be of service to designers and students in putting before them the subject from another aspect and showing them the trend of thought among Italian turbine engineers.

B. Lloyd Evans.

Heredity and Human Affairs. By Edward M. East, Professor in Harvard University. [Pp. viii + 324.] (New York and London: Charles Scribner's Sons, 1927. Price 16*s*. net.)

In the second chapter of this book the author states his purpose in writing it, which is to consider "the new orientation that genetics gives to some of the problems of social science, which are, of course, the problems of every-

day life." Prof. East considers that without some knowledge of heredity, it is impossible to decide intelligently questions relating to marriage and divorce, to education, to immigration and to public health, and that a knowledge of genetics is required by lawyer, minister, and physician, and he states that the establishment of the theory of evolution in 1859, though it had no utilitarian application, influenced the entire current of human thought more profoundly than any other scientific pronouncement has done.

The author is a firm believer in the Mendelian theory of inheritance in its latest developments and gives clearly and briefly the main conclusions reached by workers in that field, but he also accepts the work of the biometric school. From the chapters on the machinery and grammar of heredity Prof. East passes to the inheritance of human traits, and to a discussion of the inheritance of acquired characters; he accepts the possibility that the germ-plasm might be modified to a certain extent, but rejects the theory that "adaptations to conditions exhibited in various degrees by all organisms impress the germ-cells in such a manner as to make evolution proceed in the direction of these adaptations"; we may regret that efforts to educate the young will not make the race inherently more teachable, but rejoice that the terrible experiences of man in the past have not left ineradicable marks.

The view taken of the parts played by heredity and environment may be given in the author's own words: "Heredity is the exposed film, environment the developer," and he deals later with the subject in Chapters X and XI, entitled "Some Specific Race Problems" and "Genius, Mediocrity, and Education"; he scorns the idea that the air of a country can make a people great, whatever their natural endowments, and criticises the conclusions reached by Boas on the changes in cephalic indices among descendants of immigrants. He considers that mental characters have been proved to be inherited to the same degree and in the same way as physical characters; environment may determine the group of facts to be studied, the different muscles to be developed, but that is all. "If the infant Newton had been cast among Hottentots, he would have announced no law of motion; but he would certainly have shown himself to be the outstanding thinker of his homeland in some way or other." We are glad to see pointed out the fallacy in the argument that ability cannot be inherited because the absolute number of geniuses from mediocre families is high; it will be high since mediocrity is plentiful, but high grade families will produce, on the average, many more worthy members than mediocre families.

In a chapter on the marriage of near kin the conclusion is reached that the appearance of undesirable characters in the offspring of normal individuals who are of near kin, is due to the presence of those characters in the stock, and that, in general, cousin marriages are undesirable since objectionable recessive traits are common in the race. The author holds that "the fruits of family trees" should be scrupulously examined in direct and collateral lines within four generations and if certain abnormalities are found the projected marriage is inadvisable.

Prof. East regards the problem of racial development and differentiation as fundamentally a genetic problem, and considers that serious students of anthropology must accept that fact; he objects to the theory that mental ability depends on the size of a man's skull, and rejects the idea that in stamping out the degenerate and unfit genius may also disappear; he points out that there is no evidence that the parents of geniuses are more often ill-balanced than the parents of the mediocre or that the genius is inclined to be a neuropath. We do not think that Prof. East considers that when an "intelligence quotient" has been calculated the final word has been said as to the intelligence of an individual, but he points out that children with the highest intelligence quotients are physically normal, in

fact, if anything, are above the average, and that delinquency and anti-social behaviour are practically absent in this group of children. There is a plea for greater freedom in education, and a protest against the time and energy spent on teaching the feeble-minded to read simple words, which "erudition presumably costs about a thousand dollars a letter." We are not sure that Prof. East does not underestimate the advantage that would result from preventing the lowest grade defectives from having offspring; normals may breed defectives, but not in such numbers as defectives breed defectives. We fear that the psychologists and psychiatrists of to-day are not all so permeated with biological conceptions that they can be trusted to lead invariably in the right direction.

The differential birth-rate is considered and some account given of the work done in England, France, and the United States, which has shown that the reproductivity of those who stand out above the herd in economic worth, etc., is low compared with those at the other end of the scale.

In dealing with immigration into the United States, though the arguments, we hold, will apply to any country, it is pointed out that the immigrants of to-day are in too great a proportion the least intelligent of their nations; that except in rare cases, it is the inferior who are forced by economic pressure to go to some other country for a means of livelihood, and that it is a serious matter for any nation to add these people to their number. There is a plea for the study of all so-called reforms from the standpoint of their biological soundness, for the reduction of the survival of the unfit, and the author considers that "the Galtonian idea of race betterment through conscious selection as a sort of religious tenet is the only true conception of the race question."

We can heartily recommend this book, which is written in an interesting and entertaining style; it seems to us one that should appeal to the great number of persons interested in "human affairs" but who are really ignorant of the difficulties to be faced in dealing with social problems, and who, through its pages, might be induced to consider more problems from the biological standpoint. The need of a book of this type which will appeal to the non-scientific reader is very great, and for this reason we venture to think that the first chapter is a mistake; we admit its logical necessity, but fear it may be a stumbling-block in the way of many who would be interested in the remainder of the book, and for whom its lessons would be most salutary.

β_4.

The Rate of Living. By RAYMOND PEARL. [Pp. 185.] (London: University of London Press, 1928. Price 10*s*. 6*d*. net.)

IN this book Prof. Pearl brings together in a concise form the results of a series of experiments, carried out in the Institute of Biological Research of the Johns Hopkins University, in regard to the duration of life of the fly *Drosophila melanogaster* in its wild and vestigial forms and of the plant *Cucumis melo*. Life-tables are constructed for fly populations maintained in standard bottles under uniform conditions, and it is shown that, from a zero point which is the moment of emergence from the pupal stage, the life-curves obtained for the normal wild type resemble very closely those of human beings starting from the age of minimal mortality rate as zero point when the scales of time are made to correspond. For the vestigial type, the curves are quite different in form and the duration of life is also less than half that of the wild type. Under certain densities of population, however, the form of curve for the wild type approaches to that of the vestigial, and the effect of density of population on the life duration is studied from the results of a series of parallel experiments at different densities.

The author then develops the conception of "inherent vitality," which

he defines as " the total capacity of an organism to perform vital actions in the complete absence of matter or energy of exogenous derivation." When the flies of normal wild type are kept under conditions of complete starvation, the form of the life-curve on a relative scale is found to remain unchanged, although the mean duration of life is of course decreased. Moreover, under these conditions differences of density produce no effect in either type and the life-curves of the two types become substantially identical in form and duration. From this Prof. Pearl concludes that the two types have approximately the same inherent vitality, but under food conditions which are adequate for the wild type flies the potential capacity of the vestigials in inherent vitality is only able to come to less than half its complete expression.

From experiments on the growth of canteloup seedlings under " starvation " conditions, the further result is derived that rate of growth and duration of life are negatively correlated, and from this the general conclusion is drawn that the duration of life varies inversely as the rate of energy expenditure during its continuance, or " in short, the length of life depends inversely on the rate of living."

The experimental work on which Prof. Pearl's book is founded is undoubtedly of a high order both as regards the planning of the experiments and the technique, and the clarity with which the results are presented is also beyond criticism. But it is in the combination of these very qualities, description of an experimental technique which inspires confidence and an unusual lucidity of explanation, a combination which characterises most of Prof Pearl's writings, that a real danger lurks, for the reader finds himself carried forward to conclusions without perceiving that between the experiments and the conclusions he has traversed unnoticed a region of conjecture which very often will not bear critical examination. That is so in this book in regard to two of the author's most outstanding conclusions, which are already being spoken of in some quarters as established facts.

The first of these is the conception that there exists an *optimal density* of population for each type of organism under given conditions. We are becoming accustomed to discussions on what constitutes the optimal density for human populations, but in this case it is generally the optimum of social well-being rather than of life duration which is meant. But the evidence for the existence of an optimal population as regards life duration is extremely slender. It needs no experimental work to elicit the fact that when the density of any population of living things whatever passes a certain point their average duration of life must decrease. Neither would it be surprising if in an organism such as the fly, in which reproductive processes form a large part of normal life, the compulsory cessation of such processes by isolation of single individuals should also decrease the mean duration of life. Whether we were to attribute this to a direct physiological effect or to mere boredom would matter little, but it is perhaps pertinent to remark that the English Life Tables for 1921 show that at all ages widowed women have a considerably smaller expectation of life than married women, a phenomenon which seems too pronounced to be explicable by selection. At any rate, in any experiment on flies which involves some segregation of the sexes, a shortening of average life duration may well be expected, and if one fly, or two or three of the same sex, are kept in a bottle, it is not remarkable if their life duration is shorter than at higher densities where both sexes are present. A little calculation shows that this may be the sole explanation of the initial rise in the life-duration curve with increasing density of population (Fig. 12). In the experiments " 13,000 flies were divided among 530 one-ounce vials in 20 different densities, running as follows: 2, 4, 6, 8, 10, 12, 15, 20, 25, 35, 45, 55, 65, 75, 85, 95, 105, 125, 150, 200 flies per bottle. In each bottle was

put at the start an equal number of male and female flies . . ." (p. 49), and the results are shown in Table III. At the initial density of 2 the mean duration of life was 27·31 ± ·58 days with a standard deviation of 14·47 days; at initial density of 4 it was 29·32 ± ·60, at 6 in was 34·45 ± ·65, and at 8 34·20 ± ·61 days, and from that point onwards the durations at 10, 12, 15, 20, 25, 35, and 45 were 36·22, 34·31, 37·92, 37·07, 37·47, 39·43, and 37·46 respectively, with probable errors about ·6. At an initial density of 55 the mean duration was 40.04 ± ·53 and then progressively declined. These figures form the basis for the conception of an "optimal density."

Now it can be shown that if for simplicity we assume normal distributions of life duration in flies (though in reality the distributions are rather skew) and a random selection of pairs, the mean difference between the life durations of pairs will be $2\sigma/\sqrt{\pi}$ days; hence since the standard deviation for single pairs is 14·47 days, one fly out of a pair will survive the other by 16·3 days on the average. Let us suppose that when left to survive alone the duration of life remaining is shortened to p times its value normally at that age. Then if x_2 and x_1 be the life durations of the individuals in the pair, x_2 being the greater, the mean total life duration of a group of N pairs in the vials with initial density 2 would be $\frac{1}{2}N[\Sigma(2x_1)+\Sigma(x_2-x_1)]=27{\cdot}3$ days, but in the presence of other flies at ordinary densities it would be $\frac{1}{2}N\left[\Sigma(2x_1)+\Sigma\left(\frac{x_2-x_1}{p}\right)\right]=37{\cdot}5$ (using the mean value for densities 10 to 55). Subtracting and substituting $\Sigma(x_2-x_1)/N = 16{\cdot}3$ this gives $p = {\cdot}44$, which is not an unreasonable value. At initial densities 4, 6, 8 the expected mean durations on this basis would rapidly approach the normal value 37·5 as the observed figures actually do, and it must therefore be concluded that the whole conception of optimal density probably rests on the question whether a fly living by itself would be expected to have its duration of life halved or not. There may be experimental data bearing on this point, but it is not mentioned in the book, and in any case it would be wise to cease talking of optimal densities as applied to life duration until the evidence for their existence rests upon a more secure foundation.

The second outstanding conclusion which has attracted a good deal of attention and on which Prof. Pearl lays a good deal of emphasis is that (p. 151) "*the length of life depends inversely on the rate of living,*" or as elsewhere stated (p. 145), "the duration of life varies inversely as the rate of energy expenditure during life." The evidence for this conclusion which is given in the book is a series of experiments in which canteloup seeds, from a single melon and of uniform size, were allowed to grow in agar tubes in darkness and without any source of nutriment other than air and moisture, and the growth of the resulting shoots was measured at intervals. After growth had ceased, the period of growth being G days, there followed a stationary period until the first signs of death appeared, and the total time, A days, up to this point was recorded. The mean value of G was 14·13 ± ·22 and of A 27·39 ± ·22. The rate of growth was measured by the constant b of the logistic growth curve $y = k/(1 + e^{a+bx})$, x being the time, and k, a, b constants.

The correlation coefficient between growth rate b and total life duration A was found to be $r_{bA} = -{\cdot}463 \pm {\cdot}078$, which was not sensibly altered if the original seed weight or total stem length were made constant. In a later series of experiments a higher negative correlation was found ($-{\cdot}643 \pm {\cdot}057$). From this it is concluded that "the *negative* correlation between growth rate and duration of life shows that the faster the seedling grows (that is the higher the rate of energy expenditure in growing) the shorter its duration of life, and *vice versa.*" The duration A is the sum of the growing period (G) and the stationary period (A−G). For constant stem length the growth rate b must necessarily be negatively correlated with the growth period G, as

Prof. Pearl indeed points out ($r_{G b} = -\cdot 362 \pm \cdot 086$). It is not stated, however, that the correlation between the growth rate b and the duration of life after completion of growth (A − G) is zero; thus the data in Table XIV actually give a value $r = -\cdot 0002$. The conclusions to be drawn from these experiments would therefore be more correctly stated as follows: (i) the duration of growing life under starvation conditions depends inversely on the rate of growing; (ii) the duration of life after growth ceases under starvation conditions does not depend on the rate of growing. Since the expenditure of energy by the plants in the stationary period A–G under the conditions of the experiment would be practically negligible we only learn from this that seeds of the same size when cut off from external sources of nutriment other than air and water have a certain amount of energy to expend in changing into mature plants, and the faster they expend it the sooner it is expended.

The same might be said of a number of fireworks containing the same quantity of explosive, and there is a very considerable gulf between this and the general conclusion that in the presence of ordinary nutriment the length of life depends inversely on the "rate of living." The only bridge to this gulf seems to be the conclusions of Chapter XI of "Studies in Human Biology," quoted on p. 140, drawn from the relation between death-rates at ages over 45 of English males in occupations when graded according to the estimated expenditure of physical energy they involve, conclusions which are very far from convincing owing to the impossibility of allowing for the differential incidence of extraneous *disease* as distinct from organic exhaustion.

Much more work on this subject will be required before Prof. Pearl's general conclusions can be taken as established, and more caution will be advisable in jumping the gaps from one form of life to another, by taking into consideration such factors as the zero points from which life duration is being measured. Thus in this book the life duration of Drosophila is measured from its emergence as a fully fledged fly from the pupal stage, that of the canteloup from the planting of the seed, and that of human beings from the point of minimal mortality, age 12. But what justification have we for this other than the fact that the life curves give the best fit to each other when superimposed by taking these zero points?

Having made these necessary reservations the book may be recommended as being both fascinating to read and also full of suggestion for further investigations.

β_1.

The Electrical Conductivity of the Atmosphere and its Causes. By Victor F. Hess, Ph.D., translated from the German by L. W. Codd, M.A. [Pp. xviii + 204, with 15 illustrations.] (London: Constable & Co., 1928. Price 12*s.* net.)

In geophysical subjects, like the present, which are on the border-line between physics and meteorology, it is always difficult to obtain reliable data; the phenomena are complicated by the simultaneous action of so many different effects, none of which is under direct control. There is more room for honest difference of opinion than is allowable in laboratory Physics, where all the phenomena may be controlled by the experimenter. It is in subjects such as these that a book, which gives an ordered account of the work which has been done, is especially welcome.

The present book is such an account, and is mainly occupied with a description of the methods employed and the results obtained in the measurement of the ionisation of the atmosphere as observed just above the earth's surface. Measurements of the conductivity of the air, of its ion content, and of the mobility of the ions are described at length, the question of the cause of this ionisation is discussed, and the methods of measuring the radioactive content of the earth and air are indicated. This part of the book

remains as it was written in the German edition of 1926, and contains a useful set of references on the subject up to the end of 1925.

To most physicists the next section will be of most interest. It deals with the penetrating radiation whose existence was first suggested by the author in 1912. He here gives a detailed account of the experiments which led to this suggestion, and of the more recent investigations which have borne it out. The question of the nomenclature of these radiations is raised. It seems time that some definite name was given to them. The name "Millikan Rays" is misleading in view of the fact that they were known to exist long before Millikan started his experiments; the names "cosmic rays" and "ultra-gamma rays" suggest more than is definitely known at the present time about the radiations. In the present book they are known as "Penetrating Radiation" and as *Höhenstrahlen*, although the more objectionable term "ultra-gamma rays" is frequently employed. It is unfortunate that the useful and comprehensive term "X-rays" has already been appropriated; here is an ideal case for its application.

The section on the penetrating radiation has been rewritten for the English edition, and contains references up to the end of 1926. In the discussion of the properties of these rays the work of German investigators is heavily stressed, it would seem, at times, to the neglect of that of the Americans.

The subject of the ionisation of the upper atmosphere (the Heaviside Layer) is next dealt with. This section, also, has been rewritten for the English edition. Even so, the subject is now such an immense one that the twenty-three pages devoted to it at the end of the book cannot do more than touch on the fringe of the subject. It was necessary to mention it for the sake of completeness, but the account hardly does justice to what was known even in 1926. The interesting deduction is made, contrary to what is assumed by recent writers, that the penetrating radiation is responsible for an appreciable amount of ionisation in the upper atmosphere.

The translation is good and makes easy reading, although the subject-matter does not lend itself to elegancies of style. We only noticed one Germanised expression, on page 44, where the expression "independent *from*" is used.

J. A. R.

(1) **Emergent Evolution and the Social.** By WILLIAM MORTON WHEELER, Ph.D., Sc.D. (Pp. 80.)

(2) **The Standardisation of Error.** By VILHJALMUR STEFANSSON, M.A., LL.D. (Pp. 110.)

(3) **On History, A Study of Present Tendencies.** By A. L. ROWSE. (Pp. 103.)

[Psyche Miniatures. London: Kegan Paul, Trench, Trübner & Co. 1928. Price 2*s*. 6*d*. each.]

(1) The *Psyche Series* continues to issue its frequent little volumes, and they have undoubted fascination in size, price, and clearness of type; they suggest holiday rovings with a book in knapsack or pocket, not requiring any exertion, physically or mentally, and which we can leave behind us where we like, or present to a fellow-wanderer. Like all booklets issued in series, some are worth preserving and re-reading, others we are only too glad to have beguiled an idle hour with, for they have left us with a new thought or two, and the remainder we are but too ready to leave behind or pass on to another. Prof. Wheeler's booklet, we venture to think, belongs to the last class. When we first read the major portion of it in *Science*, we had no idea that it would be considered worthy not only of a single but a double reprint. The keynote to the book is more or less a protest against such sciences as biochemistry and biophysics, at present largely in evidence. It is a protest against the

"mechanical" explanation of vital phenomena. But the author gives us no definitions of wha the understands by "explanation," or by the word "mechanical." Science never has explained, and never will explain, anything; it simply describes in the briefest formulæ possible the "how" of things, not their "why." Its function is to find the most comprehensive formulæ it can for brief description, and not to "explain why A causes B." Again, "mechanism" is not something existing in the "Dinge an sich"; it is the mental model created by the scientist as a basis for his descriptive formulæ. In other words, it has a purely psychical and not a material existence. The electron is no more "real" than the atom of the mid-Victorians, nor has the curvature of space any more of "reality" than the æther of Lord Kelvin.[1] They are but steps towards more and more comprehensive descriptions of our conceptual universe, figments of the human brain, and that is why it is idle to discuss whether the "real" universe is continuous or discontinuous; either hypothesis, with the swing of the pendulum, may give for the time a better descriptive "mechanism." Once this true understanding of the function of science has been reached there remains very little of any supposed slavery of the biological to the physical sciences. The only difficulty arises from the biologist too often neglecting to measure and weigh, or being too apt to predict that what he has observed in a single individual organism, or in an absurdly small sample of such organisms, holds in general for all individuals of the species or even for individuals of other species as well. Prof. Wheeler tells us that we are never able to predict, from a knowledge of the properties of a unit, what will happen when we take a group of units. In the latter case the unknown and unexpected will arise, and this idea is apparently to be termed "emergent evolution," and gives, with his definition, "freedom," i.e. "action in accordance with those characteristics which subsist at a certain level of organisation." Thus, according to our Professor, it is never possible in one "level" of evolution to predict what will "emerge" in the next "level." Were such a statement absolutely true any scientific prediction involving *time* would appear to be impossible. Scientists know the danger of extrapolation, but they are scarcely prepared to give up time as one of their factors, and even with such an antiquated law as that of gravitation they could give a not wholly inadequate account of what would take place if a second Jupiter came with given velocity at a given epoch into our planetary system. Science does study not only the characters of units, but also the behaviour of units in combination, and the latter is not *wholly* independent of the former. The chemist, knowing the characters of individual elements, is not entirely unprepared to predict what will happen in combinations. The fact that in the *present* stage of science we are largely ignorant of what will happen in new combinations—or, as our author terms them, "new emergents"—by no means proves the *impossibility* of predicting new emergents or their "freedom"; it merely indicates the inefficiency of our mechanical models of the universe. The adequate model would indeed describe the order of successive "emergents." The proclamation of the inadequacy of "mechanism" generally to describe life and behaviour is premature. Frankly we do not know at present the whole of what even existing models can or cannot accomplish in this direction, still less what conceptual "mechanism" invented by the mind of man in the near or far future may achieve. The words "impossible to know" are mere dogmatism in science. Our author, in conclusion, cites the following words of Jennings with approval: "The doctrine of emergent evolution makes the biologist loyal to experimentation and observation in his own field of work, whatever is found in other fields. Courage and defiance sprout from his soul in place of timorous subservience to the inorganic. No longer can the biologist be bullied into

[1] "I am as certain of the existence of the æther as of that of the air," he once remarked.

suppressing observed results because they are not discovered nor expected from work on the non-living parts of nature. No longer will he feel a sense of criminality in speaking of relations that are obvious in the living, for the reason that they are not seen in the non-living. Biology becomes a science in its own right—not through rejection of the experimental method but through undeviating allegiance to it. The doctrine of emergent evolution is the Declaration of Independence for biological science." We must confess our breath is taken away! What biologists have been bullied into suppressing the results they have observed? Most biologists of our acquaintance, past and present, Huxley and Bateson, Pearl and Macbride, were, or are, very good fighters, indeed somewhat truculent men, whom to bully would take more courage and defiance than could possibly sprout from our own poor timorous soul! Who are the biologists who feel a "sense of criminality" in speaking of obvious relations among the living? Is the paragraph cited mere rodomontade, or an excuse for neglecting instead of reconciling results reached in fields adjacent to the biological? Is this new "Declaration of Independence" any more valid than numerous earlier Declarations, or is it an appeal to admit obscurantism under a doctrine of "emergent evolution," of which the foundation, according to Prof. Wheeler, appears to be the assertion that we cannot and never shall be able to assert what will emerge. *Ignoramus* is the life of science, *Ignorabimus* its death sentence. We wish the biologists joy of their newly won freedom. β_1.

(2) It is very difficult, the reviewer finds, to determine whether this booklet is an elaborate skit on human nature or is intended as a valid demonstration that there is no such thing as truth. If the former, our idiosyncrasy is such that we fail to find the skit amusing; if the latter, we hold the demonstration to be inadequate. For example, the ostrich of literature since Pliny's time has buried its head in the sand when pursued; this is not confirmed by sportsmen and zoologists. Well, the illustration is so valuable to literature and to our vocabulary that it is desirable to define the ostrich as a bird that buries its head when frightened. Or again, that the Eskimos of Alaska live in houses built of snow, or that Irishmen sleep with a pig under the bed, or that it is colder the nearer you go polewards in the summer, are matters accepted as truths. Well, define all these things to be true, and the world will get on better; the stability of nations and the morality of children depend on their acceptance of the untrue as true.

"Though we have not covered the field exhaustively, we may agree that truth is good, usually in engineering, chemistry, physics, and the allied sciences. It is good in astronomy and geology, whenever it does not conflict with religion. It is good about as often as bad in sociology, psychology, physiology, biology, and several of the related sciences; for in these it appears to support present-day manners, current morals, and the prevalent religion about half the time. In history, civics, and many such fields it is always open to the gravest suspicion. In the training of very young children it should be rather carefully avoided as a general thing" (pp. 103–4).

We may be told this is satire; if so, our individual palate prefers the omelet with the *fines herbes* not omitted.

(3) This work, if written in a very different spirit and with a very different appreciation of the nature of life's problems from those of Mr. Stefansson, is yet concerned with the relativity of truth, in this case of historical truth. Mr. Rowse realises that each age has its own historians, men who, because they belong to that age, and because they know the needs of their contemporaries, interpret history relatively to their time. It may be supposed that Thomas Carlyle and Mr. Froude satisfied the needs of the Victorian generation, however distasteful they appear to the more scholarly readers of to-day. Doubtless Hume was preferred to Keith by men of the eighteenth

century. Perhaps the pendulum will swing again, and by the middle of the century the works of Wells, Cole and Tawney grow dusty on the shelf for their lack of literary vivacity. The folk of each age—I mean those who purchase and read histories—obtain what they need from its historians and biographers, even though those writers themselves imagine they have sought, found, and told the pure truth. "The attempt to get outside of historical movements and to survey them objectively is ultimately hopeless; as Prof. Black writes, after all one can't get out of one's own skin."

The very form of words the historian adopts gives the dry bones of the records a colouring, and after all the life he breathes into his lay figures—for no records provide the psychology of the *dramatis personæ*—forms ultimately the essence of his history, and makes it a work of art. It is indeed easier to write a drama than a real history. Take Joan of Arc or Mary Stewart as centre figure for a drama, and the poet can bend the action to his psychological conception. He can leave out the ugly, nasty, non-accordant little facts. But the historian of to-day cannot do this; he has to twist, to bend, and to develop his psychological conception, if he is honest, in accordance with *all* the facts; and the psychology of men and women who lived centuries before us is not the psychology of men and women of our own time, upon which experience we base our conception of individual character. We are apt to think that the dominant passions and desires of mankind have remained the same through all the ages. The thought is false, and a little study of anthropology soon makes it evanescent. Palæolithic man must have thought and behaved on wholly different impulses from modern man, and so it is with the uncivilised races of to-day; it requires no little patient observation to lay bare their manner of thought, the springs of their action; it is nearly impossible for a cultured man of to-day to place himself wholly *en rapport* with a Catholic peasant woman of Southern Germany, or with a Salvation lass who urges the populace in strident tones at the street corner to come and be washed pure like she has been in the blood of Jesus. It means much to her, but can the reader grasp the mind to which it seems reality? We must remember that as we go back in history we have passed from an age in which these manifestations are occasional atavisms to ages in which they represented the psychical feeling of almost the entire mass of the population. It seems the hardest of all tasks to place oneself in the psychical atmosphere of a past age, and it is this fundamental difficulty of the historian which enforces the relativity of history. The facts, the mere facts, have in history, as in a murder trial, no life in them. It is the quick humanity behind the facts which appeals to our imagination, and the psychology of that quick humanity which gives history its absorbing interest and fascination for our minds. Every age recasts its history, and, as Mr. Rowse writes, "the immense work of reinterpretation of the past is likely to occupy the lifetime of the youngest of this generation." The historian has an exceedingly difficult task before him, namely, to steer between the Charybdis of merely stating a multitude of facts and the Scylla of converting history into a drama dictated by the psychology of his own age and person. We have to maintain our course between historical learning and historical dramatisation, and to write history needs not only research, but understanding of how the psychical outlook has changed with the ages.

Mr. Rowse has written a suggestive booklet, and we shall not cast it out from our wallet. β_1.

Introduction to the History of Science. Vol. I: **From Homer to Omar Khayyam.** By George Sarton. [Pp. xi + 840.] (Washington, U.S.A., the Carnegie Institute; London: Baillière, Tindall & Cox, 1928. Price 45*s*. net.)

We have had occasion to draw attention in previous review notices to the revolution in attitude towards the philosophical and historical aspects of

modern scientific studies. Science is taking her place among the humanities, and the issue of books dealing with the various aspects and phases of the history of science and of the philosophical attitudes and theories of her exponents through the ages continues with astonishing vigour. Fresh avenues of historical research have been opened up as a consequence, but there are many evidences of gaps and of incomplete records which are bringing to the fore more and more enthusiastic workers.

The results are fully justifying the efforts. What has, however, particularly manifested itself in this connection is the difficulty of ascertaining and of getting access to the many sources for critical inquiry. There are two broad aspects to this need. There is clearly the question of guidance for the research worker, and there is equally clearly the need for the provision of authoritative material for the interpretation of the work of the experts for the benefit of the general public. So far as the former of these aspects is concerned, we are bound to notice with admiration and pleasure the painstaking and arduous but invaluable work of Mrs. Dorothea Singer in the compiling of records of original manuscripts which she is undertaking so successfully. So far as the latter is concerned, the collective efforts of our research students have not yet made feasible the writing of a comprehensive and reasonably complete history of science. Gradually, however, the materials for such a large venture are coming into being, and it is to be hoped that it will be instituted in due course; though little more than a preliminary survey of a somewhat sketchy character can be hoped for for many years to come.

It is in the joint provision of authoritative material both for the research worker and for the semi-popular interpreter that the whole world of scholarship is laid under a deep debt of gratitude to Dr. George Sarton, who is devoting his whole life to the conclusion of a gigantic venture whose first-fruits are seen in the formidable volume now before us.

No one is better qualified for the task. Probably the most supreme student of the history of science in all its phases in the world to-day, his work of preparation has been patiently and persistently carried on under circumstances that, while happily smooth in the recent years of his residence in America, were at least difficult and at times almost impossible during the period of the late war. We read with a real sense of the dramatic of how, before abandoning his home in Wondelgem, near Ghent, he buried his notes in the garden, where they lay for four years before he was able successfully to recover them. Vicissitude and hardship fortunately in no way damped his ardour, and now we have this large volume of some 840 pages, the precursor of some additional six volumes or so yet to come.

What is its object? As the author himself describes it, it is "to explain briefly, yet as completely as possible, the development of one essential phase of human civilisation which has not yet received sufficient attention—the development of science, that is, *systematised positive knowledge*." This, he holds, is equally important with such other aspects of human progress as the development of religion, of art, and of social justice. The claim is pertinent, and the corollary is clear that no history of civilisation can be complete without its due record.

At the same time Dr. Sarton naturally recognises that the record of progress in science is no water-tight, self-contained compartment in the house of progress. All phases of human development are interlocked and interrelated. The links are real and important in their mutual reactions. Particularly is this so in the history of religion. As Dr. Sarton reminds us, until relatively modern times theology was an intrinsic part of science, and in medieval times in particular this was intensively true, and it is therefore impossible, for example, to "reach a correct understanding of Muslim Science if we do not fully grasp its gravitation around the Qur'ān."

The whole scope of this great inquiry impresses one with the level-headedness of its author. Nothing could be easier than for an enthusiast to run away with his subject and to suffer from false values and a wrong sense of proportion. The sane judgment and the proper maintenance of balance on the part of the author may be illustrated by his attitude towards what are frequently referred to as the pseudo-sciences of the early and middle ages—astrology, alchemy, and the like. Because they belong to the history of error, to the story of regression rather than of progress, they are kept in the background of Dr. Sarton's fabric; but because they played their very active part in their time, they call for and receive more than a passing notice.

The author describes his venture as a purely chronological survey in the form of "cross-sections of civilisation" for each half-century. His ambition is to see it as the preliminary to a further series of volumes surveying the different types of civilisations—pre-Hellenic, Greek, Roman, Semitic, Chinese, etc., to be followed in its turn by a third series intended to survey the evolution of the special sciences. The conception, whose consummation is admittedly beyond the powers of one man, is as grand as it is gigantic. It at least has in its favour the evidence of a good start in the volume before us.

Volume I, then, of this *Introduction to the History of Science* carries us in fifty-year stages from the time of Homer to the time of Omar Khayyám. It gives us, again to quote the author, "a fresco of intellectual progress during the course of two millenniums." It gives us "a history of mathematics from Thales to Omar, a history of theoretical music from Terpander to Guido of Arezzo, a history of astronomy from Philolaus to al-Zargati, a history of geography from Hecatæos to al-Biruni, a history of exploration from the time of the Phœnician navigators to that of the Scandinavian, a history of medicine from Alcmæon to Ibn Sina—in brief, a vast intellectual panorama extending from the *Iliad* to the *Chanson de Roland*."

The plan of a rigid limitation of chapters to a sequence of exact fifty-year chronicles strikes one at first as arbitrary, artificial, and unnecessary. The author's sense of perspective, however, is clear. His defence of the system is sound, and, to put it vulgarly, the proof of the pudding is in the eating. His first volume does tend to justify the method. As he very pertinently reminds us, "the centuries lasted just as long in those times as they do now, and each generation brought in sundry changes of its own." He rightly protests against the prevalence of the vague inadequacies of many writers who tell us of two men that "they flourished in the tenth century." The apparent link between them too often finds no foundation in fact, and makes them no more contemporaneous than were, say, Clerk-Maxwell and André Ampère.

The basis of Dr. Sarton's half-century unit is that this is approximately the span of man's intellectual life. Admittedly artificial, it serves its purpose well. Further, for purely mnemonic purposes, each chapter is entitled after the most representative man of the particular half-century in question, so that we have, *e.g.* Chapter III—The Time of Thales and Pythagoras (the first three chapters are exceptions in dealing with a whole century); Chapter XI—The Time of Lucretius; and Chapter XXVIII—The Time of Jabir Ibn Haiyan; and so on. Each chapter opens with a general survey of the science of the period, and follows with the men involved in the religious background, the cultural background, and the various aspects of scientific activity, concluding with the historiography and philology of the period. With each man quoted is given a very brief biography and a bibliography of texts and authoritative criticisms.

The book does not read like a novel, nor is it intended so to do. It is impressive nevertheless. No less than fifty-three pages are devoted to its very comprehensive index. Clearly, here is no work for the general reader,

unless we except the masterly introduction and the opening sections of each chapter. But for the student of science, for the student of civilisation, and for the research worker the publication of this book is an event of major importance. We can conceive of no library as being complete without its copy of Sarton. By the sheer weight of merit and authority it becomes the most valuable work of reference on the history of science yet in existence for the period with which it deals, and the world of scholarship and letters will await the next volume of the series with eagerness and with, may we add, a feeling of profound admiration and gratitude to the author.

I. B. H.

Metanthropos: or, The Body of the Future. By Ronald Campbell Macfie, M.A., LL.D., M.B.C.M. [Pp. 96.] (London: Kegan Paul, Trench Trübner & Co.; New York: E. P. Dutton & Co., 1928. Price 2*s.* 6*d.* net.)

An excellent addition to the series of little books being produced by Kegan Paul, Trench & Trübner. Dr. Macfie is the author of many suggestive works, besides being a very distinguished poet. Nevertheless, the trade of a prophet is a dangerous one, because we seldom hear of the prophets whose sayings have not come true. Those who succeed are great men, but those who do not do so were sometimes stoned. Probably by the time that the human race acquires the bicycle wheels, both the author and the reviewer will be entirely forgotten. Whether we have improved much since the days of the ancient Greeks and Romans we would be sorry to have to decide. Certainly the modern Briton thinks that he is a paragon; but in certain papers of mine I have had to complain strongly of his intellectual output in connection with malaria (see *Practitioner* and *New Health* for September 1928).

P. R.

Maps. Their History, Characteristics, and Uses. By Sir Herbert George Fordham. Second Edition. [Pp. x + 83.] (Cambridge: at the University Press, 1927. Price 6*s.* net.)

This book is designed to interest teachers of geography in cartography so that maps may be appreciated correctly. Such a task is by no means simple. The modern map, with its cold precision, is, by reason of its conventional appearance, liable to repel interest. The familiarity of students with maps often leads to the same result.

The author realised that most pupils are attracted at an early stage by maps, and planned the book to show that the process of becoming familiar should stimulate and not deaden that keenness. "La carte est le document de base indispensable pour la Géographie," wrote Vallaux. The author accepts this, yet enlarges our conceptions so that a map becomes an evolved result with a history and "as a book from which to learn—not a final object of study in itself."

In its small compass the reader will find the subject so treated that enthusiasm is aroused. The sections on historical progress and art in cartography may be cited as examples in this respect.

The work is well prepared and illustrated, whilst the appendix and index enhance its usefulness. This second edition, which removes a few trifling blemishes which were in the first, brings the work up to date. It contains not only much useful information, but also a great deal of good practical advice drawn from the author's knowledge of this subject and of teaching.

Many teachers are realising the value of panoramic sketches (p. 35), and it is to be hoped that the paragraph (p. 51) dealing with the importance of geological maps will receive the attention it deserves from geographical publishers.

We recommend this unique book to all teachers and suggest that it is a work of wide value, calculated to improve the general education and remove an ignorance under which no one should labour.

J. Eling Colelough.

Some Questions of Musical Theory. Chapters I to IV, in two vols. By W. Perrett, B.A., Ph.D. [Pp., Vol. I, 30, Vol. II, 92.] (Cambridge: W. Heffer & Sons. Price 7s. 6d. and 5s. respectively.)

Chapter I.—How Olympus found his New Scale.
Chapter II.—The Olympion.
Chapter III.—The Second String.
Chapter IV.—Ptolemy's Tetrachords.

Chapters I and II of Dr. Perrett's work on the music of the Greeks were published two years ago, and, while admiring his ingenious reasoning, the critics are almost unanimous in refusing to commit themselves as to the plausibility of the author's conclusions. The subject is very conjectural; even the enlightened few are not agreed upon it. To a mere physicist—and Dr. Perrett despises the gadgets of the physicist, although he has got one of them to provide him with a set of tuning-forks of frequency guaranteed to 1 in 1,000—the questions discussed in these books seem incapable of solution. It is a question of selecting the most plausible conjecture, and Dr. Perrett has put up a good case for his own. The book is a mine of selections from ancient and modern authors which will interest those who have studied the evolution of the scale, and may intrigue those who delight in geometric series, but is bound to remain "caviare to the general."

The musician will be bewildered by the many small graduations of pitch which have been, or may be, used to augment or diminish any note in the scale, apart from the varying positions of the semitone in the tetrachord. The adoption of the proposed two additional notes in the octave would entail the expense of reconstructing keyed instruments, just as expense forbids the imposition of a standard pitch. What scale has been evolved after all our adjustments of modes and scales by vibrations and by logarithmic cents, and after the final victory of the equal temperament? The present scale of the concert-room seems to be a compromise of various instrumental and singing scales determined by the manufacturer. Since the time of the great god Pan shaping his reeds by the river, the scale is dominated by the maker of wind instruments.

The author (as also Lord Balfour) claims for the Greeks a super-sensitiveness which compelled them to sub-hear an inevitable harmony which we moderns demand shall be grossly presented to our coarser ears by accompanying instruments. In the Greek theatre the melody of a few flutes and the arpeggios of the lyre furnished a monody which was negligibly subservient to the majestic presence of the gods, with their exaggerated height, their masked megaphonic voices, and the tremendous lines of an Æschylus. Compare (as harmony) such an accompaniment with the surging orchestra of Wagner, weaving a polyphonic tissue of ten or twenty iridescent threads, while the gods on the stage declaim the trombone part in a foreign language.

The author decries the equal temperament because it allows the composer to modulate restlessly from one key to another, stopping nowhere. This is suitable to an imperfect and emotional world, where the only restful music is that which is never composed and never performed; though Wagner gives an example of calm unmodulated music in the 136 bars of the chord of E flat in his *Rheingold*.

The book is a mine of selections from the works of all writers who treat of music since the remotest times, and is a monument of the sound scholarship

and industry of its author; it will be of the greatest interest to all who study the origin and development of music. E. G. R.

From Magic to Science. By CHARLES SINGER. [Pp. xx + 253.] (London: Ernest Benn, 1928. Price 25s. net.)

FEW men have rendered such distinguished service to the study of the history of science as has the author of this handsome book. Not only is he distinguished as an historian of medicine and biology, but he has rightly come to be regarded as one of our foremost medievalists. This is indeed a much more difficult achievement than it sounds. The medieval mind has always been a baffling puzzle and a mystery to the modern man, chiefly because it concerns a period in civilisation that had inherited a decline in learning. To the collapse of scholarship from classical times there had been added a vast superstructure of magic and superstition, and the task of reconstruction which brought about the Renaissance of science, following those of arts and letters, was thereby made many times more difficult and complicated.

Properly to appreciate the medieval mind in fact calls for an effort of inherent difficulty on the part of the modern student—he must try to forget, or rather to submerge for the time being, his modern knowledge, and must try to enter for himself into the perspective and into the normal frame of mind and outlook of a human being of those days. He must also be intimate with the legacy of classical learning that had been lost with the Dark Ages. At the same time he must be a man of science. Unfortunately most of our medieval scholars are lacking in this last respect, and as a result are unable to give "adequate attention to the development of rational conceptions of the material world."

It is fortunate that in the author of this work now before us we have a scholar who fulfils brilliantly all the necessary qualifications for a study of medievalism. *From Magic to Science* constitutes a modified reprint of various essays that have been contributed by him at one time and another in the past, many of which have been unfortunately out of print or alternatively difficult of access for some time. They present, in the form and order of their present arrangement, a vivid and scholarly picture of the degradation of thought from the " intellectual efficiency of classical antiquity, and of the earlier steps in the weaning of Western Europe from this decline, and of the setting of mankind once more upon the road of progress towards modern science. To quote the author, " Here some sort of attempt is made to trace the collapse of ancient science into the swamp of magic and the first attempts at recovery from that hideous slough." Beginning, then, with "Science under the Roman Empire," we are shown the first ste s in the decline in the rejection of that true spirit of inquiry for its own sake that characterised the great leaders of Greek thought, and the narrowing down of the fruits of their labours to the more practical and utilitarian needs of the community. The second essay, " The Dark Ages and the Dawn of Science," carries the story further, and describes the destructive forces that finally broke up all organised thought in Western Europe, the passage of learning to the East, and the beginnings of its slow infiltration back to the Christian world. The next three essays vividly portray the depths to which medieval ignorance and superstition had sunk in the light of Anglo-Saxon experience. In the sixth essay, on "The Visions of Hildegarde," we come to the first concrete evidences of the earlier attempts at a coherent philosophy in the twelfth century, and with it we see the dawn of a new era. Finally, in the concluding essay on "The School of Salerno and its Legends," we have a representative illustration of the beginnings of the centralisation of study and learning in the founding of the universities that formed so specific a factor in the period of reconstruction.

Dr. Charles Singer always writes with charm and conviction. The present

volume is no exception in this respect. It is also lavish in illustration and in the inclusion of many coloured and beautiful plates. Such a work is inevitably expensive; it is nevertheless well worth the money. It constitutes a very real contribution to the history of civilisation, and as such we unhesitatingly commend it as a success that is fully deserving alike to publisher and author.

I. B. H.

Orokaiva Magic. By F. E. WILLIAMS, with a Foreword by R. R. MARETT. [Pp. xii + 231.] (London: Oxford University Press, 1928. Price 12*s*. 6*d*. net.)

THIS volume consists of three sections entitled respectively "The Taro Cult: A Study of a Primitive Religious Movement," "The Garden Culture of the Orokaiva," and "Orokaiva Magic," which were prepared for presentation as Anthropological Reports to the Papuan Government.

Now as government reports these examples are assuredly of great value, describing in detail the origin, development and reactions of certain customs of this extremely primitive race; they bear always in mind the practical points which are the concern of the Government, namely: Are these customs in themselves harmful in any way to the subject people, or do they merely provide an innocuous and necessary outlet for primitive emotions? Do such customs have any hampering effect on the efforts of the Government to educate the aborigines and forward the evolution of their civilisation? Or again, do such customs hamper the efforts to increase the productivity of the land for the ultimate benefit of the native races and of the coloniser?

From the point of view of the anthropologist too the volume has a wide appeal, and is full of interest. The "Taro Cult"—so called because the initiates were supposed to be in touch with the spirits of the Taro plant which provides the staple food of the diet of the district, or later to be in touch with the spirits of the dead who were to encourage the growth of the plant—is another example, illustrating the extreme liability of these people to develop such cults, the influences of which sweep over the whole population and are far-reaching in their effects. This particular cult arose after the suppression of the *Baigona* cult by the Government, in the belief that this latter example was proving harmful to the people; the author tells of the origin and rapid development of the movement, indicating the source of its wide appeal to the native mind and throwing much light on the psychology and suggestibility of such people—suggestibility along certain lines only—which are in keeping with their stage of development and which act through their emotions, but not through their reason.

The second section deals with the method of gardening amongst the natives and illustrates their inherent dislike of steady work, the primitive nature of their implements and their wasteful customs with regard to the method of using and then discarding the land when the harvest has been reaped.

The section dealing with magic and sorcery throws further light on the psychology of the people. Amongst other illustrations two particularly interesting portraits are given—of Yaviripa, one of the originators of the Taro Cult, and of Bia of Manau—they suggest very alert and perhaps cunning schemers, of shallow intelligence, but not without appeal, and they are strikingly consistent with the characters described in the text.

The volume is admirable for the purpose for which it was written, but from the point of view of the general reader who may be interested in childlike races or in a knowledge of the responsibilities of the community to which he belongs, we should have welcomed some expansion of the reports before publication in book form, and an attempt to present other aspects of the life and mentality of the natives. Nevertheless, the general reader will find much to interest him and to set him thinking.

β_{10}.

BOOKS RECEIVED

(Publishers are requested to notify prices)

An Introduction to Linear Difference Equations. By Paul M. Batchelder, Ph.D., Adjunct Professor of Pure Mathematics, the University of Texas. Published with the co-operation of the National Research Council. Cambridge, Mass.: The Harvard University Press; London: Oxford University Press, 1927. (Pp. viii + 209.) Price 18*s.* net.

The Cipher of Roger Bacon. By William Romaine Newbold. Edited, with Foreword and Notes, by Roland Grubb Kent. Philadelphia: University of Pennsylvania Press; and London: Oxford University Press, 1928. (Pp. xxxi + 223.) Price 17*s.* net.

The Bakhshali Manuscript. A Study in Mediæval Mathematics. By G. R. Kaye. Archæological Survey of India. New Imperial Series. Vol. xliii. Parts I and II: Calcutta: Government of India Central Publication Branch, 1927. (Pp. 156, with 42 plates.) Price 43*s.* 6*d.* net.

Sir Isaac Newton, 1727–1927. A Bicentenary Evaluation of His Work. A series of Papers prepared under the auspices of the History of Science Society in collaboration with other Societies. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1928. (Pp. vii + 351.) Price 22*s.* 6*d.* net.

Astronomy and Cosmogony. By J. H. Jeans, M.A., D.Sc., LL.D., F.R.S. Cambridge: at the University Press, 1928. (Pp. x + 420.) Price 31*s.* 6*d.* net.

A Theory of the Solar System. By Percy John Harwood. Published by the Author, "Endersby," Ainsworth Avenue, Ovingdean, Brighton, Sussex. (Pp., Part I, 94, Part II, 63.) Price 10*s.* and 5*s.* respectively.

An Introduction to Physical Science. By Ivor B. Hart, O.B.E., Ph.D., B.Sc. Second Edition. Oxford: at the Clarendon Press, 1928. (Pp. xii + 306.) Price 4*s.* net.

Beyond the Electron. By Sir J. J. Thomson, O.M., F.R.S. A Lecture given at Girton College on March 3, 1928. Cambridge: at the University Press, 1928. (Pp. 43.) Price 2*s.* 6*d.* net.

Studies in Molecular Force. By Herbert Chatley, D.Sc., Associate of the Institute of Physics. London: Charles Griffin & Co., 42 Drury Lane, E.C.2, 1928. (Pp. xi + 118.) Price 7*s.* 6*d.* net.

Handbuch der Experimentalphysik, Band XIX. Wien-Harms. Dispersion und Absorption, von George Jaffé. Medien mit veränderlichem Brechungsindex und Lichtzerstreuung, von Richard Gans. Leipzig: Akademische Verlagsgesellschaft m.b.H. (Pp. viii + 430, with 72 figures.) Price: brosch. 39 M., geb. 41 M.

Physics for College Students. An Introduction to the Study of the Physical Sciences. By A. A. Knowlton, Ph.D., Professor of Physics, Reed College. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xix + 641.) Price 18*s.* 9*d.* net.

The Theory of Light. By the late Thomas Preston, M.A. (Dub.), D.Sc. (R.U.I.), F.R.S. Fifth Edition. Edited by Alfred W. Porter, D.Sc., F.R.S., Professor of Physics in the University of London. London: Macmillan & Co., St. Martin's Street, 1928. (Pp. xxiv + 643.) Price 25*s*. net.

Atomic Structure. As Modified by Oxidation and Reduction. By William Colebrook Reynolds, D.Sc., F.I.C., A.R.C.S. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1928. (Pp. viii + 128.) Price 7*s*. 6*d*. net.

The National Physical Laboratory. Collected Researches, Vol. XX, 1927. London: His Majesty's Stationery Office, 1928. (Pp. 444.) Price 18*s*. 6*d*. net.

Studies in Magnets and Magnetism. By R. Wild. London: Arthur H. Stockwell, 29 Ludgate Hill, E.C.4. (Pp. 70.) Price 2*s*. 6*d* net.

Volumetric Analysis. For Intermediate Students. By H. Lambourne, M.A., M.Sc., F.I.C., Head of the Chemistry Department, the Polytechnic, Regent Street, W.1, and J. A. Mitchell, M.Sc., Lecturer in the Chemistry Department, the Polytechnic, Regent Street, W.1. London: Oxford University Press, 1928. (Pp. vii + 51.) Price 2*s*. 6*d*. net.

The Chemistry of Crude Drugs. An Elementary Textbook for Students of Pharmacognosy. By John Edmund Driver, M.Sc., Ph.D., A.I.C., Lecturer in Chemistry, University College, Nottingham, and George Edward Trease, Ph.C., Lecturer in Pharmacognosy, University College, Nottingham. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1928. (Pp. vii + 157.) Price 10*s*. 6*d*. net.

Organic Syntheses. An Annual Publication of Satisfactory Methods for the Preparation of Organic Chemicals. Editor-in-Chief, Roger Adams, with Editorial Board and many contributors. Vol. VIII. New York: John Wiley & Sons; London: Chapman & Hall, 1928. (Pp. vii + 141.) Price 10*s*. net.

Elementary Organic Chemistry. By Homer Adkins and S. M. McElvain, Department of Chemistry, University of Wisconsin. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xi + 183.) Price 11*s*. 3*d*. net.

La Constituzione a l'Evoluzione. Della Materia secondo gli ultimi risultati della fisico-chimica. By Mario Motta. Roma: Prof. P. Maglione. Secc. E. Loescher & Co., 1928. (Pp. 153.)

Photochemical Processes. By George B. Kistiakowsky, Research Associate in Chemistry, Princeton University. New York: The Chemical Catalog Company, 419 Fourth Avenue, at 29th Street, 1928. (Pp. 270.) Price $5.50.

The Colloidal Salts. By Harry Boyer Weiser, Professor of Chemistry at the Rice Institute. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xi + 404.) Price 25*s*. net.

Photometric Chemical Analysis (Colorimetry and Nephelometry). By John H. Yoe, Ph.D., Professor of Chemistry, University of Virginia. Vol. I. Colorimetry. New York: John Wiley & Sons; London: Chapman & Hall, 1928. (Pp. xxi + 771.) Price 42*s*. 6*d*. net.

Chemistry for School Certificate Examinations. By W. F. F. Shearcroft, B.Sc., A.I.C., Second Master, King's School, Peterborough. London: G. Bell & Sons, 1928. (Pp. viii + 368.) Price 4*s*. 6*d*. net.

Theory of Continental Drift. A Symposium on the Origin and Movement of Land Masses. Both Inter-continental and Intra-continental, as proposed by Alfred Wegener. By W. A. J. M. van Waterschoot, van der Gracht, and others. Tulsa, Oklahama, U.S.A.; London: Thomas Murby & Co., 1 Fleet Lane, E.C.4, 1928. (Pp. x + 240, with 18 figures.) Price 15*s.* net.

The Evolution and Classification of Soils. By Dr. E. Ramann, the late Principal of the Royal Bavarian Institute for Research in Forestry. Translated by C. L. Whittles, M.A., Ph.D. Cambridge: W. Heffer & Sons, 1928. (Pp. xii + 127.) Price 7*s.* 6*d.* net.

Comparative Morphology of Fungi. By Ernst Albert Gäumann. Translated and Revised by Carroll William Dodge, Assistant Professor of Botany, Harvard University. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xiv + 701.) Price 37*s.* 6*d.* net.

A Textbook of Systematic Botany. By Deane B. Swingle, Professor of Botany, Montana State College. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xiii + 254.) Price 10*s.* net.

Sugar Beet in the Eastern Counties, 1927. Being an Investigation into the Financial Results obtained by One Hundred Farms and some of the Factors Influencing them. By R. McG. Carslaw, M.A., C. Burgess, B.A., G. Ll. Rogers, M.A., with a Foreword by J. A. Venn, M.A., University of Cambridge, Department of Agriculture. Farm Economics Branch Report No. 9, July 1928. Cambridge: W. Heffer & Sons, 1928. (Pp. xii + 94.) Price 3*s.* net.

The Romance of the Apothecaries' Garden at Chelsea. By F. Dawtrey Drewitt, M.A., M.D., Fellow of the Royal College of Physicians. Third Edition. Cambridge: at the University Press, 1928. (Pp. 175, with 15 plates.) Price 7*s.* 6*d.* net.

Pioneers of Plant Study. By Ellison Hawkes, F.R.A.S. London: The Sheldon Press. (Pp. x + 288, with 15 plates.) Price 12*s.* 6*d.* net.

Seashore Animals of the Pacific Coast. By Myrtle Elizabeth Johnson, Ph.D., Professor of Zoology, San Diego State College, California, and Harry James Snook, M.S., Head of Biology Department, Stockton High School, California. New York: The Macmillan Company, 1927. (Pp. xiv + 659, with 700 figures.) Price 32*s.* net.

How Animals Find their Way About. A Study of Distant Orientation and Place-Recognition. By Étienne Rabaud, Professor of Experimental Biology in the University of Paris. Translated by I. H. Myers, M.A. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1928. (Pp. ix + 142.) Price 7*s.* 6*d.* net.

The Idea of Memory in Biology. By E. W. MacBride, M.A., D.Sc., LL.D., F.R.S., Professor of Zoology, Imperial College of Science, London. Being the Tenth Earl Grey Memorial Lecture delivered at King's Hall, Armstrong College, Newcastle-on-Tyne, March 2, 1928. London: Oxford University Press, 1928. (Pp. 27.) Price 1*s.* net.

The Social Insects. Their Origin and Evolution. By William Morton Wheeler, Ph.D., Sc.D., LL.D., Professor of Entomology at Harvard University. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1928. (Pp. viii + 378.) Price 21*s.* net.

Biology of the Vertebrates. A Comparative Study of Man and His Animal Allies. By Herbert Eugene Walter, Professor of Biology, Brown University. New York: The Macmillan Company, 1928. (Pp. xxi + 788.) Price 21*s.* net.

The British Sea Anemones. By T. A. Stephenson, D.Sc., Zoology Department, University College, London. Vol. I. London: Printed for the Ray Society and sold by Dulau & Co., 32 Old Bond Street, W.1, 1928. (P. xii + 148, with 14 plates.)

Bibliography of Sponges, 1551–1913. By the late G. C. J. Vosmaer, Professor of Zoology in the University of Leiden. Edited by G. P. Bidder, M.A., Sc.D., and C. S. Vosmaer-Roëll. Cambridge: at the University Press, 1928. (Pp. xii + 234.) Price 5*s*. net.

The Brain from Ape to Man. A Contribution to the Study of the Evolution and Development of the Human Brain. By Frederick Tilney, Ph.D., M.D., Professor of Neurology, Columbia University. With Chapters on the Reconstruction of the Grey Matter in the Primate Brain Stem by Henry Alsop Riley, A.M., M.D., and Foreword by Henry Fairfield Osborn, Sc.D., LL.D. In two volumes. London: H. K. Lewis & Co., 1928. (Pp. xxvii + 1120, with 557 illustrations, 33 in colour.) Price £5 5*s*. net.

Leaf Mining Insects. By James G. Needham, Stewart W. Frost, and Beatrice H. Tothill. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1928. (Pp. viii + 351, with 91 figures.) Price 27*s*. net.

Contributions to Embryology. Vol. XIX, Nos. 98 to 108. Washington: Carnegie Institution, August 1927. (Pp. 300, with diagrams and plates.)

Evolution and the Spirit of Man. By J. Parton Milum, B.Sc., Ph.D. London: The Epworth Press, 1928. (Pp. 228.) Price 7*s*. 6*d*. net.

Modern Biology. A Review of the Principal Phenomena of Animal Life in Relation to Modern Concepts and Theories. By J. T. Cunningham, M.A., A.L.S. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1928. (Pp. xii + 244.) Price 10*s*. 6*d*. net.

Fundamentals of Biology. By Arthur W. Haupt, Ph.D., Assistant Professor of Botany in the University of California at Los Angeles. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xii + 358, with 256 figures.) Price 15*s*. net.

Food, Health, Vitamins. Being a New Edition of Food and Health. By R. H. A. Plimmer, D.Sc., and Violet G. Plimmer. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1928. (Pp. viii + 120.) Price 2*s*. in paper, 3*s*. in cloth.

Metanthropos, or the Body of the Future. By Ronald Campbell Macfie, M.A., M.B.C.M., LL.D. London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1928. (Pp. 96.) Price 2*s*. 6*d*. net.

Cancer, the Surgeon and the Researcher. By J. Ellis Barker. With an Introduction by Sir W. Arbuthnot Lane, Bart., C.B., M.S., F.R.C.S., Consulting Surgeon at Guy's Hospital, London. London: John Murray, Albemarle Street, W., 1928. (Pp. xii + 483.) Price 7*s*. 6*d*. net.

Fever, Heat Regulation, Climate and the Thyroid-Adrenal Apparatus. By W. Cramer, Ph.D., D.Sc., M.R.C.S., Imperial Cancer Research Fund, London. London: Longmans, Green & Co., 39 Paternoster Row, 1928. (Pp. ix + 153, with 40 plates and 11 text figures.) Price 15*s*. net.

The Blood Plasma in Health and Disease. By J. W. Pickering, D.Sc., Lecturer on Hæmatology, University of London, King's College. London: William Heinemann, 1928. (Pp. xi + 247.) Price 12*s*. 6*d*. net.

Imhotep. The Vizier and Physician of King Zoser and afterwards the Egyptian God of Medicine. By Jamieson B. Hurry, M.A., M.D. Second and Revised Edition. London: Oxford University Press, 1928. (Pp. xvi + 211, with 26 plates.) Price 10*s*. 6*d*. net.

The House-Fly. Its Life-History, Importance as a Disease Carrier and Practical Measures for its Suppression. By Major E. E. Austen, D.S.O., Keeper, Department of Entomology. Third Edition. British Museum (Natural History), Economic Series, No. 1a. London: British Museum, Cromwell Road, S.W.7, 1928. (Pp. 71, with 6 plates and 7 figures.) Price 1*s*. net.

The Extra Pharmacopœia of Martindale and Westcott. Revised by W. Harrison Martindale, Ph.D., Ph.Ch., F.C.S. Nineteenth Edition. In two volumes. Vol. I. London: H. K. Lewis & Co., 1928. (Pp. xxxvi + 1207.) Price 27*s*. 6*d*. net.

Maps. Their History, Characteristics, and Uses. A Handbook for Teachers. By Sir Herbert George Fordham, formerly Chairman of the Cambridgeshire County Council and of the County Education Committee. Second Edition. Cambridge: at the University Press, 1927. (Pp. x + 83.) Price 6*s*. net.

Psycho-Analysis for All. A Lecture delivered in Vienna by Dr. Rudolf Urbant-Schitsch. Translated by Arnold Eiloart, Ph.D., B.Sc. London: The C. W. Daniel Company, Graham House, Tudor Street, E.C.4. (Pp. 63.) Price 2*s*. 6*d*. net.

Morpheus, or the Future of Sleep. By Prof. D. F. Fraser-Harris, M.D., D.Sc., F.R.S.E. London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co. (Pp. 94.) Price 2*s*. 6*d*. net.

National Electrical Safety Code. Fourth Edition. Handbook Series of the Bureau of Standards, No. 3. Washington, D.C., U.S.A.: Government Printing Office. (Pp. xvii + 525.) Price $1.

Conversion in Science. The Huxley Memorial Lectures. By G. Elliot Smith, F.R.S., Professor of Anatomy in the University of London. London: Macmillan & Co., St. Martin's Street. (Pp. 28). Price 1*s*. net.

Telephone and Power Transmission. By R. Bradfield, B.A., and W. J. John, A.R.C.Sc., A.M.I.E.E. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1928. (Pp. xi + 238.) Price 21*s*. net.

Sound. Part III. For School Certificate Students. By E. Nightingale, M.Sc., A.I.E.E., Senior Science Master at St. Albans School, St. Albans. London: G. Bell & Sons, 1928. (Pp. ix + 379.) Price 2*s*. 6*d*. net.

Elements of Optics. By Joseph Valasek, Ph.D., Associate Professor of Physics, University of Minnesota. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xiii + 215.) Price 10*s*. net.

Judgment and Reasoning in the Child. By Jean Piaget. In collaboration with Mlles E. Cartalis, S. Escher and others. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1928. (Pp. viii + 257.) Price 10*s*. 6*d*. net.

A.B.C. of Adler's Psychology. By Philippe Mairet. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1928. (Pp. 116.) Price 3*s*. 6*d*. net.

South Lancashire. By A. Wilmore, D.Sc., F.G.S., F.R.G.S. Cambridge: at the University Press, 1928. (Pp. x + 160, with illustrations.) Price 3*s*. 6*d*. net.

The Symbolic Process and its Integration in Children. A Study in Social Psychology. By John F. Markey, Ph.D., Research Professor of Sociology, Experiment Station, Connecticut Agricultural College. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1928. (Pp. xii + 192.) Price 10*s*. 6*d*. net.

Hanno, or the Future of Exploration. By J. Leslie Mitchell. London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1928. (Pp. 94.) Price 2*s*. 6*d*. net.

Measurement and Calculation. By Norman Robert Campbell, Sc.D., F.Inst.P., a Member of the Research Staff of the General Electric Company, Wembley. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1928. (Pp. ix + 293.) Price 12*s*. 6*d*. net.

The Purdah System and its Effect on Motherhood. By Kathleen Olga Vaughan, M.B., sometime Superintendent of His Highness the Maharajah of Yammu and Kashmir's Diamond Jubilee Zenana Hospital, Srinagar, Kashmir. With a Preface by Leonard Hill, M.B., F.R.S., and an Introduction by Howard A. Kelly, M.D. Cambridge: W. Heffer & Sons, 1928. (Pp. xv + 48.) Price 2*s*. 6*d*. net.

Industrial Catalysis. By Stanley J. Green, M.A., A.I.C. London: Ernest Benn, Bouverie House, Fleet Street, E.C.4, 1928. (Pp. xi + 507, with 198 illustrations.) Price 50*s*. net.

The Industrial Uses of Bauxite. With an Account of its Origin, Occurrence, Composition, and Properties. By N. V. S. Knibbs, D.Sc., Consulting Chemical Engineer. London: Ernest Benn, Bouverie House, Fleet Street, E.C.4, 1928. (Pp. viii + 141.) Price 21*s*. net.

Probability, and its Engineering Uses. By Thornton C. Fry, Ph.D., Member of the Technical Staff, Bell Telephone Laboratories. London: Macmillan & Co., St. Martin's Street, 1928. (Pp. xiv + 470.) Price 30*s*. net.

The Unconscious in Action. Its influence upon Education. By Barbara Low. With a Foreword by T. Percy Nunn, M.A., D.Sc., D.Lit., Professor of Education in the University of London. London: University of London Press, 10 Warwick Lane, E.C.4, 1928. (Pp. 226.) Price 5*s*. net.

Geschichte der Alchemie. Herausgegeben und eingeleitet von Franz Strunz. Leipzig: Munchen-Planegg, Otto Wilhelm Barth-Verlag, G.m.b.H., 1928. (Pp. 613.) Price Rm. 10.

Astrologie, Alchemie, Mystik. Ein Beitrag zur Geschichte der Naturwissenschaften. Leipzig: Munchen-Planegg, Otto Wilhelm Barth-Verlag, G.m.b.H., 1928. (Pp. 340.) Price Rm. 6.

A New Experimental Science. By J. G. Frewin. Part III. Oxford: at the Clarendon Press, 1928. (Pp. 95.) Price 1*s*. 6*d*. net.

Origin and Development of the Microscope. As Illustrated by Catalogues of the Instruments and Accessories, in the Collections of the Royal Microscopic Society, together with Bibliographies of Original Authorities. Edited by Alfred N. Disney, M.A., B.Sc., F.R.M.S., in collaboration with Cyril F. Hill and Wilfred E. Watson Baker. Preceded by an Historical Survey on the Early Progress of Optical Science by the Editor. London: The Royal Microscopical Society, 20 Hanover Square, W.1, 1928. (Pp. xi + 297, with 30 plates and 36 text-figures.) Price 17*s*. 6*d*. net.

Horology. The Science of Time Measurement and the Construction of Clocks, Watches, and Chronometers. By J. Eric Haswell, F.B.H.I. London: Chapman & Hall, 1928. (Pp. xvi + 267, with 19 plates and 106 figures.) Price 25*s*. net.

From Magic to Science. Essays on the Scientific Twilight. By Charles Singer. London: Ernest Benn, 1928. (Pp. xix + 253, with 14 plates and 108 figures.) Price 25*s*. net.

Psychology of Infancy and Early Childhood. By Ada Hart Arlitt, Ph.D., Professor of Child Care and Training, University of Cincinnati. London: McGraw-Hill Publishing Co., Bouverie Street, E.C.4, 1928. (Pp. xi + 228.) Price 10*s*. net.

Impurities in Metals. Their Influence on Structure and Properties. By Colin J. Smitholls, M.C., D.Sc. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1928. (Pp. xi + 157, with 166 figures.) Price 18*s*. net.

Mosquito Surveys. A Handbook for Anti-Malarial and Anti-Mosquito Field Workers. By Malcolm E. MacGregor. London: Baillière, Tindall & Cox, 7 Henrietta Street, Covent Garden, 1927. (Pp. 294, with 59 figures.) Price 15*s*. net.

The Rôle of Scientific Societies in the Seventeenth Century. By Martha Ornstein. Chicago: The University of Chicago Press. (Pp. xiv + 308.) Price 15*s*. net.

The Thirsty Earth. A Study in Irrigation. By E. H. Carrier, M.A., M.Sc., F.R.Hist.S. London: Christophers, 22 Berners Street, W.1. (Pp. vii + 222.) Price 10*s*. 6*d*. net.

Crossroads in the Mind of Man. A Study of Differentiable Mental Abilities. By Truman L. Kelley, Professor of Education and Psychology, Stanford University. California: Stanford University Press, 1928. (Pp. vii + 237.) Price 4$ net.

Gentlemen Prefer Aeroplanes. Complementary to This Airship Business. By E. F. Spanner. London: E. F. Spanner, 9 Billiter Square, E.C.3. (Pp. xvii + 457.) Price 35*s*. net.

Printed in Great Britain by
Hazell, Watson & Viney, Ld., London and Aylesbury.

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By E. C. Titchmarsh, M.A., University College, London.

Almost Periodic Functions.—The theory of almost periodic functions was founded by H. Bohr, in three memoirs published in the *Acta Mathematica.*[1] In these memoirs he extended the ordinary theory of periodic functions and their representation by Fourier series to a more general class of functions. A simple function which is almost periodic, without being periodic, is $f(x) = \cos x + \cos x\sqrt{2}$. It is the sum of two periodic functions, and has many properties in common with periodic functions. But it is not actually periodic; for example, it takes the value 2 for $x = 0$, but not for any other value of x.

We begin by recalling some theorems in the classical theory of Fourier series. Let $f(x)$ be a periodic function, say with period 2π. We associated with it the Fourier series

$$\sum_{n=-\infty}^{\infty} a_n e^{inx}$$

where
$$a_n = \frac{1}{2\pi}\int_{-\pi}^{\pi} f(x) e^{-inx}\, dx.$$

This complex form of the series is perhaps less familiar than the ordinary real form, but it is easily seen that it really amounts to the same thing. The coefficients A_n are derived from the "orthogonal" property of the functions e^{inx}, viz. that

$$\frac{1}{2\pi}\int_{-\pi}^{\pi} e^{i(m-n)x}\, dx = 1 \ (m = n), = 0 \ (m \neq n).$$

We know that if $f(x)$ satisfies certain simple conditions, *e.g.* if it is continuous and has only a finite number of maxima and minima in $(-\pi, \pi)$, then the series converges to the sum $f(x)$ for all values of x. Apart from the question of the con-

[1] See the list of papers at the end.

vergence of the series, there are many interesting properties of the coefficients a_n. Thus by evaluating the integral

$$\frac{1}{2\pi}\int_{-\pi}^{\pi}\left|f(x) - \sum_{n=-N}^{N} a_n e^{inx}\right|^2 dx,$$

which is obviously positive, we obtain at once Bessel's inequality:

$$\sum_{n=-N}^{N}\left|a_n\right|^2 \leqslant \frac{1}{2\pi}\int_{-\pi}^{\pi}\left|f(x)\right|^2 dx.$$

A less simple argument leads to Parseval's theorem:

$$\sum_{n=-\infty}^{\infty}\left|a_n\right|^2 = \frac{1}{2\pi}\int_{-\pi}^{\pi}\left|f(x)\right|^2 dx,$$

which holds for all functions $f(x)$ such that the integral on the right exists. The same thing may be expressed by saying that—

$$\lim_{N\to\infty}\int_{-\pi}^{\pi}\left|f(x) - \sum_{-N}^{N} a_n e^{inx}\right|^2 dx = 0,$$

or, as we say, the Fourier series converges in mean square over $(-\pi, \pi)$ to the function $f(x)$.

Suppose now that, instead of starting with the functions e^{ix}, e^{2ix}, all of which have the same period 2π, we take the functions $e^{i\lambda x}$, where λ can assume any real value. These functions are not orthogonal over any finite interval, but they are, in a certain sense, orthogonal over the infinite interval $(-\infty, \infty)$. In fact

$$\lim_{T\to\infty}\frac{1}{2T}\int_{-T}^{T} e^{i(\lambda_1-\lambda_2)x}\, dx = 1\ (\lambda_1=\lambda_2), = 0\ (\lambda_1 \neq \lambda_2).$$

We may ask, then, what kind of functions $f(x)$ can be expanded in an orthogonal series of this type, that is a series of the form

$$\Sigma\, a_n e^{i\lambda_n x},$$

where, by analogy with the ordinary Fourier series, we put

$$a_n = \lim_{T\to\infty}\frac{1}{2T}\int_{-T}^{T} f(x)e^{-i\lambda_n x}\, dx.$$

This question was answered by Bohr in his first memoir. At any rate, he defined a very general class of functions which do permit of this kind of expansion.

Let $f(x)$ be a real or complex function of the real variable

x, continuous for all values of x in $(-\infty, \infty)$. Suppose that, for a given value of ϵ, we can find a number τ such that

$$|f(x+\tau)-f(x)| \leq \epsilon$$

for all values of x. Such a number is called a translation number of $f(x)$ belonging to ϵ. Thus a period would be a translation number corresponding to $\epsilon = 0$.

Now a periodic function, say $\cos x$, has not merely one period 2π, but an infinity of periods $2n\pi$, equally spaced out. We also require an extension of this property. *A continuous function* f(x) *is said to be almost periodic in Bohr's sense* (*a.p.*), *if to every positive number* ϵ, *however small, corresponds a positive number* l, *such that every interval of length* l *contains a translation number of* f(x) *belonging to* ϵ. Thus in the set of translation numbers, corresponding to a given ϵ, there are no arbitrarily large gaps. We call a set of numbers with this property "relatively dense." Thus the positive integers are relatively dense, but the squares of integers are not. Of course, the smaller ϵ is, the large l may have to be.

It is clear that any periodic function is almost periodic. An a.p. function does not necessarily return exactly to a value it has once taken, but it does return to it approximately, and that at a relatively dense set of points.

Sums and products of a.p. functions are also a.p. functions. The proof of this is quite elementary, though not particularly easy. In particular any sum of the form

$$a_1 e^{i\lambda_1 x} + \ldots + a_n e^{i\lambda}$$

is an a.p. function.

For any a.p. function $f(x)$ the mean-value

$$\lim_{T\to\infty} \frac{1}{T}\int_0^T f(x)dx$$

exists. We denote this by $M\{f(x)\}$. Since $e^{-i\lambda x}f(x)$ is also a.p., being the product of two a.p. functions, $M\{e^{-i\lambda x} f(x)\}$ exists for all values of λ. Thus we can form a Fourier coefficient of $f(x)$ corresponding to any term $e^{i\lambda x}$. Suppose that $\lambda_1, -- \lambda_N$ are any different real numbers, and $a(\lambda_1), \ldots$ the corresponding Fourier coefficients. Then the argument by which the ordinary Bessel's inequality is proved may be extended at once to show that

$$\sum_{n=1}^{N} |a(\lambda_n)|^2 \leqq M\{|f(x)|^2\}.$$

This has the important consequence that there are at most an enumerable set of values of λ for which the Fourier coefficient $a(\lambda)$ is different from zero; for the number of values of λ for

which, say, $|a(\lambda)| \geqslant 1/m$, is at most m^2 M$\{|f(x)|^2\}$. Thus we may arrange in a sequence, say $\Lambda_1, \Lambda_2, \ldots$ the values of λ with non-zero coefficients. Let A_n be the coefficient corresponding to Λ^n. Then the Bohr-Fourier series of $f(x)$ is

$$\Sigma A_n e^{i\Lambda_n x}.$$

The " Bessel's inequality " argument gives at once

$$\Sigma |A_n|^2 \leq M\{|f(x)|^2\}.$$

The analogue of Parseval's theorem, that

$$\Sigma |A_n|^2 = M\{|f(x)|^2\},$$

is also true ; but the proof of this is of exceptional difficulty, and the result is of fundamental importance in the theory. It is known as the " fundamental theorem."

Three essentially different proofs of this theorem have been given. The original one, due to Bohr, is fairly elementary, but very long and difficult to grasp as a whole. A second proof, depending on the theory of integral equations, was given by Weyl. He starts with the observation that $\rho = a(\lambda)$, $\phi(s) = e^{i\lambda s}$, give a solution of the integral, or rather mean-value, equation

$$M\{f(s-t)\,\phi(t)\} = \rho\,\phi(s).$$

Now formulæ analogous to Parseval's are known in the ordinary theory of integral equations, and the fundamental theorem is an extension of them to mean-value equations. Anyone with a good knowledge of integral equations would probably find Weyl's proof much easier than Bohr's, though in a sense it is not so elementary.

A third proof depends on the theory of Fourier integrals, and is due to N. Wiener. In its original form this also was very complicated, but it has recently been presented in a remarkably simple and elegant form. The well-known theorem on Fourier integrals, that if

$$F(x) = \frac{1}{\sqrt{(2\pi)}}\int_{-\infty}^{\infty} f(t)e^{ixt}\,dt,$$

then
$$\int_{-\infty}^{\infty} |F(x)|^2\,dx = \int_{-\infty}^{\infty} |f(x)|\,dx$$

provided these integrals are convergent, is of the same type as the desired result, except that it involves integrals instead of series ; and it forms the basis of Wiener's proof.

An immediate consequence of the fundamental theorem is that

$$\lim_{N\to\infty} M\{|f(x) - \sum_{n=1}^{N} A_n e^{i\Lambda_n x}|^2\} = 0,$$

i.e. that the Fourier series converges in mean square (in a generalised sense) to $f(x)$. Another result, the uniqueness theorem, is that if all the Fourier coefficients of $f(x)$ are zero, then $f(x)$ is identically zero; or, if two functions have the same Fourier series, they are identical. Naturally these statements only apply to a.p. functions. It is easily seen that all the Fourier coefficients of e^{ix^2} are zero; but this function is, of course, not a.p.

The main theorem of Bohr's second memoir is that we can approximate uniformly to any a.p. function, whose Fourier series is

$$\Sigma\, A_n e^{i\Lambda_n x},$$

by finite sums of the form

$$\sum_{n=1}^{N} a_n\, e^{i\Lambda_n x}.$$

Here the exponents Λ_n are the same as those which occur in the Fourier series, but the coefficients a_n are not necessarily the same as the Fourier coefficients A_n. Conversely, any function to which we can approximate uniformly by finite trigonometrical sums is a.p. Thus the method of approximation gives a second characterisation of the class of a.p. functions.

The approximation theorem was proved by Bohr by means of "grenzperiodischer" functions (gp. functions). A gp. function is an a.p. function all of whose Fourier exponents are rational multiples of one of them; or (an equivalent definition), a gp. function is a function to which we can approximate uniformly by means of pure periodic functions. Thus

$$f(x) = \sum_{n=1}^{\infty} 2^{-n}\, e^{ix/n}$$

is a gp. function, the approximating function

$$f_N(x) = \sum_{n=1}^{N} 2^{-n}\, e^{ix/n}$$

having the period $2\pi N$! To prove the approximation theorem, we express Λ_n in the form

$$\Lambda_n = r_{n,1}\,\beta_1 + r_{n,2}\,\beta_2 + \ldots + r_{n,q_n}\,\beta_{q_n},$$

where $r_{n,1}, \ldots$ are rational, and write

$$\Sigma\, A_n\, e^{i\Lambda_n x} = \Sigma\, A_n e^{i(r_{n,1} x_1 + \ldots + r_{n,q_n} x_{q_n})},$$

thus reducing the given a.p. function to a gp. function of an infinity of variables. But we can apply to gp. functions methods of summation similar to those in use for Fourier series. This leads to the desired result.

A new proof, independent of the theory of gp. functions of an infinity of variables, was given by Bochner. He uses the idea of a "special set" of a.p. functions, *i.e.* a set throughout which the properties of continuity and almost-periodicity hold uniformly. If such a set converges in mean square to $f(x)$, then it converges uniformly to $f(x)$ in the ordinary sense. Now we can find special sets of finite sums which converge in mean square to $f(x)$, by a method analogous to that of Fejér for summing ordinary Fourier series. This gives the result again.

A third proof, the most elementary of all, is due to Weyl. He considers the integral

$$\frac{1}{2T}\int_{-T}^{T} f_p(s+t)x(t)dt$$

where
$$f_p(s) = f(s) - \sum_{n=1}^{p} A_n\, e^{i\Lambda_n s},$$

and $x(t)$ is equal to $1/2\delta$ in the intervals $(\tau_n - \delta,\ \tau_n + \delta)$ and is zero elsewhere, the numbers τ_n being a relatively dense set of translation numbers of $f(x)$. It follows from the fundamental theorem that the above integral can be made arbitrarily small by choice of p. But the part of the integral which comes from $f(s)$ is approximately equal to $f(s)$, while the remainder is a finite trigonometrical sum of the required form.

Still another method of defining a.p. functions was given by Bochner. A continuous function $f(t)$ is a.p. if from any sequence of functions of the form

$$f(t+h_1),\ f(t+h_2),\ \ldots -$$

it is possible to select a subsequence which converges uniformly for all values of t. This definition, apparently quite new, leads to exactly the same class of functions as the previous ones.

Bohr's third memoir is on analytic a.p. functions, and connects the theory with that of Dirichlet series. An analytic function $f(s)$ of the complex variable $s = \sigma + it$ is said to be a.p. in the strip $\alpha < \sigma < \beta$ if it is an a.p. function of t for all such values of σ, and if the functions $f(\sigma + it)$ form a special set in the sense already defined. By the real-variable theory, each such function has a Fourier expansion

$$f(s) \backsim \sum_{n=1}^{\infty} A_n^{(\sigma)}\, e^{i\Lambda_n^{(\sigma)} t}.$$

It is a simple exercise in contour integration to prove that the exponents $\Lambda_n^{(\sigma)}$ are the same for all values of σ, and that $A_n^{(\sigma)} = A_n\, e^{\Lambda_n \sigma}$, where A_n is independent of σ. Thus

$$f(s) \backsim \sum_{n=1}^{\infty} A_n\, e^{\Lambda_n(\sigma + it)}$$

for all values of σ in the strip. Thus the series on the right reduces to something like the ordinary Dirichlet form.

Actually we need assume (explicitly) much less about $f(s)$ than we have done. The same results hold if $f(s)$ is an analytic function, bounded in the strip, and a.p. along one line $\sigma=\sigma_0$ in the strip.

The expansion we have obtained is a generalisation of that of Laurent for periodic functions. If $f(s)$ is periodic in the strip with period ip, its Laurent series

$$\sum_{n=\infty}^{\infty} A_n\, e^{2\pi ns/p}$$

coincides "substantially" with its Dirichlet series, *i.e.* apart from the order of the terms, and the possible occurrence of zero-terms in the Laurent form.

The uniqueness theorem has an obvious extension to analytic a.p. functions. Its later developments are much less obvious. The final form of the theorem (Bohr 5) is as follows: let $F_1(t)$, $F_2(t)$, be two a.p. functions, whose Fourier series

$$\Sigma\, B_n\, e^{i\Lambda_n t},\ \Sigma\, C_n\, e^{i\Lambda_n t}$$

correspond, in the sense that they have the same exponents, and

$$B_n = A_n\, e^{\Lambda_n \sigma_1},\ C_n = A_n\, e^{\Lambda_n \sigma_2},$$

where the A_n are constants. Then there is a function $f(s)$, continuous in $\sigma_1 \leqq \sigma \leqq \sigma_2$ and analytic inside the strip, and a.p. in the strip, such that

$$F_1(t) = f(\sigma_1 + it),\ F_2(t) = f(\sigma_2 + it).$$

The problem whether an analytic function $f(s)$ can be regular for $\sigma_1 \leqq \sigma \leqq \sigma_2$ and a.p. along $\sigma=\sigma_1$ and $\sigma=\sigma_2$, without being a.p. in the strip, is unsolved. The answer is, of course, in the negative if the function is bounded, or even if it satisfies a fairly wide inequality.

Returning to the real-variable theory, we have still to consider the problem of the convergence (or summability) of the Fourier series. The problem is complicated by the fact that the exponents Λ_n are not necessarily spaced out like those of an ordinary Fourier series; in fact they may be everywhere dense; they may, for example, consist of all rational numbers. Suppose, however, that there are gaps; suppose, for example, that there is a sequence of indices $m_1, m_2, \ldots$ such that for every k

$$\Lambda_{m_k+1} - \Lambda_{m_k} \geqq 1.$$

Then the partial sums of the Fourier series which end at Λ_{m_1}, $\Lambda_{m_2}, \ldots$ behave in the same sort of way as the partial sums

of an ordinary Fourier series, and the classical tests for convergence and summability can be extended at once to sequences of partial sums of this type. This follows from the fact (extension of Fejér's integral) that

$$\sigma_\lambda(x) = \frac{1}{\pi}\int_{-\infty}^{\infty} f(x+2t)\,\frac{\sin^2\lambda t}{\lambda t^2}\,dt$$

is an a.p. function, whose Fourier series is

$$\sum_{|\Lambda_n|<\lambda}\left(1-\frac{\Lambda_n}{\lambda}\right)A_n\, e^{i\Lambda_n x}.$$

Now $\sigma_\lambda(x) \to f(x)$ at every point where $f(x)$ is continuous. This gives "summability C1" if the numbers Λ_n have no limit-point except at infinity. Otherwise the series by which we approximate are themselves infinite.

Similarly we discuss the convergence problem by considering the integral (extension of Dirichlet's integral)

$$S_\lambda(x) = \frac{1}{\pi}\int_{-\infty}^{\infty} f(x+2t)\,\frac{\sin(2\lambda+1)t}{t}\,\frac{\sin t}{t}\,dt.$$

Bohr-Fourier series in which the exponents satisfy certain special conditions have some remarkable properties. If the exponents Λ_n are linearly independent, *i.e.* if there is no relation of the form

$$r_1\Lambda_1 + \ldots + r_n\Lambda_n = 0,$$

where the r_n are integers, not all zero, then the Fourier series is absolutely convergent. Another condition for absolute convergence is that all the Fourier coefficients should have the same sign.

If the Fourier exponents are bounded, *i.e.* if $a \leqq \Lambda_n \leqq b$ for all values of n, then the generating function is an integral function; in fact, an a.p. function with bounded exponents is very much like a Fourier integral with finite limits.

So far all the functions which we have discussed have been continuous. The theory was extended to discontinuous functions by Stepanoff and Wiener. A function $f(x)$ is almost periodic in Stepanoff's sense (a.p. S.), if it is finite almost everywhere and measurable, and if to any positive ϵ corresponds a relatively dense set of numbers τ, such that $|f(x+\tau) - f(x)| < \epsilon$, except for a set of values of x whose measure, in any interval of length l, is less than ϵ. This definition is too general to lead us very far. A function which is integrable in Lebesgue's sense over any finite interval, and such that, given $\epsilon > 0$, we can find a relatively dense set of numbers τ such that for all a

$$\int_a^{a+1}\left|f(x+\tau) - f(x)\right|dx \leqq \epsilon$$

is said to be a.p. S. 1. Any such function has a Fourier series of Bohr's type.

A still more interesting class of functions (a.p. S. 2) is obtained by supposing that $|f(x)|^2$ is integrable over any finite interval, and that

$$\int_a^{a+1} f(x+\tau) - f(x) \quad dx \leqq \epsilon$$

for all a and a relatively dense set of numbers τ. This definition is equivalent to that given by Wiener. If a function is a.p. S. 2, it is a.p. S. 1, and so has a Fourier series. But in the S. 2 case we have also the analogue of Bohr's fundamental theorem, that

$$\Sigma |A_n|^2 = M\{|f(x)|^2\}.$$

The Stepanoff class S. 2 therefore includes Bohr's class, and preserves one of its most important properties.

It is natural to ask at this point whether we can prove a converse theorem of the Riesz-Fischer type, that is, if $\Sigma |A_n|^2$ is convergent, whether there is a function $f(x)$ which generates the series $\Sigma A_n e^{i\Lambda_n x}$. It was proved by Besicovitch that there is such a series which does not correspond to any function of the class S. 2; but he showed that a function of a certain more general class always corresponds to such a series. A function $f(x)$ is a generalised a.p. function in Besicovitch's sense if there is a set of a.p. functions $s_1(x)$, $s_2(x)$, . . . such that

$$\lim_{n\to\infty} \overline{\lim_{T\to\infty}} \frac{1}{T} \int^T \left| f(x) - s_n(x) \quad dx = 0.\right.$$

To every such function correspond a Fourier series $\Sigma A_n e^{i\Lambda_n x}$ such that

$$\Sigma |A_n|^2 = M\{|f(x)|^2\};$$

and to every series $\Sigma A_n e^{i\Lambda_n x}$ with $\Sigma |A_n|^2$ convergent corresponds one such function, if we do not consider functions $f(x)$, $g(x)$, such that

$$\lim_{T\to\infty} \frac{1}{T} \int^T \left| f(x) - g(x) \right|^2 dx = 0$$

as distinct.

We may note finally that Bohr's theory has already found applications in the theory of differential equations.

The following are the principal papers which have hitherto appeared on the subject:

H. Bohr (1) (2) (3), "Zur Theorie der fastperiodischen Funktionen," *Acta Math.*, **45** (1924), 29–127; **46** (1925), 101–214; and **47** (1925), 237–281.
(4) "Einige Sätze über Fourierreihen fastperiodischer Funktionen," *Math. Zeitschrift*, **23** (1925), 38–44.

(5) "Sur le Théorème d'unicité dans la théorie des Fonctions presque Périodiques," *Bull. Sciences Math.* (2), **50** (1926), 1–5.
(6) "On the Explicit Determination of the Upper Limit of an Almost Periodic Function," *Journal London Math. Soc.*, **1** (1926).
(7) "Ein Satz über analytisch Fortsetzung fastperiodischer Funktionen," *Journal für Math.*, **157**, 61–5.
H. Bohr and O. Neugebauer, "Über lineare Differentialgleichungen mit konstanten Koeffizienten und fastperiodischer rechter Seite," *Göttinger Nachrichten*, 1926, **1–18**.
A. Besicovitch and H. Bohr, (1) "Some Remarks on Generalisations of Almost Periodic Functions," *Danske Vidensk Selskab.*, **8** (1927), No. 5.
(2) "On Almost Periodic Properties of Translation Numbers," *Journal London Math. Soc.*, **3** (1928), 172–6.
A. Besicovitch, "On Generalised Almost Periodic Functions," *Proc. London Math. Soc.* (2), **25** (1926), 495–512.
S. Bochner, (1) "Sur les fonctions presque périodiques de Bohr," *Comptes Rendus*, **180**, 1156.
(2) (3) "Beiträge zur Theorie der fastperiodischen Funktionen," *Math. Annalen*, **96** (1926), 119–47 and 383–409.
(4) "Über Fourierreihen von fastperiodischer Funktionen," *Berliner Sitzungsberichte*, **26**, 1926.
(5) "Properties of Fourier Series of Almost Periodic Functions," *Proc. London Math. Soc.* (2), **26** (1927), 433–52.
(6) "Konvergenzsätze für Fourierreihen grenzperiodischer Funktionen," *Math. Zeitschrift*, **27** (1927), 187–211.
J. Favard, "Sur les équations différentielles linéaires à coefficients presque périodiques," *Acta Math.*, **51** (1927), 31–81.
E. H. Linfoot, "Generalization of Two Theorems of H. Bohr," *Journal London Math. Soc.*, **3** (1928), 177–82.
W. Stepanoff, "Über einige Verallgemeinerungen der fastperiodischen Funktionen," *Math. Annalen*, **95** (1926), 473–98.
G. Szegö, "Zur Theorie der fastperiodischen Funktionen," *Math. Annalen*, **96** (1927), 378–82.
H. Weyl, "Integralgleichungen und fastperiodische Funktionen," *Math. Annalen*, **97** (1927), 338.
N. Wiener, (1) "On the Representation of Functions by Trigonometrical Integrals," *Math. Zeitschrift*, **24** (1925), 575–617.
(2) "The Harmonic Analysis of Irregular Motion," *J. Math. and Phys., Massachusetts Institute of Technology*, 1926.
(3) "The Spectrum of an Arbitrary Function," *Proc. London Math. Soc.* (2), **27** (1928), 487–96.

ASTRONOMY. By W. M. SMART, M.A., D.Sc, Observatory, Cambridge.

IN the *Publications of the Washburn Observatory* (University of Wisconsin), vol. xv, Professor Joel Stebbins describes his long and patient work on the photoelectric photometry of the stars. Guthnick, Rosenberg, and Stebbins are the three pioneers in this difficult branch of practical astronomy, and the volume under consideration presents a vivid account of the immense instrumental difficulties encountered, the slow progress made in overcoming them, and the final successes with variable stars achieved. A large proportion of Stebbins' accurate work was done with a quartz cell, which displayed fewer idiosyncrasies than the many glass cells used in the

preliminary experimental work. The apparatus attached to the telescope consists essentially of two parts, (1) the cell, of which the photoelectric metal (sodium, potassium, etc.) is maintained at a high potential depending on the characteristics of the particular cell, and (2) a sensitive galvanometer or electrometer with which to measure the photoelectric current resulting from the action of the starlight on the metal of the cell. The electrometer in Stebbins' apparatus is of the "string" variety. The main features of the Wisconsin instrument, which is attached to the 15-inch refractor, are illustrated in a photograph. If the sensitivity of such an installation is expressed in terms of volts per second produced by a star of standard brightness (this is taken to be of magnitude 1·0 and spectral type A0), Stebbins' instrument gives as the result 3 volts per second, which means a sweep of the electrometer thread across a field of forty divisions (on an arbitrary scale) in one-third of a second; a seventh magnitude star of the same spectral type produces a sufficiently rapid drift of five divisions in ten seconds. For still fainter stars the irregularities of the apparatus become more important in relation to the current to be measured, and accordingly 7·0 represents approximately the limiting magnitude which can be usefully observed. The question of atmospheric absorption is discussed; this is naturally of great importance (magnitudes are measured to the third decimal place) when there is a marked difference in altitude between the variable star and the comparison star, which latter serves as a standard of reference by which the magnitude changes of the variable can be measured. The accuracy with which the differential observations can be made with the apparatus is represented by a probable error of ± 0·012 m. for a single observation. Stebbins gives a list of 235 stars observed between 1916 and 1926; it includes variables and suspected variables together with the comparison stars. Most of the former stars are spectroscopic binaries; the closer the binary, the greater is the chance that it is also an eclipsing binary. Over a score of spectroscopic binaries have been discovered by Stebbins to be variable, and the resultant knowledge of these systems is of very great value.

In the 38*th Annual Report* of the Astronomer Royal for Scotland (1928), Professor Sampson notes that considerable progress has been made in respect of the new equipment which has been authorised by H.M. Treasury. The contract for the 36-inch reflector has been let to Messrs. Grubb, Parsons & Co., and for the spectrograph to Messrs. Adam Hilger, Ltd. The programme with the 6-inch photovisual telescope in the spectroscopic observation of stars down to the third magnitude has been continued; from the observations the distribution

of energy over their spectra is measured, and hence the effective temperatures can be deduced. The study of atmospheric absorption as it affects this work has also been continued.

In *Monthly Notices R.A.S.*, **88**, p. 548 (May 1928), Dr. J. A. Carroll contributes an interesting paper on "The form of an absorption line in the spectrum of a rotating or expanding star." The observed spectrum is of course the integrated result from each element of the stellar surface: if the star is expanding or rotating, the line of sight velocity varies from element to element of the surface, and consequently the position and relative intensities within the absorption lines may be anticipated to be affected. Carroll also includes in his discussion the effect of darkening towards the limb. The case of an expanding star may be briefly mentioned—for any likely velocity of expansion, the absorption line is merely displaced without any change in its form. But in the case of a rotating star, although the centre of the line is undisplaced, the line is broadened and its intensity depth reduced by an amount depending on the equatorial velocity of the star (or rather the component in the line of sight). If the distribution of intensity within an absorption line can be measured, it is possible, by applying the formulæ of the paper, to deduce the star's rotational speed. Carroll has examined several spectra, but he concludes that so far there is no evidence of stars rotating with an equatorial velocity greater than 50 kilometres per second. The method is, however, likely to be given an extended trial as soon as more refined photometric measurements are available.

Contribution No. 356 *from Mt. Wilson Observatory* by A. van Maanen records the results of the tenth series of parallax determinations. This work was originally begun in 1913 for the express purpose of providing adequate material to Dr. Adams and his associates in connection with the new method of estimating the distances of the stars from certain characteristics in their spectra, these characteristics being standardised by means of stars of known parallax and spectral type. In fourteen years, 250 fields have been photographed and measured, and the distances of 277 objects determined. These figures give some idea of the rate of progress attainable in this fundamental department of astronomy. It should be added that practically the whole of this work has been done by Dr. van Maanen alone. At different times, the originally planned programme has been extended by the inclusion of special classes of objects, amongst which we may mention the following: (*a*) faint stars with very large proper motions. The most noteworthy of the twenty stars of this class is Wolf 359 which,

with the exception of the triple system of *a* Centauri and Barnard's star, is our nearest stellar neighbour. With plausible assumptions its mass is estimated to be one-tenth that of the sun, its volume approximately that of Uranus, and its density about two thousand times that of water ; (*b*) eclipsing variables ; (*c*) Cepheids ; (*d*) long-period variables ; (*e*) M-type stars with absolute magnitudes possibly intermediate between the hitherto well-separated giant and dwarf groups. Two suspected stars were measured and found " not guilty " of tampering with the giant and dwarf classification ; (*f*) novæ, and finally (*g*) planetary nebulæ. The mean absolute magnitude of the last objects is very much on a par with the mean absolute magnitude of the novæ at minimum, and van Maanen adds that this agreement would appear to strengthen the belief that planetary nebulæ are old novæ, a belief which is based both on the resemblance of their spectra and on the fact that in a few instances nebulæ have been observed to form around novæ. The objections to this point of view are also examined.

In *Contribution No.* 357, van Maanen discusses the systematic errors, depending on right ascension, in modern trigonometrically determined parallaxes. The material discussed is restricted to the recent series of observations made at the Alleghany, Dearborn, Greenwich, McCormick, Mt. Wilson, Sproul, and Yerkes Observatories. Such errors have very probably a seasonal origin, and the causes tentatively suggested are the fatigue of the observer, the fatigue being supposed to be different in summer from that in winter with its long cold nights, errors in the driving clock which are likely to be different in the evening and morning observations and in winter and in summer, differences in seeing which again will have a diurnal and seasonal variation, and the differences in galactic latitude of the fields observed, for when the field is in or near the Milky Way, the comparison stars can be chosen sufficiently near the parallax star, thus tending to mitigate the effects previously noted. Some of these causes may be expected to give a twelve-hour, some a twenty-four hour effect (in right ascension). The standards of reference in van Maanen's discussion are taken to be the spectroscopic parallaxes which are not likely to be influenced by the causes already mentioned. Van Maanen's analysis of the available parallaxes points strikingly to the existence of systematic errors which are functions of the right ascension. He then gives a table of corrections to be applied to the observed parallaxes in each hour of right ascension for each of the seven observatories ; the range in the corrections (to two places of decimals) is found to be 0·01" for Mt. Wilson and Alleghany parallaxes, 0·05" for Dearborn, and 0·02" for the remainder.

Harvard College Observatory Circular No. 317, by H. Shapley and C. H. Payne, is entitled "Spectroscopic evidence of the fall of meteors into stars." Arguing from the enormous numbers of meteors in the solar system, of which the daily catch by the earth alone must be reckoned in thousands of millions, the authors estimate that at least two thousand tons of meteoric matter fall into the sun per second. A short digression on the evolutionary history of the stars follows. It is known that the radiation of heat and light from the sun is equivalent to a diminution of four million tons per second in its mass; in rich nebulous regions such as Orion and the Pleiades, the accession of mass to a star, by the infall of meteors (assuming that the "nebulous" matter is mainly meteoric in character) might well counterbalance or possibly exceed the loss of mass by radiation, with the result that in such regions the stars may very well grow in mass and "youth" for long periods of time, thus reversing or suspending what is believed to be the normal process of evolution.

The constitution of comets is mainly meteoric, and the characteristic radiation of these objects is composed chiefly of emission bands of cyanogen, carbon-monoxide, and the Swan spectrum. Meteors falling into a star would, when first volatilised, give rise to an absorption spectrum; in particular, the stellar spectra ought to show the cyanogen absorption bands. But such bands would be displaced considerably to the red owing to the Doppler effect; in the case of the sun the parabolic velocity at the solar surface is about 600 kilometres per second, and this figure may be taken to be as the order of magnitude for the velocity of meteors at or near the surfaces of the stars in general. The actual Doppler displacement will of course depend on the stellar mass and radius (or rather the distance from the centre at which absorption becomes effective). Four years ago the absorption bands of cyanogen were detected in the spectra of several early type stars, stars so hot that a band spectrum of molecular origin, such as is found in the spectra of the cooler stars, would seem at first sight to be outside the bounds of possibility. It is believed by the authors that this spectroscopic information can be interpreted by the perishing of meteors in the environs of the stellar surfaces. Further spectroscopic evidence is adduced; in several O, B, and A type stars, in which the cyanogen absorption is strongest, some forty ultimate and low excitation lines of iron and magnesium (two of the chief constituents of meteors) in the region from 0·356μ to 0·388μ are in evidence as a diffuse band; owing to the high temperatures of these stars such lines cannot of course be a reversing layer phenomenon. It should be mentioned that the stellar spectra considered have been

analysed by a self-registering microphotometer; the features of the spectra already mentioned cannot in general be detected by visual examination. The authors conclude with some general observations, of which one may be mentioned. If the phenomena are correctly interpreted as due to the falling of meteors into the stars, the displacement of the head of the cyanogen band, for example, will give the maximum velocity of the in-falling meteors, which will yield a relation between the masses and the radii of the stars concerned.

A publication that ought to prove of great benefit to astronomers engaged in numerical computations dealing with planetary orbits has just appeared from the Astronomischen Rechen-Institut at Berlin-Dahlem. It is Publication No. 46 from that institute, and is entitled " Tafeln der elliptischen Coordinatem $C = \frac{r}{a} \cos v$ und $S = \frac{r}{a} \sin v$ für Exzentrizitäts-winkel von $0°$ bis $25°$." It has been compiled under the direction of Dr. G. Stracke.

Acta Astronomica Ser. A, Vol. I (of Cracow Observatory) contains the reduction by Dr. Eugene Rybka of 204 occultations of stars by the moon observed at the Observatories of Cracow, Warsaw, and Lwow during the period 1901–22. The material is intended for a systematic investigation of the moon's motion, and has been worked up as far as the equations of condition. From additional observations it is hoped to make later a complete solution. The author points out the importance of the limit irregularities of the moon, and urges a plea for additional information to supplement Hayn's hypsometrical maps of the moon which, based on photographic observations between 1908 and 1912, are hardly sufficient for the problem in hand.

In the *Astronomical Journal No.* 903 (June 2, 1928), Professor K. Hirayama contributes a " Note on an explanation of the gaps in the asteroidal orbits." The conclusion with which the author joins issue is that reached by Dr. A. Wilkens, whose work was published in *Sitzungsberichte d. Bayer. Akad.* (1927). Wilkens had considered an idealised dynamical problem: he assumed that Jupiter's orbit is circular, and that initially an asteroid is moving in a circular orbit with a mean motion exactly twice that of Jupiter. At the beginning, then, the asteroid is in the middle of what is known as the 2/1 gap. The variation of the asteroidal orbit has been followed by Wilkens step by step by the laborious method of mechanical integration for eighteen revolutions of Jupiter. This computation shows that both the mean motion and eccentricity increase gradually, and attain the following maximum values: $n = 609''$; $e = 0{\cdot}1046$. Wilkens' conclusion is that the asteroid,

originally in the centre of the 2/1 gap, is removed by Jovian perturbations to its border, and remains near the border without returning to its original position. This is the conclusion from which Hirayama dissents. His argument is based on this : the variations of the mean motion and the eccentricity are governed by the values of the sine of the critical argument $(l - 2l' + w)$, and hence, if the sign of the argument is changed, the variations become opposite in sense ; the asteroid must then return to or near its original position. Hirayama asserts his conviction that if Wilkens had continued his mechanical integration up to thirty-four revolutions of Jupiter he would have completely altered his conclusions.

In the July number of *Science Progress* (1928) it was stated, with reference to the Lick Observatory Catalogue of Radial Velocities, that " not less than 15,000 spectrograms were taken and measured." Dr. Aitken writes me to say that I have omitted to mention the 10,300 spectrograms taken at the Lick station in Santiago, thus failing to give a correct impression of the immense work contributed by the Lick astronomers to this important branch of astronomy.

PHYSICS. By L. F. Bates, B.Sc., Ph.D., F.Inst.P., University College, London.

Important New Experiments in Radiation.—In his address to the South Indian Science Association (*Ind. Journ. of Phys.*, **2**, p. 1, 1928), C. V. Raman gave an interesting account of experiments, carried out at Calcutta, which established the existence of a new type of radiation. For some years, Raman has devoted considerable attention to the phenomena of the scattering of light by transparent media, and the first evidence of the new radiation was obtained when Raman and Seshagiri Rao, on examining the light scattered transversely by distilled water, found that the polarisation of the scattered light decreased very markedly when a violet filter was placed in the path of the incident light. The same effect was observed with ethyl and methyl alcohol, and it was further noted that the colours of the light scattered from the various liquids did not exactly match. The observations were carried a stage further by Ramanathan (*Proc. Ind. Assoc. Sci.*, **8**, p. 190, 1923), who concluded that " some slight fluorescence " accompanied the scattering of the light, and that this fluorescence was not due to the presence of chemical impurity in the transparent medium. Following these experiments, Venkateswaran found that when ultra-violet light fell upon pure dry glycerine, the scattered light was strongly polarised. Raman concluded that these results pointed to the existence of a phenomenon which he

described as "the optical analogue of the Compton effect observed with scattered X-rays." This description was somewhat unfortunate; however, the whole subject was re-examined in the light of this conception, and some eighty liquids were examined in order to determine whether the phenomenon was universal. In these experiments a powerful beam of sunlight from a heliostat was concentrated by a telescope objective combined with a short focus lens, and passed through a blue violet filter into a bulb of carefully purified and dust-free liquid. The scattered light was viewed through a filter of green glass complementary to the first filter, and in every case the track of the beam of light in the liquid was visible. The phenomenon thus observed and that of ordinary fluorescence were immediately found to differ in two ways. Firstly, the intensity of the secondary radiation was of an entirely different order, and, secondly, the radiation transmitted by the complementary filter was often polarised almost as strongly as light scattered in the familiar manner, whereas ordinary fluorescent light is usually unpolarised. The phenomenon was also observed with the gases CO_2 and N_2O, with transparent crystals, such as ice, and with amorphous solids. An observation of the utmost importance was that when the incident light consisted of definite sharp lines, the scattered light contained the lines present in the incident beam, together with new lines which were quite sharp. With some liquids a continuous spectrum was also observed.

The explanation of the phenomenon given by Raman was that the new or modified radiation was produced in the following manner. A quantum of the incident radiation was partly absorbed by a molecule, which was thereby raised to a higher energy state, whilst the remainder of the incident energy reappeared as a quantum of scattered radiation of lower frequency than the incident radiation. Consequently, the difference in frequency of the incident and modified radiations should correspond exactly to a definite characteristic frequency of vibration of the molecule. Thus, for every line in the spectrum of the incident radiation, there should be present in the scattered radiation a set of modified lines, the number of modified lines in each set being equal to the number of characteristic frequencies exhibited by the molecule. An examination of the scattered radiation from benzene, toluene, pentane, ether, methyl alcohol and water by Raman and Krishnan (*Ind. Journ. of Phys.*, **2**, p. 399, 1928) showed that these conditions were satisfied. Particular attention was devoted by them to the examination of the light scattered by pure benzene. The source of light was a 3,000-c.p. mercury vapour lamp in quartz, and the light was concentrated on to a

bulb of clear, non-fluorescent glass containing the dust-free liquid, which had been carefully purified by low temperature distillation *in vacuo*. The measurements proved conclusively that each line in the spectrum of the incident light generated its own modified lines in the scattered light, independently of any other lines present. The modified lines were also found in definite positions with respect to the incident line which produced them. The relative intensities of the lines in a given set of modified lines did not appear to depend much on the frequency of the incident line. The most striking result, however, was that whilst most of the modified lines were of lower frequency than the incident line, some modified lines of higher frequency were also recorded. These, of course, must have been produced by the incidence of a quantum of radiation upon a molecule already raised to a higher energy level than the normal, so that the molecule was able to return to its normal state, the energy thus available being utilised in the emission of a quantum of modified radiation of higher frequency than the incident radiation. In the case of benzene, moreover, the frequency intervals of the modified lines with respect to the incident line were found to agree with the characteristic frequencies of the benzene molecule found by infra-red determinations. This was naturally a most important point, for it showed that the phenomenon was eminently suitable to form the basis of a simple method for the accurate measurement of the characteristic frequencies of molecules of transparent substances.

R. W. Wood (*Nature*, **122**, p. 349, 1928) has verified Raman's observations, using an improved apparatus which made it possible to photograph the strongest lines in a few minutes. He found " anti-Stokes " lines—i.e. lines of higher frequency than the incident lines—of intensity nearly equal to that of the modified lines of lower frequency in the case of chloroform and carbon tetrachloride. The latter gave a triplet on both sides of the 4046, 4358, and 5461 lines of mercury. Crystalline quartz exhibited a strong modified line corresponding to the absorption band at 20μ, and a faint modified line corresponding to an infra-red absorption at about 75μ was found. So far, Wood has not found a line corresponding to the more familiar quartz absorption band at $8 \cdot 5\mu$, possibly because such a line would require comparatively large energy changes, and these are less probable than small energy changes. Many of the modified lines discovered by Raman were actually found to be double, when examined with Wood's improved apparatus, and certain lines were distinctly banded, with a sharp, intense, long wave edge and shaded off towards the short wave side. In a letter to *Nature* (**122**, p. 398, 1928) Saha, Kothari, and

Toshniwal have pointed out that the theory of modified radiation accounts for the phenomena of resonance spectra of vapours of sodium, potassium, and certain halogens, described by R. W. Wood. The latter found that when these vapours were illuminated with monochromatic light, a partially polarised spectrum, containing the original line and a number of fine lines spaced at equal frequency intervals on both sides of it, was emitted in a direction at right angles to the incident beam. The frequency interval in the case of sodium was 145. Now, Pringsheim and his collaborators have shown that Na_2 molecules can exist in sodium vapour, and give rise to a characteristic band spectrum in which one of the most pronounced frequency intervals is 145, which may be identified with a characteristic frequency of vibration of the component atoms. When, therefore, the incident light traverses the vapour, such molecules give rise to modified lines corresponding to the energy difference between the excited and the normal states of the Na_2 molecule.

It is therefore obvious that Raman's discovery has opened up a very wide field of investigation, and the progress of these researches will be watched with the greatest interest by all physicists.

[Since the above account was written further interesting papers on the Raman effect by Pringsheim and Rosen (*Zeit. für Phys.*, **50**, p. 741, 1928) and Bleeker (*Zeit. für Phys.*, **50**, p. 781, 1928) have appeared. R. W. Wood (*Phil. Mag*, **6**, p. 638, 1928) has also described the efficient optical arrangements which he used to study the phenomena.]

BIOCHEMISTRY. By R. Keith Cannan, M.Sc., University College, London.

The Physiological Action of Drugs.—There have been many attempts to establish the quantitative relations between the concentration of a drug and the magnitude of the physiological response. In general, it has been found that the latter followed either a linear or a logarithmic function of the former. To explain the linear relation no ingenious theory is necessary, but to comprehend the logarithmic function it has been necessary to assume that individual cells differ in the threshold concentration of drug to which they respond. Alternatively, it has been suggested that the relation is due to the varying accessibility of the different parts of the tissue. Now both these explanations make the implication that the action of the drug is an irreversible process. The activity of many drugs, however, is known to be quantitatively reversible. For some of these A. J. Clark and his colleagues have been able to establish an interesting relation. Clark (*J. Physiol.*, 1926, **61**, 530)

first studied the action of acetyl choline upon the isolated ventricle and upon the rectus abdominus of the frog. Working over a wide range of concentration, he found that, in both cases, the relation of the response to the concentration of drug was given by

$$Kx = \frac{y}{a-y},$$

where K is a constant, x is the concentration of acetyl choline, a is the maximum response which can be evoked, and y is the fraction of a which is given by experimental values of x. Clark points out that the results of Gaddum (*J. Physiol.*, 1926, **61**, 141) on the activity of ergotamine and of adrenalin on the uterus of the rabbit, conform to this equation in a fairly satisfactory manner. More recently D. Wilkie (*J. Pharm. Exp. Thera.*, 1928, **34**, 1) has continued these investigations. He finds that the same relation holds for the isotonic and isometric responses of strips of sheep's carotid artery, for the rate of outflow of blood from the frog's aorta and for the rise in blood-pressure in the decerebrate cat—all under the influence of adrenalin. The physiological convenience of such a relation is pointed out. At the lower ranges of concentration of a drug or hormone—where the responses elicited may be described as physiological—the magnitude of the reaction will be approximately proportional to the concentration. As the latter increases, however, the sensitivity of the tissue to changes in concentration will progressively diminish. Clark suggests that the simplest explanation for the relation which has been found is that the reaction which governs the response is a reversible monomolecular process. Clark also points out that the actual amount of acetyl choline which became " fixed " to the tissue was very small indeed. He calculates that a demonstrable response was elicited when only some 20,000 molecules of the drug were fixed to each individual cell. This amount was much too small to give even a monomolecular layer over the cell surface.

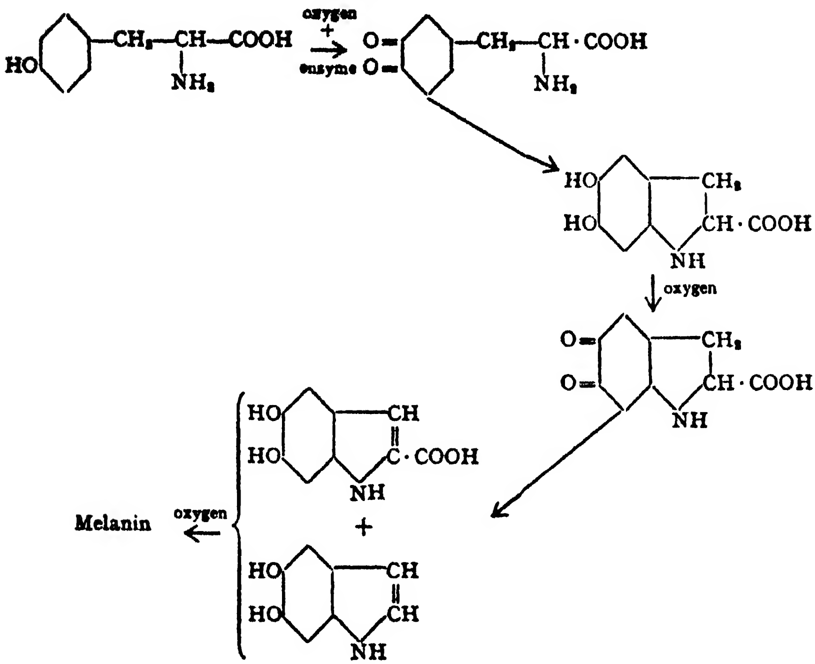
Unsaturated Hydrocarbons in Animal Fats.—Some account has recently been given in this review of the progress so far made in attempts to isolate vitamin A from cod-liver oil. Although the separation and identification of this nutritive factor have not yet been achieved, the endeavours have been of great interest. They have, for example, stimulated interest in the chemical composition of that ill-defined fraction of natural fats which is known as the unsaponifiable fraction. Complex unsaturated alcohols and hydrocarbons have figured prominently in recent investigations of this material. Ten years ago Tsujimoto (*J. Ind. Eng. Chem.*, 1916, **8**, 889, and

1920, **12**, 63) separated from fish-liver oil a substance which he identified as $C_{30}H_{50}$ and to which he gave the name squalene. Independently, Chapman (*J. Chem. Soc.*, 1917, **111**, 56; 1918, **113**, 458; and 1923, **123**, 769) isolated a similar substance which he called spinacene, but which is probably identical with squalene. While Drummond, Channon, and Coward (*Biochem. J.*, 1925, **19**, 1047) were fractionating the unsaponifiable matter of cod-liver oil in the quest for vitamin A they found as much as 8 per cent. of this hydrocarbon present in it. More recently, Channon and Marrian (*Biochem. J.*, 1926, **20**, 400) found a hydrocarbon allied to squalene, but not identical with it, in the liver oils of a number of mammals. In an attempt to arrive at some idea of the physiological significance of such substances, Channon (*Biochem. J.*, 1928, **22**, 51) fed squalene to rats. He found that partial absorption occurred and that there was a rise in the amount of unsaponifiable matter in the liver fats of the experimental animals. Some of this was due to accumulation of squalene, but the greater part was due to an increase in the amount of cholesterol. This observation naturally led to the plausible suggestion that the precursor of cholesterol in the animal body may be squalene or some related structure. In this connection we are reminded of the fact that the same observer found that rats fed upon a diet free from cholesterol were able to synthesise it within their bodies (*Biochem. J.*, 1925, **19**, 753). Cholesterol is one of those universal constituents of living tissues about whose origin, purpose, and destiny we have practically no information. The suggestions which have arisen as the result of the above investigations are, therefore, highly significant.

The Chemistry of the Tyrosinase Reaction.—Tyrosinase has figured long and prominently in discussions of enzyme activity and of biological oxidation. Its part in the production of that widely distributed group of natural pigments—the melanins—has long been apprehended, but there have been no data upon which to base a rational theory of the chemical changes involved in the action of the enzyme on tyrosine. Even the chemical nature of melanin was little understood. This substance—or group of substances—is found in hair, horn, feathers, retinæ, sun-burn, the skin of the pigmented races, and in melanotic tumours. It is a black colloidal material containing about 8 per cent. of nitrogen. Now, as a result of an extended series of investigations in the laboratory of Raper we are able to acknowledge important contributions to these questions. The probable structural changes involved in the conversion of tyrosine into melanin have been determined with some precision, leading, thereby, to a better understanding of the chemical nature of the latter substance.

Tyrosinase is most active between p_H6 and 8. The first visible effect of its action on tyrosine is the formation of a "red substance" which then is slowly converted into melanin. Raper and Wormall (*Biochem. J.*, 1923, **17**, 454, and 1925, **19**, 84) observed that the rate of change of this intermediate into the black precipitate was a function of the p_H being most rapid at the greater alkalinities. Taking advantage of the relative stability of the "red substance" in the more acid solutions they were able to separate it from the enzyme. They then found that its solutions underwent spontaneous decolorisation, even in the absence of oxygen. When air was readmitted to the colourless solutions, the black precipitate slowly formed. It was evident that the enzyme was necessary only for the early stages of the reaction. At that time, the view of Chodat and others was widely held that deamination of tyrosine accompanied the reaction. Happold and Raper (*Biochem. J.*, 1925, **19**, 92) showed, however, that no ammonia was formed and produced other evidence that deamination did not, in fact, occur. A big step forward in the problem was made when Raper isolated from the decolorised solutions of the "red substance" three compounds which proved to be 3-4-dihydroxyphenylalanine, 5-6-dihydroxyindole, and 5-6-dihydroxyindole carboxylic acid. Raper and Oxford (*J. Chem. Soc.*, 1927, 417) synthesised the two latter indole derivatives and demonstrated their identity with the products previously isolated.

The substance 3-4-dihydroxyphenylalanine was found to form melanin with tyrosinase even more readily than does tyrosine and produced a red substance as an intermediate. There seems little doubt that the first change which is initiated by tyrosinase is the formation of the ortho-quinone of tyrosine. This conclusion is fortified by the observation that the enzyme also converts cresol and phenol into their respective o-quinones. Assembling all this and other evidence, Raper (*Biochem. J.*, 1926, **21**, 89; and *Fermentforschung*, 1927, **9**, 206) proposes the following scheme for the action of tyrosinase. The ortho-quinone is first produced. This is the only change for which the enzyme is indispensable. This substance then undergoes spontaneous tautomeric change to 3-4-dihydroxydihydroindole carboxylic acid. The latter then oxidises to the quinone which is supposed to be identical with the "red substance." This undergoes further tautomeric change to 5-6-dihydroxyindole carboxylic acid, and some of it, by loss of carbon dioxide, to 5-6-dihydroxyindole. The two latter compounds may safely be assumed to be readily autoxidisable. It is believed that by oxidation and extensive condensation they pass into melanin.



The Acid-base Relations of Proteins.—The reactions of proteins toward changes in the hydrogen-ion concentration of their solutions have been variously interpreted according as the chemical or colloidal character of the systems was the subject of emphasis. To-day, however, it is generally agreed that their reversible reactions with acids and bases are stochiometric and conform to the quantitative relations of electrolytic dissociation. Qualitative explanations in the language of colloid chemistry become superfluous once it is conceded that this is so, *i.e.* that simple ions freely permeate the protein micelle and establish equilibria with all the dissociating groups present. Biochemistry owes a great debt to J. Loeb for the striking experimental arguments by which he first popularised this conclusion. More recent work has but refined the proof and increased the precision of experimental technique. The acid- and base-binding capacities may, most adequately, be defined by means of hydrogen electrode titration curves. The certainty of the method is limited only by the idiosyncrasies of electrodes in the presence of colloidal material and the difficulty of obtaining reproducible preparations of a protein. It must be remembered that there are no simple analytical criteria of the purity of a protein. Within these limitations, however, there is no doubt that the electrometric data establish the stochiometric reaction of proteins with

dilute acids and bases and their quantitative reversibility. A fairly recent review of this subject is that of E. Cohn (*Physiol. Rev.*, 1925, **5**, 349).

If, then, proteins are to be regarded as weak electrolytes, the implications of recent developments in the theory of electrolytic dissociation must be followed up. It is well known that the anomalous behaviour of strong electrolytes has led to the replacement of the molecular concentration terms in the equilibrium constants of ionic equilibria by thermodynamic activities. It is agreed that the activity coefficient of an ionic species is a function of the total ionic strength of the solution, and the most adequate description of this relation is the familiar Debye-Hückel equation. In view of the wide importance of the effects of electrolyte concentration on protein behaviour an extension of the activity concept to embrace proteins is eminently desirable. The difficulty has been that no comprehensive treatment of weak electrolytes in general has yet been made. Several attempts have nevertheless been made to determine the activity coefficients of proteins (Cohn, *loc. cit.*; Sorensen, Lang and Lund, *J. Gen. Physiol.*, 1928, **8**, 543; and Stadie and Hawes, *J. Biol. Chem.*, 1927, **74**, 31). More recently Simms (*J. Physical Chem.*, 1928, **32**, 1121 and 1495) has carried out a detailed investigation of the question of the extent to which the activity coefficients of weak polyvalent electrolytes may be predicted from the Debye-Hückel equation. He discusses, in particular, the simple amino-acids. In a further paper (*J. Gen. Physiol.*, 1928, **1**, 613) the inquiry is extended to the case of the protein gelatin. It is not possible to summarise his argument here beyond the general statement that the result of the application of the activity theory is highly promising. His general conclusion may, however, be quoted. "The data indicate that gelatin is a weak polyvalent ampholyte having distant groups and that the molecule has an arborescent structure with interstices permeated by molecules of the solvent and of other solutes. The size and shape probably vary with p_H."

In discussing the relation of proteins with acids and bases a matter of some formal importance deserves mention. The titration curves of the simple amino-acids exhibit a group dissociating in acid solution and another in alkaline solution. Since these substances form salts with acids only in acid solution and salts with metals only in alkaline solution it has become the custom to regard the group with a dissociation constant in the acid range of p_H as the amino-group and that with a constant in the alkaline range as the carboxyl. It followed that an amino-acid was present substantially as an undissociated molecule in the p_H range around neutrality. It was

generally overlooked that this allocation of dissociation constants made both the carboxylic and the amino-groups only about 10^{-7} of the strength of the corresponding groups in the simple carboxylic acids and amines. Adams (1917) was the first to suggest that the usual allocation should be reversed. More recently, Bjerrum (*Z. physikal. Chem.*, 1923, **104**, 147) has advocated the same view, emphasising the advantage of regarding the amino-acid in neutral solution as in the state of a "zwitterion" rather than of an undissociated molecule. There can be no doubt that the views of Bjerrum lead to a much more rational treatment of the general behaviour of these substances in solution. For example, for the determination of carboxyl groups in amino-acids, advantage has long been taken of the fact that alcohol and formaldehyde move the second dissociation constant of amino-acids some $3 p_H$ units in the direction of greater hydrogen-ion concentration. Now, according to the old convention, this constant was that of the carboxyl group. That is to say, alcohol and formaldehyde should have been described as increasing the strength of carboxyl groups. We know quite well, however, that their effect is to suppress the basicity of the amino-groups. This and other facts fit comfortably into Bjerrum's treatment of the acid-base relations of the amino-acids, and this reorientation of the theory of their electrolytic dissociation should certainly be generally adopted.

As a matter of fact, for many purposes, the treatment of ampholytes is greatly simplified by the new definition of acids and bases which we owe to Brönsted (*Rec. trav. chim.*, 1923, **42**, 718). This definition may be briefly put in the form

acid = base + proton (hydrogen ion).

An acid is a molecular species which is capable of dissociating a hydrogen ion. A base is a structure capable of accepting a hydrogen ion.* Undissociated carboxyl groups and the cations of amino groups are, thus, both to be regarded as acids, while carboxyl anions and undissociated amino groups are to be described as bases. This description has many theoretical and practical advantages.

One important aspect of the acid-base relations of proteins has been the light which their study has thrown upon the structure of the protein molecule in aqueous solution. According to the simple polypeptide theory of E. Fischer, a protein consists of long peptide chains formed by the union of amino-acids through their α-amino and α-carboxyl groups. Such a structure will, therefore, be an ampholyte. If monamino monocarboxylic acids alone are present the resulting molecule

will be monobasic and monacidic. In view, however, of the huge size of the average protein molecule the actual equivalent combining capacities will be so small that negligible amounts of acid and base will be bound. If diamino and dicarboxylic acids are also present, however, each of these will contribute a free basic or acidic group to the polypeptide chain, unless branching of the chain or ring formation occurs. Under such simple conditions, then, the acid and base-binding capacities of the protein should be equal to the numbers of equivalents of dibasic and of dicarboxylic acids respectively which are contained in the molecule. In other words, the acid-combining capacity should equal the number of molecules of lysine, arginine and histidine present and the base-combining capacity should equal the number of molecules of aspartic glutamic and hydroxyglutamic acids. Comparison of the titration data for a number of proteins with the amino-acid content (based upon the products of complete hydrolysis) has been made by Cohn (*loc. cit.*), with results which point quite definitely to the conclusion that the simple polypeptide theory does, in fact, account for the acid-base relations with a fair degree of accuracy.

Quite recently, the argument has been carried a step further by Simms (*J. Gen. Physiol.*, 1928, **11**, 629). Upon the assumption that the free groups in a protein have dissociation constants of the same order as those which they exhibit in the uncombined amino-acids to which they belong, he has determined the respective numbers of equivalents of these groups which must be present to give the experimental titration curve of the protein. The case of gelatin may be quoted in brief detail.

Groups.	Approx. p_k.	Equivalents present. Titration.	Analysis.	Difference.
Dicarboxylic acids	3·5	1·65	1·75	+ 0·1
Histidine	6·1	0·15	0·15	0·0
Arginine	8·1	1·2	0·2	− 1·0
Lysine	10·6	1·0	0·9	− 0·1
Unknown basic group	4·6	0·0	1·0	+ 1·0

The equivalents quoted are those for 2,500 gm. of gelatin. With the exception of arginine the agreement between the numbers of equivalents found by titration and the numbers of equivalents of the various tervalent amino acids which analysis has shown to be present is quite satisfactory. The titration data, however, indicate that there is very little of any basic group with a dissociation constant similar to that of the guanidine group in arginine. There is found, instead, an exactly equivalent amount of a weaker basic group with a p_k of about 4·6. Simms suggests that arginine exists in protein chiefly in a tautomeric form. When we remember the

anomalous character of the guanidine group in many of its derivatives we have small cause to quarrel with this decision. Simms also analysed the titration curves of edestin and egg-albumin and found the same general agreement and the same specific anomaly in the case of arginine.

A short account was given in this review, a year ago, of the arguments of those schools—particularly that of E. Abderhalden—which do not favour the simple polypeptide theory of protein structure. In their view, diketopiperazine or other ring systems are prominent features in the architecture of the protein molecule. The evidence assembled was acknowledged to be interesting in detail and telling as a massed argument, but was, at the same time, open to serious criticism. It was based entirely upon qualitative analytical methods which involved very drastic treatment of the protein, and it was open to question whether the results provided any useful information on the question of the nature of the protein molecule in aqueous solution. From the biological point of view it is the behaviour of protein in solution and its reactions to acids and bases which are the important questions, and it is therefore significant that the results which have been here discussed make it difficult to believe that any considerable proportion of the "third" groups of the tervalent amino acids can be bound in peptide links in those natural proteins investigated. If this is so, there does not seem to be any room for extensive branching or ring formation of the polypeptide chain unless some other form of linkage be brought into function. Of such there is no consistent evidence, though the presence of a variety of alternative forms of union of amino acids has been suggested from time to time. The criticism of all of these is that they have not led anywhere.[1]

One case may be referred to. Various workers have reported discrepancies between the numbers of carboxyl and amino groups liberated during digestion of a protein by pepsin. The methods employed have been the Van Slyke method of determining amino groups on the basis of their reaction with nitrous acid and various modifications of the alcohol or formalin titrations of carboxyl groups in amino acids. The question has recently been revived by Steudel and Ellinghaus (*Z. physiol. Chem.*, 1927, **166**, 84; see also Steudel, Ellinghaus, and Gottschalk, *ibid.*, 1926, **154**, 21 and 198), who found a variable

[1] After this review had gone to press a highly important article upon the structure of proteins was published by H. B. Vickery and T. B. Osborne (*Physiol. Review*, 1928, **8**, 393). After a detailed review of various hypotheses they conclude that "the peptide theory, in spite of shortcomings, is still the foundation-stone of protein chemistry. The future development of protein structure theory may be most confidently looked for in the direction of the expansion of the peptide hypothesis."

excess of carboxyl groups set free over the amino groups simultaneously liberated in the digestion of a variety of proteins by pepsin. This result naturally led to the suggestion that there were present, in the original protein, linkages other than those between a-amino and a-carboxyl groups of the constituent amino acids. The experimental results are, however, challenged by a number of other laboratories. Sorensen and Linderstrøm-Lang (*C.R Carlsberg. Lab.*, 1927, **17**, No. 4, 1, also Sorensen and Katschioni-Walther, *Z. physiol. Chem.*, 1928, **174**, 251), found the ratio $\frac{COOH}{NH_2}$ to be consistently unity when the methods of determination were rigidly controlled. They discuss the many causes of error when the methods above referred to are applied to the analysis of proteins without special modifications. Several other authors report results in agreement with those of Sorensen (Waldschmidt-Leitz, Schaffner and Grassman, *Z. physiol. Chem.*, 1926, **156**, 68; Waldschmidt-Leitz and Simons, *ibid.*, 1926, **156**, 99; and Felix and Harteneck, *ibid.*, 1927, **165**, 103). In this connection mention may be made of a recent electrometric study of the progress of the peptic digestion of gelatin carried out in the writer's laboratory. The increases in acid- and base-binding capacities were found to be equal at all stages of the digestion. Moreover, the approximate p_k values of the liberated groups were of an order to suggest strongly that these were a-amino and a-carboxyl groups of the constituent amino-acids.

PHYSICAL CHEMISTRY. By R. K. SCHOFIELD, M.A., Ph.D., Rothamsted Experimental Station, Harpenden.

Chemical Kinetics in Homogeneous Systems—Gaseous Reactions.—Those who have persevered in the study of reaction velocity in gaseous systems are to be congratulated on having at length obtained results the interpretation of which seems reasonably clear. On account of the numerous cases in which traces of "impurities" and the walls of the reaction vessel were found to have a catalytic effect, the very existence of a true homogeneous gaseous reaction was at one time held to be doubtful. The substantial agreement of the results of a number of investigators who had made independent studies of the decomposition of nitrogen pentoxide (cf. SCIENCE PROGRESS, 1926, **21**, 35) effectively dispelled this doubt.

More recently Rice and Getz (*Physic. Chem.*, 1927, **31**, 1572) have made a very complete study of this reaction. They found the velocity constant of this unimolecular decomposition to be unaffected by the presence (or absence) of water vapour, nitric acid or dust particles, and to be independent of the

mode of preparation of the nitrogen pentoxide. Hibben (*Proc. Nat. Acad. Sci.*, 1927, **13**, 626) finds this constant to have the same value from 0·2 to 0·002 mm. pressure as was found at normal pressures by previous workers, and so fails to confirm the observation made by Hirst and Rideal that k increases at very low pressures.

It will be recalled that the theory of F. A. Lindermann (*Faraday Soc. Trans.*, 1922, **17**, 598) requires a *decrease* in k at sufficiently low pressures. On this theory the molecules of a gas which reacts at a rate proportional to the first power of its concentration are so constituted that, after receiving a critical increment of energy through an unusually violent collision, they must pass through some internal phase before reaction occurs. Except at very low pressures the majority of these activated molecules lose their extra energy by a second collision before the phase is complete. Thus although activated molecules are produced at a rate proportional to the square of the concentration, the proportion that survive long enough to complete the internal phase and react will vary inversely as the concentration, *i.e.* directly as the average time that elapses between the molecular encounters, provided that this time is short compared with the time lag between activation and reaction. When the pressure is too low for this condition to be fulfilled, the reaction should no longer be unimolecular; and the ratio of the reaction velocity to the concentration should fall off with decreasing pressure. It is of great interest, therefore, to learn that several reactions have now been found to show the effect predicted by Lindermann. The decomposition of gaseous propaldehyde (Hinshelwood and Thompson, *Proc. Roy. Soc.* A., 1926, **113**, 221, and Hinshelwood and Askey, *ibid.*, 1927, **116**, 163) follows the unimolecular law at high pressures, but the ratio falls off more rapidly when the pressure is reduced below 80 mm. A similar behaviour is found in the decomposition of dimethyl ether (Hinshelwood and Askey, *ibid.*, 1927, **115**, 215) and of diethyl ether (Hinshelwood, *ibid.*, 1927, **114**, 84), the rate falling off below 300 mm. in the former case and below 150 mm. in the latter. It was found with all three substances that, on adding sufficient hydrogen, the velocity coefficient does not fall off with decreasing partial pressure of the reactants. As the absolute value of the coefficient at higher concentrations is unaffected by the presence of the hydrogen, the effect cannot be chemical, and it is reasonable to conclude with Hinshelwood that the hydrogen acts merely by keeping up the Maxwell distribution of energy among the molecules of the reacting gas, when the supply of activated molecules would otherwise begin to fall short of that required to keep the coefficient constant. It is noteworthy,

however, that the action of hydrogen seems to be specific; neither helium nor nitrogen has the same effect.

The discovery by Hinshelwood and his co-workers of a number of cases of unimolecular decomposition in the gaseous state makes it clear for the first time that these substances (*viz.* N_2O_5, $CH_3.CO.CH_3$, $C_2H_5.CO.C_2H_5$, $C_2H_5.CHO$) have molecules which are large in comparison with those of the substances which give a bimolecular constant (*viz.* HI, Cl_2O, N_2O). Thus the suggestion arising out of Lindermann's theory, that the former require a longer time between activation and reaction, is seen to be reasonable in view of their more complex structure. The fact that complexity of structure is the essential factor in bringing about unimolecular decomposition is confirmed by a consideration of the absolute value of the reaction velocity. Christiansen (*Proc. Camb. Phil. Soc.*, 1926, **23**, 438) showed that in five out of the six known examples of bimolecular gas reactions the rate is given by the expression

$$\text{number of collisions} \times e^{-E/RT}$$

where E is the energy of activation. In the case of unimolecular reactions, it has been shown (G. N. Lewis and D. E. Smith, *J.A.C.S.*, 1925, **47**, 1508, and Fowler and Rideal, *Proc. Roy. Soc.* A., 1927, **113**, 570) that it is necessary to consider the internal degrees of freedom of the molecule as contributing towards the energy of activation in order to account for a rate many times larger than that given by the above expression, such as is found in these cases. In this way it has been calculated that four modes of internal vibration may be involved on the decomposition of ether and six in that of propaldehyde.

These researches suggest that, for the type of reaction under consideration, unimolecular and bimolecular rates are to be expected at either ends of a continuous range. The appearance of the former type is favoured by complexity of molecular structure and high pressure, the latter by simplicity of structure and low pressures.

Acidic and Basic Catalysis.—A totally distinct class includes such classical reactions as the inversion of cane sugar and ester hydrolysis. These reactions which occur in solution are greatly influenced in rate by the presence of acidic and basic catalysts. But in common with the above class they proceed at a perfectly reproduceable rate. Considerable advance has recently been made towards a better understanding of these reactions. The original idea of Arrhenius and Ostwald, based on the electrolytic dissociation theory that $H^{\cdot}$ and OH' ions are the sole catalysts in these reactions, and that the velocity is proportional to their concentration, has long been proved

inadequate. Neither the use of activities in place of concentrations, nor the dual theory in its cruder forms has succeeded in removing the difficulties. The new method of approach, which is mainly due to Brönsted, is to recognise that the net action that occurs to the substrate is either the acceptance or loss of a proton ($H^{\cdot}$) or its transference from one position in the molecule to another. Hence in the presence of molecules which are ready acceptors of protons and of others that are ready donors the prototropic change is facilitated, and the reaction catalysed. In acid catalysis by a strong acid the molecule H_2O can accept a proton and become $H_3O^{\cdot}$ (a hydrogen ion in solution), while $H_3O^{\cdot}$ can give up a proton becoming H_2O. Similarly with a strong base, OH' can accept a proton and become H_2O, while H_2O can lose one and become OH'. Looked at from this standpoint, however, it is at once evident that other molecules besides the above will be catalytically active; all proton donors and all proton acceptors will be more or less active in this respect. It is found, for instance (Brönsted and Pederson, *Z. physik. Chem.*, 1925, **115**, 337), that nitramide decomposes slowly in aqueous solution at a rate which is increased by the presence of OH'-ions. An acceleration of the reaction is also brought about by the addition of acetate ions, which being anions of a weak acid readily combine with protons to form undissociated acetic acid. A similar effect is brought about by other such anions, and also by amines (which take up protons forming kations). It is thus to be expected that the velocity constant, k, will steadily increase as the readiness with which the catalyst takes up a proton increases, or as the corresponding dissociation constant, K, decreases. Brönsted (*Faraday Soc. Trans.*, forthcoming) finds $d\log k/d\log K = -0{\cdot}8$ is in good agreement with observations on nitramide, both with neutral molecules and ions with one and two negative charges. This reaction is catalysed only by proton donors, and not at all by proton acceptors. Substances are also known for which the reverse is true.

Brönsted and Guggenheim (*J.A.C.S.*, 1927, **49**, 2554) and Lowry and Smith (*J.C.S.*, 1927, p. 2539) have studied the mutarotation of glucose. This reaction, which involves the transference of a proton from one place in the molecule to another, is catalysed by both proton donors and proton acceptors. The reaction is most rapid when a ready donor and a ready acceptor are both present; but since water can act in either capacity (though somewhat reluctantly) some measure of catalysis is effected if only one be added to an aqueous solution of glucose. The dual character of such catalysis is strikingly brought by Lowry and Faulkner (*J.C.S.*, 1925, **127**, 2884), who studied the mutarotation of tetramethyl glucose

in non-aqueous solvents. Neither pyridine nor cresol alone is a complete catalyst, although pyridine, being a ionising solvent, should, on Lapworth's theory, have been very effective. A mixture of the two is, however, a far better catalyst than water.

As with nitramide, Brönsted (*loc. cit.*) has studied the quantitative relationships between the dissociation constants of weak acids and bases and their catalytic effect upon the glucose reaction, and finds the same type of correlation. His remarkable success in this field has in great measure been due to his elucidation of the "kinetic salt effect," which is superimposed upon the main catalytic effect already discussed. In many cases this effect, which Brönsted considers to be due to the change in the medium in which the reaction occurs, bears a linear relationship to the concentration. It would appear that failure to separate the true catalytic effect from the kinetic salt effect has prevented the many other workers in this field from making a satisfactory interpretation of their results.

Reactions between Ions.—The question as to whether activities should be used in reaction velocity equations in place of concentrations is still an open one. The theory of Brönsted that reaction velocity depends on the concentration of a critical complex in equilibrium with the reactants can be put in some measure to the test when the primary reaction is between ions. For in this case since the reactants and the critical complex will have different electric charges, their activity coefficients will be affected to different extents by changes in the ionic strengths brought about by the addition of salts. Hence for a given concentration of the reacting ions, the concentration of the critical complex, and hence the reaction velocity, should vary with the ionic strength in a manner that can be calculated, in the case of very dilute solutions, by applying the theory of Debye and Hückel. Brönsted and Livingston (*J.A.C.S.*, 1927, **49**, 435) have studied the reactions

$$[CoBr(NH_3)_5]^{\cdot\cdot} + OH' \rightarrow [Co(OH)(NH_3)_5]^{\cdot\cdot} + Br'$$

and $$2[CoBr(NH_3)_5]^{\cdot\cdot} + Hg^{\cdot\cdot} + 2H_2O \rightarrow 2[Co(NH_3)_5H_2O]^{\cdot\cdot} + HgBr.$$

Both are bimolecular, the rate of the second being determined by the process

$$[CoBr(NH_3)_5]^{\cdot\cdot} + Hg^{\cdot\cdot} \rightarrow.$$

The slope of the graph obtained by plotting the logarithm of the velocity coefficient against the square-root of the ionic strength is equal to the product of the valency of the primary reacting ions (− 2 and + 4) as the theory demands. A number of investigations have yielded results which enable Brönsted's

theory to be tested, and they show a general agreement with specific effects superimposed.

It is, perhaps, as well to emphasise that all the reactions that have been considered are of the class that proceed at a controllable and reproduceable velocity. They are not accelerated by traces of "impurities," nor are they similarly inhibited. They stand in marked contrast to the large class of "uncontrollable" reactions which are now generally believed to be brought about by a chain mechanism.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., Ph.D., University, Glasgow.

Volcanology.—A new book on volcanoes by Dr. K. Sapper, the famous German authority, has appeared (*Vulkankunde*, Stuttgart, 1927, 424 pp.). The writer has not yet seen this work, but a full critique over the initials of Dr. C. N. Fenner has been published in the *Journal of Geology* (xxvi, 1928, pp. 185–6).

Volcanologists will also find Th. Thoroddsen's massive posthumously published work on the *History of Icelandic Volcanoes* ("Die Geschichte der Isländischen Vulkane," *Mem. Acad. Roy. Sci., Lett. Danemark*, Sect. Sci., 8me. Sér., t. 9, 1925, 458 pp.) a rich mine of information. There are also included long sections on the physico-geological relations and geographical distribution of the hot springs of Iceland, and on Icelandic earthquakes between 1013 and 1908.

Dr. Niels Nielsen has described the volcanic phenomena of the little-visited region of the Hvítárvatn and Hofsjökull in the interior desert of Iceland (*Med. Dansk Geol. Fören.*, Bd. 7, Heft 2, 1927, pp. 101–28).

A valuable study of the characters and classification of pyroclastic rocks has been published by Dr. H. Williams (*Proc. Liverpool Geol. Soc.*, xiv, 1926, pp. 223–48). He restricts the term *agglomerate* to contemporaneously formed, coarse, pyroclastics consisting mainly of large, rounded or subangular fragments. The term *volcanic breccia* is applied to similar material of angular shapes, also including under this class the non-contemporaneous breccias due to dry avalanches within a crater (*e.g.* Vesuvius), and breccias due to the erosion and reassortment of coarse angular volcanic ejecta. Williams also defines lapilli and tuff, and distinguishes between *cognate* pyroclastic material, consisting of lava fragments derived from contemporaneous activity ; *accessory cognate* material, derived from previously consolidated lavas erupted in the same cycle of igneous activity ; and *accidental* material, consisting of foreign igneous, sedimentary, or metamorphic fragments derived from the country rocks.

In a paper on "A Further Contribution to the Petrography

of the Late-Palæozoic Igneous Suite of the West of Scotland" (*Trans. Geol. Soc. Glasgow*, xviii, pt. 2, 1928, pp. 259–94) the writer of these notes extends Williams's nomenclature with the term *intratelluric* to indicate pyroclastic material derived from great depth (*subvolcanic* of Dr. H. S. Washington, see SCIENCE PROGRESS, April 1928, p. 585); and while in agreement with the great majority of Williams's terms he thinks that the distinction between rounded and subangular, and angular fragments, is not significant enough to warrant more than one name for coarse ejected material which is the immediate product of explosive volcanic activity, namely, agglomerate. In discussing the Permian volcanic necks of the West of Scotland, the writer points out that the fragmental fillings of volcanic vents may be derived as follows: (1) explosive disintegration of the molten lava filling a volcanic funnel or crater, giving rise to cognate material; (2) material derived from the walls of the vent, or from a solidified infilling, by explosive disintegration, or by subsequent avalanching within the crater. A distinction may be made between accessory cognate and accidental materials; (3) fragmental material derived from great depth—intratelluric. As examples we may cite the ultrabasic rock fragments in the Carboniferous vents of the Midland Valley of Scotland, and the eclogites, etc., of the Kimberley diamond pipes.

Dr. H. Williams has also described a unique type of volcanic flow product near Lassen Peak (Cal.), which he calls a torrential volcanic breccia (*Univ. Cal. Publ., Bull. Dept. Geol.*, **17**, No. 7, 1928, pp. 241–63). This flow, forming a tract of country called the Chaos Jumbles, combines many of the features of dry rock streams with those of true mud flows. Its age is estimated at about 200 years. The material consists of blocks of dacite derived from Péléan spines which were disrupted by explosion, and hurled, avalanche fashion, on to an earlier bed of fine tuff in the presence of steam. The condensation of the steam as rain converted the tuff into mud which behaved as a lubricant between the coarser blocks.

An important paper by C. K. Wentworth deals with the pyroclastic geology of Oahu, one of the Hawaiian islands (*Bernice P. Bishop Mus.*, Bull, 30, 1926, 121 pp.). The geology, physiography, and sedimentology of the tuff craters are discussed, twenty separate volcanic centres are mapped, and thirty episodes distinguished. The tuff, which is estimated to make up 90 per cent. of the combined masses of the craters, consists of magmatic ejecta, mingled with detrital limestone, reef rock, and ancient basalts. Less than one-hundredth of 1 per cent. is basalt extruded during the eruptions. The petrography of the pyroclastics and the associated basalts is

fully dealt with. The author concludes that the last eruption on Oahu occurred before the advent of the native Polynesian, but probably not more than 5,000 years ago.

The great basalt floods of the world are believed by most geologists to have been emitted from fissures with a minimum of explosive action. Hence the discovery of a series of vents filled with fragmental materials in the Columbia River basalts at Asotin on the Washington and Idaho boundary by Dr. R. E. Fuller (*Journ. Geol.*, xxxvi, 1928, pp. 56–74) is of considerable interest. He offers evidence, however, that this explosive activity was due to the contact in depth of basaltic fissure eruptions with water-saturated gravels, producing palagonite and pumice, with a partial volatilisation of the iron content of the material.

An interesting summary of the work of the Geophysical Laboratory on hot springs, by Dr. E. T. Allen ; an illuminating statement of the present position of the volcano gas problem, by Dr. E. S. Shepherd ; and papers by Dr. E. G. Zies on the concentration of metallic constituents by volcanic emanations, and by Dr. F. E. Wright on factors bearing on the formation of the lunar craters, were given at the 1927 meeting of the Section of Volcanology, American Geophysical Union (*Nat. Research Council*, Bull. 61, 1927, pp. 255–69). Dr. Shepherd dismisses volcanic gas reactions, and engulfment of oxygen-carrying debris, as insufficient to supply the heat necessary to maintain the Kilauean lava lake in action, and even suggests that the classic equation between ferrous oxide and water may now be permanently retired as the expression of any significant heat reaction in lava. Nearly all the suggested heat reactions involve the rapid movement of large quantities of gas through lava, which is improbable. Dr. Shepherd at present believes that the necessary amount of heat could be easily supplied by the rise of a few cubic metres of lava per second, with only a 200° drop in temperature.

Another fine volcanological work has been issued by the Carnegie Institute of Washington in the memoir on "Steam Wells and Other Thermal Activity at 'The Geysers,' California" (*Carn. Inst. Wash.*, Publ. No. 378, 1927, 106 pp.), by E. T. Allen and A. L. Day. A great store of hot steam, increasing with depth, has been revealed at the "Geysers," and economic exploitation is in progress. That this steam rises in a region of deficient ground water, is superheated, and is accompanied by volcanic gases, is good evidence of its magmatic origin. The volcanic gases and their oxidation products are potent causes of the superficial rock decomposition which is so pronounced in this locality. The causes, and the relation between the acid and alkaline hot springs, are dealt with to some extent.

The concentration of the acid springs, as found also at Lassen Peak, is very much greater than that of the alkaline springs. It has been found possible to connect acid springs with places where oxidation is active, and alkaline springs with places where oxidation is feeble.

Geology of Igneous Rocks.—In a valuable discussion and analysis of recent theories regarding the genesis of the alkaline rocks Prof. C. H. Smyth (*Proc. Amer. Phil. Soc.*, **66**, 1927, pp. 535–80) concludes that the development of alkaline magmas has been largely effected through the agency of the primary volatile constituents. He further maintains that, in so far as the development of alkaline magmas is influenced by tectonic conditions, the controlling factor is stability, as contrasted with orogenic activity. He believes that the assimilation of limestone, or of other rocks, is not essential to, or even important in, the development of alkaline magmas.

The problem of the origin of the alkaline rocks has also been attacked by J. L. Gillson (*Journ. Geol.*, xxxvi, 1928, pp. 471–4), who suggests that the process of albitisation, known to be a common feature in granitic intrusives, may lead to the formation of alkaline rocks. This process implies the passage of emanations rich in soda and alumina through the magma, which may thereby have been desilicated and enriched in soda and alumina to the point of the formation of nepheline. Fenner has also suggested that differentiation may arise by the passage of fugitive constituents through a liquid or partially liquid magma. Such a process as stated above would naturally lead to the production of deuteric minerals such as sodalite, cancrinite, ægirine, etc., which are found in many alkaline rocks.

The typical lavas of the Javanese volcanoes are pyroxene-andesites and basalts of calc-alkaline affinities. Prof. H. A. Brouwer, however, has shown (*Journ. Geol.*, xxxvi, 1928, pp. 545–8; *Proc. Kon. Akad. Wetens. Amsterdam*, xxxi, 1928, pp. 492–8) that, in the products of the volcano Merapi, trachyte, phonolite, and leucitic phanerites have been produced by the reaction of pyroxene-andesite lava on a large xenolith of limestone. Prof. Brouwer suggests that similar reactions on a larger scale may have contributed to the production of the sporadic alkaline rocks of the East Indian Archipelago.

Turjaite is a remarkable coarse-grained igneous rock, first described (1921) by Ramsay from Turja in the Kola Peninsula, consisting of melilite, nepheline, biotite, apatite, with accessory perovskite and titanomagnetite. Dr. E. H. Kranck has now fully described the very abundant and comprehensive collections from Turja made by Ramsay and other Finnish geologists (*Fennia*, **51**, 1928, No. 5, 104 pp.). The turjaite forms part of a great complex of highly alkaline rocks, including rocks of the

ijolite series, and dyke rocks rich in carbonate minerals. The melilite in turjaite has originated from pyroxene, and sometimes from olivine, by the action of residual solutions or melts rich in lime which had penetrated into a partially consolidated pyroxene-rich rock of the ijolite or lamprophyre class, but not by Bowen's intramagmatic reactions. The carbonate-silicate rocks are believed to have crystallised from residual magmas supersaturated with water and carbon dioxide, the temperature of crystallisation corresponding closely to that of the pneumatolytic and hydrothermal phases of the other igneous rocks of the complex.

In a paper on "The Magmatic Origin of Ores," Dr. R. H. Rastall (*Geol. Mag.*, lxv, 1928, pp. 270–9) restates his view that a very large number of vein deposits and replacements are of magmatic origin, and that the term "magmatic" should be interpreted in the broad sense which includes not only the ore derivatives of relatively dry magma, but also those which are the natural and inevitable consequence of the escape of volatile constituents. He sees no reason for denying the term magmatic to those processes such as tourmalinisation, greisening, kaolinisation, and topazisation, which are usually classed as pneumatolytic, or to such later deuteric processes as sericitisation, propylitisation, albitisation, etc. "They are due to agents derived from the intrusion itself, segregated during the process of its crystallisation, and many of them are initiated, even if not completed, long before the final solidification."

The explanation of corundum-plagioclase (plumasite) and albitite rocks as due to the desilication of ordinary pegmatites by contact and exchange with the peridotites and pyroxenites with which they are invariably found, is combated by E. S. Larsen in a paper on "A Hydrothermal Origin of Corundum and Albitite Bodies" (*Econ. Geol.*, xxiii, 1928, pp. 398–433). He points out that reactions between pegmatites and wall-rocks are very exceptional; that in the rock-types discussed the minerals and the relations of the reaction-zones are not such as would be expected between pegmatite and ultrabasic rocks; that the material taken from the pegmatite is excessive and has little similarity to that added in the reaction zones; and finally that there is a conspicuous lack of relation between the sizes of the dykes and the widths of the respective reaction-zones. For these and other reasons Larsen prefers to explain the origin of plumasite and albitite dykes, with the reaction-zones, as due to moving hydrothermal solutions.

In a paper on "Chlorophæite, Sideromelane, and Palagonite from the Columbia River Plateau," M. A. Peacock and R. E. Fuller (*Amer. Min.*, 13, 1928, pp. 360–82) state that these three mineraloids are characteristic of Kainozoic

basaltic fields. From the existing analyses of chlorophæite, together with a new analysis of the Oregon material, the approximate formula $(Fe,Al)_2O_3 . 2(Mg,Fe,Ca)O . 4SiO_2 + 10H_2O$ is derived. It is urged that the definition of this mineraloid be extended to cover all hydrous, amorphous, pitch-like materials of deuteric origin, some of which have been called palagonite in dolerites and basalts. Sideromelane remains as a specific name for ideal basaltic glass formed under conditions of especially rapid cooling. Palagonite is a gel derived from sideromelane by the action of water or water vapour of exotic origin. Thus there is both a chemical and genetic distinction between palagonite and chlorophæite.

Describing the geology and petrology of the remarkable group of tholeiite and dolerite sills of the Dalmahoy Syncline west of Edinburgh (*Trans. Roy. Soc. Edin.*, lv, pt. 2, 1927, pp. 489–505), R. Campbell and J. W. Lunn note the occurrence of iron-rich olivine, primary quartz, and the abundance of chlorophæite in these basic rocks. The order of crystallisation is also noteworthy. As in the case of basalts from Franz Josef Land described by Newton and Teall, it has led to the formation of a mother-liquor poor in silica and alumina, and rich in iron. The first mineral to crystallise was an intermediate plagioclase, followed by augite. Only towards the end of the period of pyroxene growth does magnetite make its appearance. The last of the early constituents to crystallise was fayalite. The late concentration of iron is seen again in the unusual abundance of magnetite and hæmatite in the mesostasis of the tholeiites and dolerites, accompanied by skeletal fayalite in the dolerite-pegmatites, and by fayalite in the ground mass of the basalts. But the most striking evidence of the late concentration of iron is seen in the chlorophæite which, in the opinion of the authors, represents the final, highly differentiated, aqueous mother-liquor of the rocks.

Dr. A. Brammall has concisely discussed the composition and intrusion-tectonics of the Dartmoor Granite (*Proc. Geol. Assoc.*, xxxvii, 1926, pp. 251–77). This paper is the first in which the methods of H. Cloos have been applied to a British granite mass. The sequence of events in the emplacement of the Dartmoor Granite is long and complicated, but the main flow appears to have spread northward, and extended laterally beneath a cover of country rock which had already suffered overthrusting from the S.S.E., and was traversed by a zone of crustal weakness trending in a direction N. of E. The upper contact surface was therefore domed and grooved in various directions, and had been plastered with early basic microgranite and granite-porphyry before the advent of the main intrusion.

In another paper Dr. Brammall shows that the Dartmoor Granite contains primary gold and silver (*Min. Mag.*, xxi, 1926, pp. 14–20). Visible gold occurs in both quartz and felspar, and both gold and silver in biotite. Assays show that both metals occur in apparently barren granite. The closest examination of vein-quartz, and assays of vein-pyrites, joint-incrustations, and pneumatolysed rocks, afforded no grounds for attributing the gold-silver content of the granite to secondary hydrothermal or pneumatolytic processes.

A paper by Dr. Brammall and Dr. H. F. Harwood (*Min. Mag.*, xxi, 1927, pp. 205–20) discusses the temperature-range of formation of tourmaline, rutile, brookite, and anatase in the Dartmoor Granite.

A further paper by Dr. Brammall deals with the detrital minerals of Dartmoor as a " study in provenance " (*Proc. Geol. Assoc.*, xxxix, 1928, pp. 27–48). The three most distinctive features of the heavy mineral assemblage are : (1) the association of manganiferous garnet, titaniferous biotite, zoned zircon, monazite, and ilmenite, with tourmaline, cassiterite, brookite, and anatase ; (2) ilmenite in excess of magnetite though sphene and rutile are rare ; (3) the rarity of topaz and fluorite.

In a study of Cornish granite and its relation to the occurrence of tin and other metallic ores, E. H. Davison (*Trans. Roy. Geol. Soc., Cornwall*, xv, pt. 8, 1927, pp. 16, reprint) provides a good general account of the Cornish granites, and shows that the granite in highly mineralised areas always contains much tourmaline, and white mica, but little or no biotite, whereas in granite from areas devoid of lodes tourmaline and muscovite are rare, while biotite occurs in normal abundance. It seems, therefore, that the mineralising agents, while introducing tourmaline and white mica into the granite, have effected the more or less complete destruction of biotite.

C. W. Osman has made a very detailed study of the Scilly Isles granite mass (*Quart. Journ. Geol. Soc.*, lxxxiv, pt. 2, 1928, pp. 258–92). He shows that the positions of the granite laccoliths of Cornwall and Devon are directly related to the intersections of the ridges of three separate foldings : (1) Post-Silurian Caledonian folding ; (2) Post-Middle Coal Measures Malvernian folding ; and (3) Post-Carboniferous Armorican folding. When the two flanking masses of Dartmoor and the Scilly Isles are compared a remarkable parallelism in composition and intrusion-succession is disclosed. The Bodmin Moor and Scilly Isles granites are represented as having risen up a great fault or faults with the Caledonian trend ; while the remaining granites of Cornwall and Devon probably rose up along the Start Point-Lizard series of faults. Prof. H. Cloos's methods have been used by Mr. Osman to demonstrate the

mode of emplacement and directions of flow of the Scilly Isles Granite.

Dr. M. Billings's memoir on "The Petrology of the North Conway Quadrangle in the White Mountains of New Hampshire" (*Proc. Amer. Acad. Arts and Sciences*, **63**, 1928, pp. 67–137) is chiefly concerned with the great alkaline batholith of the White Mountains, the age of which is doubtfully referred to the Devonian. The igneous activity began with the establishment of several volcanic centres, giving rise to explosion-breccia and lava flows of comendite and trachyte. During the rise of the subsequent batholith the roof of volcanic rock was affected by cauldron subsidence in much the same way as in Glen Coe and other regions familiar in this country. The main batholith is composite, and covers an area of 680 sq. miles. It consists principally of biotite-granite, but there are also considerable masses of diorite, nordmarkite, riebeckite-granite, and hastingsite-granite. Finally Dr. Billings gives an excellent discussion of the mechanism of roof-collapse over the batholith.

The great Wiborg granite mass in S.E. Finland is one of the most extensive of uniform igneous masses that is known. It consists of a potassic granite with the thickly distributed ovoid felspar phenocrysts and aggregates to which the name "Rapakiwi" has been long applied. Prof. W. Wahl has published a detailed account of the petrography, contact-relations, and differentiation of the Wiborg granite, and a discussion of the origin of the rapakiwi structure (*Fennia*, **45**, No. 20, 1925, 127 pp.). In the true rapakiwi type the rounded phenocrysts consist of orthoclase surrounded by a ring of oligoclase.

The Wiborg granite is associated with relatively small bodies of hypersthene-granite, syenite, augite-diorite, and quartz-norite; and Prof. Wahl discusses the differentiation of this assemblage at length. In treating of the origin of the rapakiwi structure he points out that these granites always carry idiomorphic quartz, and that both quartz and felspars, often also the dark constituents, occur in two generations. The consolidation of the rapakiwi granites has therefore taken place in two stages. In brief Prof. Wahl ascribes the production of rapakiwi structure to the refusion of previously crystallised constituents in consequence of a single, but considerable, relief of pressure during the magmatic period.

Two further papers on Finnish rapakiwi granites have been published by P. Eskola ("On Rapakiwi Rocks from the Bottom of the Gulf of Bothnia," *Fennia*, **50**, No. 27, 1928, pp. 1–29) and by I. Kanerva ("Über das Rapakiwigebiet von Vehmaa im südwestlichen Finnland," *Fennia*, **50**, No. 40, pp.

1–25) respectively. Prof. Eskola shows that certain wide-spread boulders of red graphic granite on the eastern shores of the Gulf of Bothnia come from a rapakiwi granite intrusion which is now buried beneath the waters of the gulf. He discusses the origin of the graphic structure, and stresses the importance of the study of boulders for the geology of marine areas. Dr. Kanerva describes the petrography of the Vehmaa rapakiwi granite in south-western Finland, and discusses its intrusion-tectonics by application of the methods of Prof. H. Cloos.

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., University College, London.

The effect of anæsthetics in shortening the rest period of deciduous shrubs is well known, and recent investigations have shown that a variety of chemical vapours, and even warm water, effect this in varying degrees. Denny and Stanton, experimenting with several species and a variety of chemical vapours, obtained the maximum diminution of the rest period with ethylene chlorhydrin and ethylene dichlorid amounting to two months in the case of *Pyrus ioensis*. The chief interest of their experiments, however, is the demonstration of the localisation of the stimulus. Not only can the rest period be shortened for individual twigs upon a plant, but individual buds can be forced in this way, whilst other buds immediately adjoining remain dormant (*Amer. Jour. Bot.*, xv, pp. 327–44, 1928). These and results obtained by other workers suggest that the order of development of the successive layers in a woodland and of the organs on an individual may be associated with the marked vertical gradient in the external conditions in this type of habitat.

"The Biology of Photosynthesis" is the subject of an article by Lubimenko in the *Rev. des Sci. Naturelles*, pp. 415–47 and 486–512, 1928. He here emphasises the hereditary character of chlorophyll content and the relation between this feature and geographical range. Data are furnished for the chlorophyll contents of over six hundred species from Petrograd, the Crimea, and Java. These show that the chlorophyll content in general increases from the pole to the equator. The mean values obtained being 66° N. 2·36 gms. of chlorophyll per kilogramme of leaves; 45° N. 2·52 grms.; and 6° S. 2·66 gms. In the shade the same species shows increase in size of the plastids and an increased chlorophyll content of the plastids, though decrease in their number as compared with the sun plants. The amount of chlorophyll in one kilogramme of fresh leaves for sixteen species showed an increase

of from 9·3 to 129 per cent. (mean 50 per cent.) in the shade leaves as compared with the sun leaves.

In an extensive study of the ecology and sociology of *Sesleria cærulea* Zlatnik (*Études écologiques et sociologiques sur le S. cærulea . . . en Tchécoslavaquie,* Prague, 1928) finds that the range of reaction of the soil varies between *p*H 5·5 and 8·37, the $CaCO_3$ from zero to 57 per cent., but *S. cærulea* only becomes dominant where the soil is absorptively saturated with ions of either calcium or magnesium. In artificial cultures with varying hydrogen-ion concentration the growth showed a bimodal form both for *Sesleria cærulea* and the segregate *S. uliginosa.* For the former the best growth was at reactions above *p*H 6·5, but though there was marked depression at *p*H 6 and below *p*H 5 good growth occurred at *p*H 5·5. The ash of plants grown on calcareous soil (4·79 per cent.) differed little from that of plants grown on serpentine (4·85 per cent.), but whereas the former contained 19 per cent. CaO and 4·16 per cent. MgO, the latter contained 6·7 per cent. CaO, 10·8 per cent. MgO, and 10 per cent. more Silica. The conclusion reached is that *S. cærulea* var. *calcarea* is not a calcicole species, but an oxyphobe which is restricted to special types of soil by the pressure of competition. The subspecies *uliginosa* is similarly restricted by reaction and competition, but is associated with a relatively highwater-content of the soil.

Thoday and Pocock record the occurrence of *Myosurus,* probably *M. minimus,* from Cape Colony, apparently as a wild species. The very marked discontinuity which characterises the distribution of the genus as a whole, and this species in particular, renders this record of peculiar interest. In the southern hemisphere *Myosurus minimus* (as var. *australis*) was only known hitherto from Australia, though congeners occur in New Zealand and South America (*Trans. Roy. Soc. S.A.*, xvi, p. 23, 1928).

Very few ecological accounts of the Bryophytic communities of any area have been published, so that the ecological notes on the Bryophytes of Middlesex by P. W. M. Richards (*Jour. Ecology,* 1928, p. 269) are a welcome addition. Four strata of moss vegetation are distinguished in the woodlands, viz. (1) Xerophytic epiphytes on trees and branches, of which there are but five species due to smoke effects ; (2) the epiphytes of the tree-boles and stumps, numbering twenty-five species ; (3) Wood-floor species (62) ; and (4) Woodland bank vegetation (28 species). The Bryophytic flora of the hedgerows is shown to correspond to that of woodlands on the same type of soil, and evidence is afforded, by the persistence of Heath species, of the value of bryophytes as indicators of former conditions.

Stapledon and Davies report the results of some interesting

experiments on the effects of competition between clovers and grasses in various hay mixtures at the Welsh Plant Breeding Station (*S.H.*, No. 8, 1928). The importance of early growth in the competitive struggle is emphasised and is one of the chief factors in the aggressiveness of *Lolium italicum*. It is claimed that the evidence shows that overcrowding lowers both hay and seed yields and that adverse conditions tend to favour delayed germination of seeds. The percentage establishment is markedly decreased by crowding. Field trials would appear to support the conclusion that the yield of hay is higher where unlike species are concerned than where a single species or closely allied species are alone present. Thus for two years the average yield in pounds per rod was 196 when only a single species of clover was present, 197 with various species of grass only, with one species of clover and one grass 227, and with one clover and several grasses 222.

Ciesielski, in 1911, published the results of experiments that appeared to demonstrate the influence of the age of the pollen on the sex of the resultant offspring in Hemp. Subsequent experiments by Lilienfeld failed to confirm these results. Bessy has now carried out similar experiments with Hemp, and concludes that age of pollen has no influence on the sex of the offspring (*Amer. Jour. Bot.*, July 1928).

The occurrence of Mycorrhiza in the following species has been recorded by E. Mason (*N. Phytologist*, p. 193, 1928): *Plantago coronopus*, *Plantago maritima*, *Aster tripolium*, *Glaux maritima*, *Cochlearia officinalis*, *Agrostis alba*, and *Glyceria maritima*.

From a study of reciprocal hybrids between *Aquilegia vulgaris* and *A. chrysantha* Marja Skalinska (*Acta, Soc. Bot. Pol.*, vol. v, p. 141, 1928) finds that there is an absence of normal segregation attributable to the marked partial sterility of both the pollen and the ovules. An interesting outcome of these experiments is the different behaviour of the seeds of the various crosses as regards the manner of their germination. The latter extends over a period of about forty-eight days, some seeds not germinating, however, till the second year.

Elton and Summerhayes (*Jour. Ecol.*, 1928), dealing with the vegetation of Spitzbergen, recognise four vegetation zones characterised by decreasing severity of the climatic conditions. The first is the Feldmark, or arctic desert, in which a very open community consisting of 35 species of phanerogams is characterised by the presence of *Salix polaris* and *Luzula confusa*. The second zone in the fog belt consists mainly of *Dryas* Heath with a richer fauna and flora (69 species of phanerogams). Further inland, beyond the fog belt, occur heaths dominated by *Cassiope tetragona* with a still richer flora (91 species),

whilst a further increase of species is found in the inner Fjord zone, where in the better climatic conditions at the head of the Fjords 127 species of phanerogams occur and *Empetrum nigrum*, *Betula nana*, *Rubus chamæmorus*, and *Vaccinicum uliginosum* make their chief home. As one passes from the most severe to the most favourable climatic conditions the number of geophytes increases, and it is only in the last two zones that annual species are met with.

In a revision of the genera *Chantransia*, *Rhodochorton*, and *Acrochætium*, Drew describes nineteen new species (*Univ. California Publ.*, vol. 14, No. 5, 1928). The name *Chantransia* is discarded on the grounds of the diversity of types originally included and the species of Rhodophyceae placed here and in the genus *Acrochætium* are all referred to *Rhodochorton*. Of the new species six are epiphytic, ten are endophytic, one is epizoic on *Sertularia turgida*, one is endozooic in *Sertularia furcata*, and one is parasitic on *Eisenia arborea*. The largest species attains about 2 mm. in length and the smallest is microscopic. They constitute an interesting series of types exhibiting all stages of incipient parasitism in the endophytic species from *R. porphyræ*, which scarcely affects its host, to *R. elegans*, which caused the death of the host cells. In some the penetrating organs are but single cells (*R. arcuatum*), in others a richly branched system (*R. rhizoideum*), whilst the degree of development of the exterior branch system shows a similar range. The cells contain one or more chromatophores which may be a parietal band, either reticulate or stellate. In *R. rhizoideum* sp. nov. the chromatophore is a single band in the free filaments, but in the endophytic cells is either a single reticulate structure or there are several ribbon-shaped chromatophores.

PHYSIOLOGY. By Prof. Walter Stiles, Sc.D., F.R.S., The University, Reading.

Photosynthesis.—The publication this year of three more papers in the series of "Experimental Researches on Vegetable Assimilation and Respiration" under the general direction of F. F. Blackman brings the total number of these up to nineteen. The three new contributions all deal with photosynthesis. The first of them ("The Diurnal Rhythm of Assimilation in Leaves of Cherry Laurel at 'Limiting' Concentrations of Carbon Dioxide," *Proc. Roy. Soc.*, B, **102**, 467–87, 1928), by E. J. Maskell, records experimental work carried out with the object of determining how far the relations between environmental factors and photosynthetic activity can be explained in terms of limiting factors in the case of leaves of land plants. The observations were made for the most part

with leaves of cherry laurel, the method employed for measuring rate of photosynthesis being that described in 1904 by Miss Matthaei in which a current of gas containing a known amount of carbon dioxide is passed over a leaf and the amount absorbed found by determining the residual carbon dioxide in the gas after this leaves the leaf chamber. The results are given as values of apparent assimilation, that is, the actual assimilation measured without any allowance being made for respiration which may take place at the same time. It was found that with a high intensity of light and a comparatively low concentration of carbon dioxide, that is, conditions under which carbon dioxide concentration, on the theory of limiting factors, should determine the rate of photosynthesis, there occurred nevertheless a characteristic nocturnal depression in the rate between about 4 p.m. and 6 a.m., although the light intensity, temperature, and carbon dioxide concentration remained the same. It was found that with a lower light intensity the nocturnal depression occurred just the same. It was thought that the nocturnal depression must be due to an increase in the resistance offered to the diffusion of carbon dioxide into the leaf and experiments made to test this idea confirmed it. Thus, increasing the carbon dioxide concentration should result in an increase in photosynthesis during the nocturnal period ; this was found to be the case. It was found possible to increase the rate of photosynthesis by increasing the carbon dioxide concentration during the nocturnal period until the rate was limited by the light intensity. By slashing the leaf, and so reducing resistance to diffusion of carbon dioxide into the leaf, the rate of photosynthesis could also be increased. The conclusion is drawn that the observed variations in assimilation during the day are due to changes in the degree of stomatal opening. A seasonal variation in the general level of photosynthesis is attributable to the same cause.

In the second paper (E. J. Maskell, " The Relation between Stomatal Opening and Assimilation—A Critical Study of Assimilation Rates and Porometer Rates in Leaves of Cherry Laurel," *Proc. Roy. Soc.*, B, **102**, 488–533, 1928) the relation between stomatal opening and photosynthesis is examined experimentally by means of the porometer, measurements of stomatal aperture and photosynthesis being made in each experiment. It was found that stomatal opening exhibits diurnal and seasonal rhythms in photosynthetic activity. Calculation shows that the observed changes in stomatal opening are of about the order required to produce the observed changes in assimilation rate.

It is concluded therefore that for the cherry laurel leaf

there is not for any one external carbon dioxide concentration one definite rate of photosynthesis, nor can the rate of photosynthesis be defined by defining carbon dioxide concentration and light intensity. Variations in photosynthesis also occur on account of stomatal resistance; moreover, there may also be non-stomatal resistances to diffusion of carbon dioxide up to the chloroplast. The stomatal resistance depends on (1) the time of year, (2) the time of day, (3) the time that has elapsed from the commencement of an experiment, (4) the light intensity, (5) the previous history of the leaf, and (6) the previous moisture conditions of the plant. Non-stomatal resistance depends on (1) the time that has elapsed from the commencement of an experiment, (2) light intensity, and (3) moisture conditions of the leaf.

The paper concludes with a consideration of the interaction of limiting factors. It is pointed out that the form of the relation between carbon dioxide concentration and photosynthetic activity and between light intensity and photosynthetic activity, may be greatly modified by alterations in the resistance to diffusion, and this must be taken into account in considering these relations.

The third paper referred to above is by W. O. James and records the results of an investigation on water plants on similar lines to that made by Maskell with a land plant ("The Effect of Variations of Carbon Dioxide Supply upon the Rate of Assimilation of Submerged Water Plants," *Proc. Roy. Soc.*, B, **103**, 1–42, 1928). The water plant used was the moss *Fontinalis antipyretica*. A specially designed apparatus was employed in which a solution containing carbon dioxide flowed through the plant chamber. Instead of an aqueous solution of carbon dioxide a solution of sodium bicarbonate was used in some experiments. It was found that when the solution flowed through the leaf chamber at a rate of 400 c.cs. per hour a solution of sodium bicarbonate gave rise to a higher rate of assimilation than a pure solution of carbon dioxide of equal partial pressure, no other factor being limiting, but that with a flow of 600 c.cs. per hour the rate of assimilation increased when a pure solution of carbon dioxide was used, but not when a solution of bicarbonate was employed. This is explained on the ground that in the former case the diffusion resistance in the layer of liquid surrounding the plant is lessened by increasing the rate of flow. The action of the bicarbonate solution is in sharp contrast to this. In low intensities of illumination when, in consequence, the rate of assimilation is slow, the bicarbonate gives rise to about the same rate of photosynthesis as the highest obtainable with moving carbon dioxide solutions; in high light intensities it was not found

possible to obtain the same rate of photosynthesis with pure carbon dioxide solutions at any rate of flow as was given with bicarbonate solutions of equivalent concentration. This result is explained on the ground that the buffer action of the bicarbonate maintains more effectively the " close-up carbon dioxide supply " than any practicable movement of the solutions and reduces the external diffusion resistance to a low value. The curves obtained relating carbon dioxide concentration and rate of photosynthesis and those relating light intensity to rate of photosynthesis were all smooth curves of hyperbolic form. Previous workers had found that in lower concentrations of carbon dioxide the rate of photosynthesis is approximately proportional to the carbon dioxide concentration. This linear relationship is held by James to be due to the conditions of diffusion obtaining in the experiments rather than to the internal stages of photosynthesis.

Methods for studying photosynthesis have formed the subject of a few papers apart from those already mentioned. J. C. Waller (" The Katharometer as an Instrument for Measuring the Output and Intake of Carbon Dioxide by Leaves," *New Phytologist*, **25**, 109–18, 1928) gives some results obtained by the method he describes. B. D. Bolas (" Methods for the Study of Assimilation and Respiration in Closed Systems," *New Phytologist*, **25**, 127–44, 1926) describes several methods and decides in favour of a colorimetric one as most satisfactory. Lastly, an apparatus for measuring rate of photosynthesis in which a current of gas is drawn through a leaf chamber is described by P. Boysen Jensen (" Über neue Apparate zur Messung der Kohlensäureassimilation, der Respiration, der Öffnungsweite der Spaltöffnungen und der Beleuchtungsstärke," *Planta*, **6**, 456–72, 1928).

Translocation of Carbohydrates.—A problem related to photosynthesis is that of the removal from the leaf of the carbohydrates formed there. A contribution to this subject of the first importance has been made by T. G. Mason and E. J. Maskell in the form of two papers published under the general title of " Studies on the Transport of Carbohydrates in the Cotton Plant." The first of these is " A Study of Diurnal Variations in the Carbohydrates of Leaf, Bark, and Wood, and of the Effects of Ringing " (*Annals of Botany*, **42**, 189–253, 1928), the second is entitled " The Factors determining the Rate and the Direction of Movement of Sugars " (*Annals of Botany*, **42**, 571–636, 1928). The papers are also interesting as they are the first scientific contributions from the recently established Cotton Research Station in Trinidad and afford evidence of the excellent work that is to be expected from the Station and of the wisdom of those responsible for founding it.

The first paper records the results of a great number of determinations of total carbohydrate, reducing sugars, sucrose and reserve carbohydrates in the sap of the leaf and of the tissues (bark including phloem, and wood), which various workers have concluded to be the conducting channels. Determinations of the total carbohydrate content of the tissues of the stem were also made at comparatively short intervals of time. The results are expressed in terms of the residual dry weight, that is, the total dry weight less the weight of carbohydrate, which is thought to be a sounder basis than either dry weight or fresh weight. The variations in the amount of carbohydrates in the different tissues suggest that the carbohydrates are transported in the bark, that is, in the phloem. Thus the variations throughout the day in the concentration of total sugars in the leaf are more highly correlated with the variations of those in the bark than with those in the wood. Moreover, the variations in the bark show a lag behind those in the leaf. Again, variations in the rate of translocation of sugar into the boll, that is, the capsular fruit, are correlated with variations in the sugar gradient from bark to boll.

Further evidence in relation to the path traversed by carbohydrates was obtained by ringing experiments, that is, by experiments in which a ring of bark is removed from the stem at a certain level. When a stem is ringed below the foliage region an interruption in the passage of carbohydrates through the stem at the level of the ring occurs within a few hours of the time of ringing. The transport of carbohydrates from leaves into the stem immediately above the ring is not affected, nor is transport of carbohydrates out of the stem immediately below the ring. There is, however, an accumulation of carbohydrates in the bark and wood above the ring, and even in leaves two feet away from the ring. These observations all suggest that carbohydrates are conducted downward in the bark (phloem) and not in the wood. Other evidence in favour of the same conclusion is provided by the fact that the downward movement of a dye in the wood is not interrupted by ringing. If a piece of paraffined paper is inserted between bark and wood the transport of carbohydrates takes place at almost the normal rate. This indicates that the wood is not concerned in the transport as contact between wood and bark is not essential to the transport.

The work recorded in the second paper had for its purpose the obtaining of evidence for formulating the laws governing the translocation of carbohydrates. For this determinations were made of the amounts of carbohydrate moving through the stem under definite conditions, and of the variation of sugar concentration throughout the conducting tissues at different

times. It was found that the downward movement of sugars takes place down a well-marked sugar concentration gradient and that with reversal of the gradient the movement is also reversed. It was further concluded that sugars, particularly sucrose, in the sieve-tubes vary with variations in the supply of sugar to the bark, whereas variations in the sugar content of the parenchyma are very small. Thus the sieve-tubes appear to be the actual conducting cells. The most striking result is probably that relating to the velocity of movement of carbohydrate through the stem. Whereas the rate of movement of sugar from bark to wood appears not very much greater than what could be accounted for by physical diffusion, the movement down the sieve-tube is about 40,000 times as rapid as simple diffusion through a 2 per cent. solution of sucrose. Indeed, the calculated diffusion constant is almost identical with the diffusion constant for molecules of the size of those of sucrose diffusing in air. As is so frequently the case in scientific research, the solution of one problem has presented us with a still more striking one, for the physical difficulties in the way of accepting this result are tremendous.

ENTOMOLOGY. By H. F. Barnes, B.A., Ph.D., Rothamsted Experimental Station, Harpenden.

Notices of various recent books on entomology have appeared in book reviews.

General Entomology.—At present considerable attention is being paid to biological control; this has until lately been chiefly the control of insects by insects, but more recently the control of plants by insects has been developing. An idea of the lines on which research in this direction is based is given in a popular article by W. M. Davies (*N.W. Naturalist*, **3**, 1928, 22–5). In this connection the bionomics of *Apion ulicis* Först, a weevil whose grubs develop inside gorse pods and eat the seeds, has been studied by W. M. Davies (*Ann. Appl. Biol.*, **15**, 1928, 263–86). It seems possible, from a survey of the occurrence of this weevil in England, that it is a potent factor in preventing gorse areas in this country from spreading. The most striking successes with the method of biological control by the introduction of parasitic and predacious insects to keep down the numbers of foreign pests have been obtained in island areas (A. D. Imms, *Ann. Appl. Biol.*, **13**, 1926, 402–23). Doubt has existed whether the method could be used in continental areas. W. R. Thompson (*Parasitology*, **20**, 1928, 92–112) sets out the three main possible objections: the similarity of the faunas and floras of continental areas such as Europe and America, the polyphagous habits of many parasites,

and the behaviour of hyperparasites in relation to introduced primary parasites. The writer maintains that the introduction of native parasites and predators of foreign pests should be used even in continental areas and supports this view by showing that the factor, of all the complex of factors controlling indigenous insects, which has by its absence allowed increase and spread, is the parasitic factor. The mathematical theory of the action of parasites and the chance factor are dealt with by W. R. Thompson (*Ann. Fac. Sci.*, Marseille, **2**, 1924, 69–89). R. N. Chapman (*Jl. Econ. Ent.*, **21**, 1928, 349–52) states one may not assume that a change in insect population has been caused by a change in some environmental factor unless it is known that when the environment is constant the population is constant. He then compares results from constant population experiments with observations on the European corn-borer. When studying insect abundance, attention should be paid to the stage of growth at which the host has arrived, growth condition of the host as termed by C. R. Cutright and L. L. Huber (*Ann. Ent. Soc. America*, **21**, 1928, 147–53). This is particularly needed in carrying out experiments on the biology of insects, for example, testing out insects that have been suggested for weed control. Among other papers that are of interest in view of biological control, two may be mentioned. R. Melvin (*Biol. Bull.*, **55**, 1928, 133–42), in a study of the oxygen consumption of insect eggs, comes to the conclusions that the weight of egg-shell is an important factor, the effects of temperature are not so pronounced in the formative period as during the period of late incubation and the explanation for variation in length of formative period is the length of incubation. L. M. Peairs (*West Virginian Sta. Bull.* 208, 1927, 62 pp.) deals with some phases of the relation of temperature to the development of insects.

Improvements in the technique of breeding insects are continually being developed. Useful methods in breeding houseflies, leather beetles, weevils, cockroaches, and clothes moths throughout the year are to be found in a series of papers by A. G. Grady (*Jl. Econ. Ent.*, **21**, 1928, 598–612). A simple method for life-history studies of root-feeding arthropods is described by E. M. Searls (*Jl. Agric. Res.*, **36**, 1928, 639–45), in which the basis is glass tubes, with one end stopped by plaster of Paris, placed in sand.

The success of the effort to control the increase and spread of injurious insects depends very largely on the possibility of preventing any such spread. Therefore E. P. Felt's paper (*New York State Mus.*, Bull. 274, 1928, 59–129) is particularly interesting. He examines critically the evidence relating to the part winds play in the spread of insects, bringing together

older observations together with more recent knowledge obtained in aviation and pertinent meteorological investigations. It is interesting to note that Theobald in England records *Aphis rumicis* and *Anuraphis helichrysi* caught in an aeroplane at an altitude of 1,000–1,600 ft., and Felt in America records the capture of a *Mesogramma* and a *Hylemyia cilicrum* at an altitude of 3,000 ft. Further records relating to insect migration by C. B. Williams (*Trans. Ent. Soc. London*, **76**, 1928, 79–91) have appeared. Such records are essential for the study of insect spread.

Eight more fascicles of the *Insects of Samoa and other Samoan Terrestrial Arthropoda*, which is being published by the Trustees of the British Museum (Nat. Hist.), have appeared recently.

The grouping of the insect orders and their lines of development is the subject of a paper by G. C. Crampton (*Entomologist*, **61**, 1928, 82–5). The same author (*Canad. Ent.*, **60**, 1928, 129–41) deals with the evolution of Insects, Chilopods, Diplopods, Crustacea and other Arthropods indicated by a study of the head capsule. T. D. A. Cockerell (*Psyche*, **35**, 1928, 126–30) discusses the Jurassic insects of Turkestan.

E. P. Felt (*New York State Mus.*, Bull. 274, 1928, 3–58), in a paper entitled "Insects and Health," deals in a very lucid manner with those insects, for example, houseflies, mosquitoes, black flies, cockroaches, and lice, which are known to interfere with man's health. R. H. van Zwaluwenburg (*Bull. Expt. Sta. Hawaiian Sugar Planters' Assn., Ent. Series*, Bull. 20, 1928, 68 pp.) has made an exceedingly useful attempt to bring together all references to the subject of insect-nematode association up to 1927. Such well-known diseases as filariasis, hookworm, and trichinosis of human beings, and others of domestic animals and poultry are involved. The writer describes five types of association. Firstly, primary, *i.e.* causing death or sterilisation of host, *e.g.* bumble bees by *Sphærularia bombi*, orthopterons by *Gordius* and *Mermis*; secondly, secondary parasitism, *i.e.* with intermediate host, *e.g.* thorn-headed worm of swine in larval stages in Phyllophaga beetle larvæ, mosquitoes and filariasis; thirdly, mechanical association, internal, *e.g.* eggs of *Ascaris lumbricoides* through the tract of cockroaches and housefly; fourthly, mechanical association, external, e.g. *Rhabditis coarctata* on scarabeid beetles; and fifthly, commensalism in nests of ants, termites, beetles on grass and rubbish.

W. Fassnidge (*Ent. Rec.*, **40**, 1928, 70–3) gives notes on birds as enemies of mining insect larvæ. An interesting paper on insect scatology by S. W. Frost (*Ann. Ent. Soc. America*, **21**, 1928, 36–46) has appeared.

O. Shinoda has recently been working on the intestinal secretion of insects and two contributions have already appeared; the mid-intestinal secretion of Lepidoptera together with an appendix on the behaviour of mitochondria in the mid-intestinal epithelium of the silkworm (*Bombyx mori* L.) (*Mem. Coll. Sci. Kyoto Imperial Univ.*, **2**, 1926, 93–116), and a comparative study of the histo-cytology of the mid-intestine in various orders of insects (*Zeits. f. Zellfor. u. mikr. Anat.*, **5**, 1927, 278–92).

Orthoptera.—Three papers by V. B. Wigglesworth concerning the digestive enzymes of the cockroach have recently appeared. The first and second (*Biochem. Jl.*, **21**, 1927, 791–811) showed that the crop contents are normally rendered acid by the action of micro-organisms upon the carbohydrates in the food and that these enzymes are active over the same range of acidity, but in other respects resemble the corresponding enzymes from other sources. The third paper (*Biochem. Jl.*, **22**, 1928, 150–61) deals with the enzymes concerned in the digestion of proteins and fats.

R. K. Nabours and B. Snyder (*Genetics*, **13**, 1928, 126–32) have reported, in *Telmatettix aztecus*, that individuals that gave parthenogenetic progeny were all descended directly and exclusively from specimens captured in a small area in Texas and none of the progeny from California were mixed with them. This is interesting, since A. D. Peacock and J. W. H. Harrison (*Trans. Nat. Hist. Soc. Northumberland, Durham, and Newcastle-upon-Tyne*, N.S., **6**, 1925) put forward the hypothesis that parthenogenesis was consequent on hybridity. Of course, the possibility of hybridisation has not entirely been controlled by collecting from a single small area.

A. C. Davies (*Univ. California Publ. Entom.*, **4**, 1927, 159–208) discusses in detail the anatomy and histology of the alimentary canal of *Stenopelmatus fuscus* Hald. (*Tettigoniidæ*).

Coleoptera.—L. Daviault (*Ann. Soc. Ent. France*, **97**, 1928, 105–32) has made a thorough study of the post-embryonic development of *Acanthoscelides obtectus* Say. The biology of the difficult group of flea-beetles (*Phyllotreta*) attacking cultivated *Cruciferæ* has been worked out by H. C. F. Newton (*Jl. S.E. Agric. Coll.*, **25**, 1928, 90–115) and the larval forms of the blister beetles (*Meloidæ*) are described and discussed by A. Cros (*Ann. Soc. Ent. France*, **97**, 1928, 27–58).

R. G. Fisher (*Forestry*, **2**, 1928, 40–6) shows that elm trees with poor or deficient root systems are more liable to attack by bark beetles than trees with ample root systems and that a close relationship exists between the health and vigour of the tree and its liability to attack. The same writer (*loc. cit.*, 53–61), in a study of *Lyctus* attack, points out that, while variations in

moisture content of wood affect its liability to attack, how such changes bring about the requisite conditions in wood is still to be ascertained. With this end in view the food of *Lyctus* is studied and a possible symbiotic relationship existing between fungi and wood-destroying beetles of the families *Lyctidæ* and *Anobiidæ* are being studied.

Lepidoptera.—Systematic studies of the Japanese Crambinæ and Pyralinæ have been made by J. Shibuya (*Jl. Fac. Agric. Sapporo*, **21**, 1928, 121–76).

C. Dumont (*Ann. Soc. Ent. France*, **97**, 1928, 59–104) has made a valuable contribution to the knowledge of the effects of change of food, in this case carrot, upon caterpillars. Such a study as this is particularly important in connection with the biological control of weeds. The "Hopkins host-selection principle" is discussed by W. R. Thompson and H. L. Parker (*Bull. Ent. Res.*, **18**, 1928, 359–64) in relation to *Pyrausta nubilalis* Hübn., and to some extent finds support.

After studying the periodic vibratory movements of the larvæ of *Nymphala maculalis* Clemens, P. S. Welch and G. L. Sehon (*Ann. Soc. Ent. America*, **21**, 1928, 243–57) came to the conclusion that the principal if not sole function of these vibrating movements is to ensure the continual contact of the respiratory surfaces with a changing supply of amply oxygenated water.

Hemiptera.—A comparison of the sucking mouth-parts of the Hemiptera has been made by H. Weber (*Zeits. f. vergl. Physiologie*, **8**, 1928, 145–86) and deals with the larvæ and adults of the Psyllids and Aleurodids, and also the Coccids, Aphids, Cicads, Heteroptera, and *Corixa*. An important work on the classification of the higher groups and genera of the Coccid family Margarodidæ has been completed by H. Morrison (*U.S. Dept. Agric. Tech.* Bull. 52, 1928, 239 pp.). Enid K. Sikes (*Proc. Zool. Soc. London*, 1928, 269–305) has worked out the life-history of the Coccid bug (*Orthezia urticæ* Linn.) and also its external morphology.

The black scale, *Saissetia oleæ* Bernard, is a serious pest of the citrus and olive in California. It is heavily parasitised, about 51 primary and secondary parasites being known to attack it in various parts of the world. H. S. Smith and H. Compere (*Univ. California Publ. Entom.*, **4**, 1928, 231–334) have endeavoured to cement the foundations of the biological control of this scale by a detailed report on the parasites, their life-histories, habits, and ecological relationships.

In a paper dealing with the bionomics of the green capsid bug (*Lygas pabulinus*) F. R. Petherbridge and W. H. Thorpe (*Ann. Appl. Biol.*, **15**, 1928, 446–72) have traced a definite migration from woody plants such as currants, apples, roses,

to herbaceous hosts in the first generation and back to woody hosts in the second generation.

Hymenoptera.—Several important papers on the biology of various parasitic Hymenoptera have appeared recently. H. C. James (*Ann. Appl. Biol.*, **15**, 1928, 287–316) has done some valuable work on the life-histories of certain Cynipid parasites of Dipterous larvæ. In this paper the writer, in addition, describes some new larval forms and has an interesting section on early larval forms of parasitic Cynipids. R. N. Chrystal and J. G. Myers (*Bull. Ent. Res.*, **19**, 1928, 67–77) have studied two natural enemies, *Rhyssa persuasoria* L. and *Ibalia leucospoides* Hochenw., of *Sirex cyaneus* in England. H. E. Box (*Bull. Ent. Res.*, **18**, 1928, 365–70) describes the introduction of Braconid parasites of *Diatræa saccharalis* Fabr. into certain West Indian islands. The biology of *Dinocampus* (*Perilitus*) *rutilus* Nees, a Braconid parasite of *Sitona lineata* L. by Miss D. J. Jackson (*Proc. Zool. Soc. London*, 1928, 597–630), contains a useful review of the literature on the parasitism of Braconids of the genera *Dinocampus* and *Perilitus* which are usually parasites of adult beetles, but occasionally of the larvæ also. The life-history of *Exeristes roborator* Fab., a parasite of the European Corn Borer, has been studied by J. H. Fox (*Ottawa*, 1927, 58 pp.). An interesting fact arising from this study is that parthenogenesis may occur and unfertilised eggs hatch into males, thus a scarcity of males results in an increase of males in the immediate offspring.

M. R. Smith (*Ann. Ent. Soc. America*, **21**, 1928, 307–29) deals with the biology of *Tapinoma sessile* Say, an important house-infesting North American ant.

The second of G. D. Morison's series of papers on the muscles of the bee has appeared (*Q.J.M.S.*, **71**, 1928, 563–651). In this the muscles of the alimentary canal, heart, diaphragms, and reproductive organs, and the indirect muscles of the wings are described in gross and in histological detail. *Controlled Mating of Queen Bees*, by L. R. Watson (*published by American Bee Journal, Hamilton, Illinois*, 1927, 50 pp.), is a complete account of all the facts leading up to the discovery of the possibility of artificially inseminating queen bees. How important this is may be gathered when one states that now genetical research in line-breeding, hybridisation and general race improvement becomes possible. Unfortunately only skilled workers can successfully do this operation. An article summarising this account has appeared by the same writer (*Quart. Rev. Biol.*, **3**, 1928, 377–90). G. A. Rösch (*Zeits. f. vergl. Physiol.* **6**, 1927, 284–98) has made a valuable contribution to the knowledge of building among bees and the age of the builders. K. v. Frisch (*Aus den Leben der Bienen, Berlin*,

1927, x and 149 pp.) has brought out a very useful and well-illustrated small book on bees which summarises his own work and also Rösch's. It is a great pity that this book is not in English.

Diptera.—C. J. Wainwright's paper on British Tachinidæ (*Trans. Ent. Soc. London*, **76**, 1928, 139–254) has now appeared. This is an exceedingly valuable contribution to the systematics of this difficult group of flies. It seems a pity, however, that so little of the biology of these flies is mentioned. A useful contribution to the study of Dipterous parasites of the European earwig has been made by W. R. Thompson (*Parasitology*, **20**, 1928, 123–58) who gives an original account of the anatomy and biology of the two species, *Digonichæta setipennis* Fall. and *Rhacodineura antiqua* Meig., and summarises the known facts concerning their biology. H. E. Box (*Bull. Ent. Res.*, **19**, 1928, 1–6) deals with a Tachinid parasite, *Lixophaga diatrææ* Townsend of *Diatræa saccharalis* Fabr. H. J. Muller (*Genetics*, **13**, 1928, 279–357), in an important review paper on the measurement of gene mutation rate in *Drosophila*, shows how it has a high variability and depends on temperature. An hitherto undescribed fly, *Hydellia nasturtii* Collin, has been discovered in this country mining watercress stems by T. H. Taylor (*Ent. Mo. Mag.*, **64**, 1928, 126–8), and is described by J. E. Collin (*loc. cit.*, 128–9).

H. M. O. Lester and Ll. Lloyd (*Bull. Ent. Res.*, **19**, 1928, 39–60), in describing the process of digestion in tsetse-flies, discuss a powerful anticoagulin found in the salivary glands and a powerful coagulin found in the hinder part of the mesenteron which neutralises the anticoagulin, causing a rapid clotting, in order to retain the fluid meal while draining and assimilation take place.

Efforts have been made by M. T. Townsend (*Ann. Ent. Soc. America*, **21**, 1928, 121–32) to correlate the distribution of certain insects, including *Lucilia cæsar* L., with environmental factors. In general this species was found to be more abundant during periods of dry warm weather with a high percentage of sunshine, steady barometer and moderate wind. The seasonal frequency of Calliphorine blowflies in Great Britain is dealt with by R. A. Wardle (*Jour. Hyg. London*, **26**, 1927, 441–64).

An important paper on the pigmentation of the larvæ of *Simulium* by V. Voïnov (*Archiv. Zoologie Exper.*, **67**, 1928, 223–58) has appeared. H. F. Barnes deals with the genus *Lestodiplosis* of gall midges (*Ent. Mo. Mag.*, **64**, 1928, 68–75 and 142–8); the larvæ of these midges are predacious on other gall midge larvæ. Keys are given for the identification of the British species. P. P. Perfiliev has made studies on the anatomy of *Phlebotomus* adults (*Bull. Soc. Path. Exotique*,

21, 1928, 159–257) and their larvæ (*Centra. f. Bakt., Parasit. v. Infektion*, **107**, 1928, 296–305).

J. G. Myers (*Ent. Mo. Mag.*, **64**, 1928, 57–8) describes a peculiar feeding habit of *Culex pipiens* L., in which the females drink milk in a dairy from pans containing milk that had stood sufficiently long for cream to form on the top in preference to fresh milk. No cream was noticeable in dissected specimens. As the writer states, the conditions are strikingly analogous to those offered by the vertebrate host itself—a skin and a liquid beneath—and they are exploited apparently solely by the female insects.

Other Orders.—E. Percival and H. Whitehead have made (*Proc. Leeds Phil. Soc.*, **1**, 1926, 136–44) observations on the biology of the Mayfly, *Ephemera danica* Müll., and later (*Proc. Leeds Phil. Soc.*, **1**, 1928, 271–88) give further observations on the ova and oviposition of certain Ephemeroptera and Plecoptera. The ecological significance of the facts recorded forms an interesting summary to the latter paper.

Some results of J. V. Pearman's work on Psocoptera are now appearing; sound production is dealt with and a presumed stridulating organ is described (*Ent. Mo. Mag.*, **64**, 1928, 179–86), and biological observations on this group are described (*loc. cit.*, 209–18 and 239–40).

A. E. Emerson (*Bull. Amer. Mus. Nat. Hist.*, **57**, 1928, 401–574) has made a very valuable contribution to our knowledge of the termites of the Belgian Congo and the Cameroon.

H. Womersley (*Ent. Mo. Mag.*, **64**, 1928, 113–5), in further notes on British Protura, gives a key to the British species.

AGRICULTURE: ANIMAL NUTRITION.

By Herbert Ernest Woodman, M.A., Ph.D., D.Sc., School of Agriculture, Cambridge.

Evaluation of Feeding Stuffs for Milk and Fat Production in the Animal.—Three systems are in vogue at the present time among agriculturists for assessing the productive values of feeding stuffs: (1) The Kellner system of starch equivalents; (2) the Hansson system of food units; (3) the Armsby system of net energy values. In this country the German system of starch equivalents has formed for many years the basis of successful feeding practice, and only in quite recent times has the American method of measuring the nutritive value of a feeding stuff in terms of therms of energy begun to attract attention. The food unit system, although employed with conspicuous success in Sweden, Norway, Denmark, and Finland, has received but scanty recognition in this country, the probable reason for this being that most of the treatises dealing with this question have been published in Swedish, a language with which not many British scientists are familiar.

An interesting paper has appeared recently from the pen of the eminent Swedish agriculturist, Prof. Nils Hansson, in which he raises a number of objections to the use of the Kellner method of assessing food-values (Nils Hansson, *Versuchstationen*, cv, 1, 1927). In the same communication he describes the origin and development of the Scandinavian food unit system. The main difference between the German and Scandinavian systems lies in the fact that, whereas the Kellner starch equivalents were arrived at by measuring, in experiments conducted in a respiration chamber, the actual fat-forming values of feeding stuffs when included in the rations of fattening bullocks, the Swedish feeding standards are the result of numerous feeding trials, conducted under ordinary farm conditions, with dairy cows, growing pigs and working horses. Hansson points out that up to 1923, over a period of 15 years, 77 different feeding stuffs had been investigated in Swedish feeding trials, this having necessitated the employment in experiment of no fewer than 1,536 dairy cows, 1,758 pigs and 462 horses. The object of the earliest trials was to find out in what amounts the different feeding stuffs could replace each other in the rations of animals without affecting the rate of production (in terms of meat, milk, or work) in the animal. One kg. of average barley, representing 1 food unit, was adopted as the standard for comparison, and the number of food units in 1 kg. of any other feeding stuff was based on the amount required to replace a given weight of the standard feeding stuff in the rations of the animals under experiment.

In his feeding trials with dairy cows, Hansson discovered that the relative values of the different feeding stuffs for milk production by no means agreed with the values assigned to them by Kellner. In particular, the protein-rich foods displayed higher values than are indicated by Kellner's starch equivalents. Hansson concludes, therefore, that although the usually accepted starch equivalent of a protein concentrate may be a correct expression of its fat-forming power in a mature bullock, it is far too low as an expression of its milk-producing value. He attributes this discrepancy to the circumstance that digestible protein is utilised more efficiently in milk production than in fat production and suggests that the milk-producing value of protein, in relation to that of starch, should be based on a comparison of the heats of combustion of these constituents. In effect, this implies the use of the factor 1·43 (*i.e.* 5·71 ÷ 4·00) for digestible protein instead of the Kellner factor 0·94 when calculating the starch equivalent for *milk* production of any feeding stuff. When assessing the starch equivalent for purposes of *fattening*, the ordinary expression of Kellner should be retained.

In a spirited reply to Hansson's paper (G. Fingerling, *Versuchstationen*, cv, 16, 1927) Prof. Fingerling, Kellner's successor at Leipzig-Möckern, argues that it is quite unnecessary to raise Kellner's protein factor from 0·94 to 1·43 in order to obtain a better expression of the value of a feeding stuff for milk production. It is incorrect, in the first place, to state that the relative effects of protein and carbohydrates in milk production are in the same ratio as the heats of combustion of these constituents. Further, Hansson makes the assumption that only the protein has a higher value for milk production than for fat production, whereas this is true of *all* the constituents of the ration. Kellner recognised the fact that the transformation of *all* the food constituents into milk constituents demanded a smaller expenditure of energy on the part of the organism than was required for their transformation into body fat. According to Kellner, the starch value of a feeding stuff for milk production is 25 per cent. higher than its starch value for fattening.

It would certainly seem, in the writer's opinion, that when rationing is based on starch equivalents, the difficulty raised by Prof. Hansson disappears automatically, since the fundamental fact is taken into account that 1 lb. of starch equivalent in the ration produces in the animal, during fattening, 1,071 Cals. of energy in the form of body fat, or, in the case of the dairy cow, 1,350 Cals. (*i.e.* 25 per cent. more) of energy in the form of milk. Since the energy content of 1 gallon of milk containing 3·7 per cent. of fat is 3,000 Cals., it follows that the food requirement per gallon of milk should be 3,000 ÷ 1,350, *i.e.* $2\frac{1}{4}$ lb. of starch equivalent. It has been the custom to base milk rationing in this country on this figure. Quite recently, however, the requirement has been raised, somewhat arbitrarily, to 2·5 lb. of starch equivalent per gallon of milk containing 3·7 per cent. of fat.

The Nutritive Value of Sugar-beet By-products.—A great deal of the farmer's interest in the newly established beet-sugar industry is centred on the values, from the feeding standpoint, of the various by-products of the beet crop, namely, sugar-beet tops, sugar-beet pulp and beet molasses. Investigations have been carried out during the last two years at Cambridge with a view to securing information on these matters

It has been demonstrated (H. E. Woodman and J. W. Bee, *Journ. Agric. Sci.*, xvii, 477, 1927) that sugar-beet tops constitute a very useful food which is readily consumed by farm animals. They are highly prized by the German farmer, so much so that the German sugar factories go to a great deal of trouble and expense in drying down artificially large masses of the wet tops for preservation for winter use. Beet tops

contain as much as 85 per cent. of water, and are therefore to be classified with the succulent foods. Their most important food constituent is the sugar of the crowns, this valuable constituent amounting frequently to as much as a fifth of the dry substance of the tops. The beet leaves are extremely rich in protein and possess many of the nutritional virtues associated with young pasture herbage. Like other green fodders, they contain only an insignificant amount of oil. The Cambridge experiments have demonstrated the highly digestible character of beet tops, and have shown that 25 lb. of these residues are able to replace 40 lb. of mangolds in the rations of live-stock.

Fresh beet tops contain quite appreciable amounts of oxalic acid. Wilting of the tops, however, leads to a very material reduction in the content of this harmful constituent, and for this reason it is of the greatest importance that animals should only be permitted access to tops which have been allowed to wilt for some days in the field. The further destruction of oxalic acid as a result of fermentative action in the rumen of the animal renders the risk of trouble almost negligible. Nevertheless, the results of the Cambridge experiments indicate the desirability of giving farm stock access to lime compounds when feeding on the tops, especially in the cases of the ewe flock and the dairy herd. Beet tops should never form more than one-third of the rations of dairy cows, though fattening bullocks may be given larger allowances. On account of their laxative effect, they should be fed with a more binding fodder like hay. Care should be exercised in the field to prevent undue contamination of the tops with soil. With the first signs of putrefaction, which may not set in until after the lapse of several weeks, feeding should be discontinued and the remainder should be ploughed into the soil as manure. Beet tops are extremely rich in mineral constituents, and their manurial value, particularly in respect of potash, is quite considerable.

Where very large areas of sugar beet are grown, it may only be possible to secure consumption of a fraction of the tops before decomposition begins to set in. In such cases, the farmer may wish to preserve a portion of the crop in the form of silage. The question of the ensilage of sugar beet tops has been dealt with in a recent paper (H. E. Woodman and A. Amos, *Journ. Agric. Sci.*, xvi, 406, 1926). For various reasons, the farmer may be led to attempt the preservation of the tops in the pit or clamp. The result, however, is likely to be disappointing, since owing to the presence of betaine in the crowns, such silage readily acquires a most offensive character. Beet tops silage of excellent quality was obtained

at Cambridge by filling the whole tops into small wooden tower silos in such a manner as to ensure tight packing. Eminently satisfactory results were also obtained in respect of palatability, digestibility and nutritive value by following the common continental custom of ensiling alternate layers of whole sugar-beet tops and wet sugar-beet pulp.

A recent communication (H. E. Woodman and W. E. Calton, *Journ. Agric. Sci.*, xviii, 544, 1928) deals with the composition and nutritive value of sugar-beet pulp. By sugar-beet pulp is meant the residue of the sugar beet after extraction of most of the sugar. In the fresh condition it contains nearly 95 per cent. of water, but this is reduced to about 85 per cent. by pressing. The bulk of the wet beet pulp is dried in special drying appliances and sold under the name of dried sugar-beet pulp. At certain factories a proportion of beet molasses is added to the wet pulp prior to drying, the product being known as molasses-sugar beet pulp.

Sugar-beet pulp is essentially to be regarded as a source of carbohydrate, the dried product, on the basis of a 10 per cent. moisture content, containing as much as 59 per cent. of this constituent. The amount of sugar remaining in the dried pulp varies from 1 to 6 per cent., the main carbohydrate present consisting of pectic material. Dried beet pulp contains no more protein than does an average sample of meadow hay. It is also seriously deficient in mineral substances. Owing to its high fibre content, it has become customary to regard dried beet pulp as possessing the character of a roughage of moderate feeding value rather than that of a carbohydrate concentrate. The common view is to look on this feeding stuff solely as a substitute for roots in the ration and to advise against using it for the replacement of concentrates in the productive part of the ration.

The results of the Cambridge investigations, however, have demonstrated that this view is not correct. Sugar-beet pulp has been shown to be highly digestible in the ruminant organism. In respect of the digestibility of its carbohydrate and organic matter, as well as in respect of its total content of digestible material, it compares very satisfactorily with maize meal. The process of drying the wet beet pulp in the factory does not in any way depress its digestibility. Further, from the standpoint of digestibility, it is immaterial whether dried sugar-beet pulp is included in the rations of ruminants in the dry or the soaked condition. When, however, liberal allowances of the dried product are being fed to animals, it is desirable that the food should be well softened in water prior to feeding. This procedure will ensure a higher availability of the digestive nutrients for productive purposes in the animal, and further,

will avert risk of choking trouble which sometimes arises, especially with sheep, during consumption of the dried product. The fibrous constituent of sugar-beet pulp is very little inferior in respect of digestibility to the carbohydrate component, a result which justifies the conclusion that the fibre in this feeding stuff is present almost wholly in the form of simple cellulose, unmixed with any significant amount of the indigestible lignocellulose.

The digestion coefficient of the protein constituent is relatively low and it would appear that the inclusion of sugar-beet pulp in the ration may have the effect of depressing slightly the extent to which the animal is able to utilise the protein in its food. The possible explanations of this peculiar effect are discussed in the original publication. Attention is also directed to the fact that almost four-fifths of the total dry matter of sugar-beet pulp is digested not by the normal enzymic processes, but by the agency of bacteria. The results of the Cambridge inquiry, in respect of the digestibility and nutritive value of sugar-beet pulp, are therefore only applicable to ruminant animals. The extent to which swine are able to digest and utilise this feeding stuff will form the subject of a separate inquiry.

The final conclusion is drawn that dried sugar-beet pulp must be regarded as a carbohydrate concentrate, 1 lb. of which is capable of replacing 0·8 lb. of maize or 0·9 lb. of barley in the productive part of the rations of ruminants. Moreover, from the standpoint of price per lb. of starch equivalent, dried sugar-beet pulp is shown to be a cheap source of digestible carbohydrate in comparison with either maize meal or barley meal.

The Food Requirements of Farm Animals.—Mention has been made in previous reviews of the investigations which are being carried out by Prof. Wood in Cambridge with a view to securing precise information concerning the food requirements of farm animals. In particular, several papers have been cited in which Prof. Wood has dealt with the question of the maintenance and production requirements of adult sheep. These investigations have been rounded off by the appearance during the past year of a paper (T. B. Wood and W. S. Mansfield, *Journ. Min. Agric.*, xxxv, 211, 1928) which treats of the requirements of ewes and lambs, the authors' intention being to fill in the important gap in the information required for computing rations for sheep from birth to maturity.

By the method of comparative slaughter, it was shown that the production requirement of lambs for the first month of life is 1 lb. of starch equivalent per lb. of live-weight increase. The interesting fact was elucidated that, in the lamb, there

is no increase in basal metabolism per unit area of surface soon after birth. The maintenance requirement per unit of surface in the sheep is constant through life. In this respect, therefore, the lamb differs from the young pig, in which the basal metabolism per unit surface area at four months is over 70 per cent. higher than it is in the adult (T. Deighton, *Proc. Roy. Soc.*, B., vol. 95, 1923). This physiological difference is attributed to the difference in the lengths of the gestation periods of the ewe (154 days) and the sow (115 days), and the authors make the interesting suggestion that there is probably a relation between the degree of development of young animals at birth and the intensity of the subsequent basal metabolism.

Other data of interest obtained in this investigation are: (1) The production requirement of suckling ewes is about 3 lb. of starch equivalent per gallon of milk; (2) average ewe's milk contains 2·67 lb. of starch equivalent per gallon; (3) an average ewe yields from 2½ to 4 pints of milk per day. Finally, the authors show how the data thus secured provide a scientific basis for computing rations for suckling ewes and young lambs, a problem in animal husbandry which has hitherto been considered as belonging to the province of the feeder's art rather than to the domain of the scientist.

Attention has also been directed in earlier reviews to Prof. Wood's proposals respecting a new method of computing rations for farm animals. These proposals have now been embodied in complete form in a recent publication (T. B. Wood and J. W. Capstick, *Journ. Agric. Sci.*, 486, xviii, 1928) in which the principles underlying the rationing of dairy animals and fattening stock are dealt with. The writers demonstrate that the method of computing rations for milk-producing animals, by summating the separate requirements for maintenance and production, can be applied with equal success to meat-producing animals. Their proposals are reduced to the form of the following equation:

$$\mathrm{R} = \mathrm{Am} + gc,$$

where R is the net energy of the ration, A the surface area of the animal, m its maintenance requirement per unit surface area, g its gain in live-weight and c the energy stored in unit live-weight increase. Where A, m and c are known, and g is given any desired value, R, the net energy of the ration to be fed for the purpose of producing the desired rate of gain of live-weight, can be calculated. The writers show further that under certain conditions of experiment, the equation can be used for computing the net energy or starch equivalent of a feeding stuff.

PREHISTORIC ARCHÆOLOGY. By L. J. P. Gaskin, Librarian to the Royal Anthropological Institute, London.

Revue Anthropologique, July–September 1928, contains an important article by Dr. van Giffen on the position of Holland as an intermediary region for the cultural influences of the Neolithic cultures of N.W. and Central Europe.

In support of this assertion Dr. van Giffen divides the Netherlands into three regional zones: (1) Southern, (2) Northern, (3) Central, and trace cultural affinities from the adjacent countries in each. Of particular interest, in this connection, is the question of the relationship between the population of the north of Holland in Megalithic times and that of the east of England.

In a long and unfinished article on the "Neolithic Cultures of France," M. Poisson adopts the geographic and comparative method for his argument. He discusses the work of Aoberg, Bosch-Gimpera, Schuchardt, Breuil, and Boule in the same field. The facts, when finally correlated and co-ordinated, should prove important to Prehistorians, but it is a pity that the article is not illustrated.

Trabalhos da Sociedade Portuguesa de Antropologia e Etnologia, vol. 4, Fasc. I, 1928.—Dr. R. de Serpa Pinto writes on the Asturien Station of Ancora, which he discovered in 1925 and excavated in 1928.

The Asturien is a culture which Dr. de Serpa Pinto correlates with the Campignian in France and the Maglemose in Scandinavia; it extends from Spain (Galicia and Cantabria) to Portugal (Ancora and Afife), France (Biarritz) and, perhaps, to Ireland, County Antrim.

More than five hundred implements were discovered at Ancora; of interest in this respect are the hand-axes with curvilineal base. The article is well illustrated and contains an excellent bibliography.

Antiquity, September 1928.—In a long article on the "Rock-pictures and Archæology of the Libyan Desert," Mr. Newbold makes a careful survey of the areas which he has explored and of the stone implements and other archæological material which he found. He classifies the Libyan Desert Rock-pictures into four rough groups: (*a*) Bushman, late palæolithic or early neolithic; (*b*) early Libyan, neolithic, predynastic, Old Empire; (*c*) Middle Libyan, Middle and Late Empire to the early Meroitic period; (*d*) Roman, mediæval and modern.

In conclusion, he points out that there still remain three unexplored areas, namely, the country west and south-west of Kufara, the triangle Terfawi-Owenat-Nukheila, and the unknown country south of Siwa.

Skara Bræ.—In a letter to *The Times* of September 3, Prof. V. Gordon Childe describes this prehistoric village, and in particular his discoveries during the excavations in progress this summer. The huts, it was found, were roughly square and the walls built of dry masonry; the street on to which these huts opened was roofed with stone slabs, and on these slabs the inhabitants threw their kitchen refuse. The excavators had to dig down through 4 or 5 ft. of this midden deposit before coming to the intact hut to which the street eventually led. Along the walls of this hut were enclosures resembling pigsties made of great stone slabs. In the floor of the sty on the right a grave had been dug, and in it lay two skeletons in the contracted posture. The cover-stone of the grave had been built into the wall in such a way that it was clear that the burial was contemporary with the building of the hut. Prof. Childe believes this to evidence a human sacrifice to the stability of the walls. On the front of the stone slab which enclosed the grave an inscription was found in an unknown script.

The complete absence of metal in the hut and the discovery of well-worked flints and pottery on the floor suggest a Neolithic culture which may have survived to a much later era in so remote an island, but the runic inscription would seem to point to a date not later than A.D. 600. In the face of such conflicting evidence, it is hoped that a close study of the pottery and the skeletal remains may shed further light on these important discoveries.

Excellent illustrations of the finds have appeared in the *Illustrated London News* for September 15.

Mitteilungen aus dem Museum für Völkerkunde in Hamburg, vol. 13.—Dr. Gustav Schwantes surveys in great detail the Northern Palæolithic and Mesolithic Era. He deals in turn with Northern Chronology, Traces of Man in the first and last Ice Age, the Schlutup settlement, and the following cultures: Schaalsee, Ahrensburger, Duvensee, Maglemose, Havel, Boberg, Oldesloe, and Bloksbjerg. Dr. Schwantes concludes his study in a comparison of the Northern cultures with those of Western Europe in so far as they have cultural affinities.

The article is profusely illustrated and has a full bibliography.

L'Anthropologie, Tome 38, Nos. 3–4, contains an account of the Azilian cave of "Trou Violet" at Montardit by Ida Vaillant-Couturier Treat and Paul Vaillant-Couturier.

In this extremely careful survey of a typical site of the Azilian culture there is little to remark. The finds, save for the paucity of the material discovered, resemble in every way, by the presence of painted pebbles, harpoons, etc., those of the *Mas d'Azil*, and the two burials prove beyond doubt the dating of the culture.

A fuller survey of the human remains by Miss Sawtell will be found in the *Papers of the Peabody Museum*, vol. xi, No. 4. The article is well illustrated.

Journal of the Royal Anthropological Institute, lviii, 1928. —In the "Introduction of Civilisation into Britain," Mr. H. J. E. Peake traces the routes by which civilisation spread over Europe and finally into Britain. The first grain-growers and potters, who introduced their art into the Ægean at an early date, probably followed the route of the Danube to Switzerland, the Rhine, and the country around Liége. A Mediterranean trade route existed in early Minoan times, from the head of the Gulf of Corinth to Sicily and Southern Italy, and the presence of Cycladic beads in Portugal indicates that the elements of civilisation had reached the Atlantic coast before 2200 B.C.

It is thought that agriculture and the potter's art reached England at the dawn of the Neolithic Era. Fragments of pottery found at Mortlake and Peterborough and at Rodmarton and other sites in Gloucestershire have been described by Prof. O. Menghin as *Grimston-Keramik* and *Peterborough-Keramik*. The *Peterborough-Keramik* bears close resemblance to a similar type of pottery found in Finland and East Sweden. At Windmill Hill, Avebury, a prehistoric village of the *Michelsberg* type, pottery resembling the *Grimston-Keramik*, but bearing closer affinity to the *Westische-Keramik*, has been found.

It seems likely that the elements of civilisation passed up the Rhone valley into Burgundy, where the *Westische-Keramik* developed among a people who lived in fortified villages of the *Michelsberg* type, thence the culture spread to Switzerland, the Rhine, the north of France and Belgium, and from there to the south of England some little time before the arrival of the *Peterborough-Keramik* on the north-east coast.

Miss Garrod describes in great detail the excavation of a Mousterian Rock-shelter at Devil's Tower, Gibraltar. All the implements discovered in the different stratas were of Upper Mousterian type, and the cave had apparently been used only at certain seasons of the year.

Subjoined to this paper is a report on the human remains by Mr. L. H. Dudley Buxton and a report by Prof. Elliot Smith on an endocranial cast of a Neanderthaloid skull of a child of five or six years of age discovered in the cave. The chief interest in the examination of this endocranial cast lies in the fact that the Devil's Tower skull affords proof of developments in cerebral formation formerly supposed to be the exclusive privilege of *homo sapiens*. The paper is well illustrated and, besides a history of the excavation of the site, contains an excellent bibliography.

Mitteilungen der anthropologischen Gesellschaft in Wien, vol. lviii, 3–4.—In an article on "The First Appearance of the Indo-Germans in Hither-Asia," Herr v. Christian divides his paper into two parts; in the first he traces the descent of the Hittites and their first appearance in Hither-Asia, and in the second he does the same for the Indo-Aryans.

Much of the archæological material upon which he bases his study is gathered from such important works as Frankfort, *Studies in the Early Pottery of the Near East*; Childe, *The Aryans*; de Genouillac, *Céramique Cappadocienne*; Chantre, *Mission en Cappadoce*; and other authors of repute. The article is illustrated.

East African Archæological Expedition.—In a letter to *The Times* of August 1, Mr. L. S. B. Leakey describes the achievements and the hopes of the expedition which has just started for East Africa.

In explaining the position and importance of East Africa in the scheme of cultural relationships with Europe, Mr. Leakey holds the view that the Capsian Folk came from somewhere in the Sahara Desert, which at that time had a temperate climate, and that as the Sahara began to dry up these people took their culture to the North African littoral and on into Europe, where it is known as Aurignacian, and, perhaps, to the south and east of Africa. In the excavations of 1926–7 the Expedition found a culture closely resembling the Capsian and Aurignacian in the Elmenteita district of Kenya Colony. The human remains discovered bore close resemblance to those of the Lower Aurignacian culture in France, and in no way resembled the present negro population.

A disturbing feature of the Elmenteita culture was the presence of large quantities of pottery—pottery is usually regarded as one of the criteria of the Neolithic Era. The Expedition will search for fresh evidence in this connection.

A culture of a later date (2000 B.C.), called the Nakuru culture, was found associated with the discovery of a well-preserved skeleton, microlithic implements, and evidences of an agricultural population. The relation of this culture to that of Elmenteita will be one of the problems which the Expedition will have to solve.

The Antiquaries Journal, October 1928.—In an illustrated article on the "Maltese Rock-tombs," Prof. T. Zammit points out that it is usual to consider the rock-tombs as characteristic of four distinct groups: (1) the Phœnician; (2) the Carthaginian; (3) the Roman; and (4) the early Christian.

But further discoveries at Bukana, Xaghra, Gozo and Nadur Benjemma have led Prof. Zammit to believe that the Neolithic population also used the rock-tombs; and, in this

connection, it seems probable that many of these early tombs were cleared of their contents in Phœnician days and used by the later inhabitants.

Dr. Cyril Fox and Mr. G. R. Wolseley report on an early Iron Age site at Findon Park, Findon, Sussex. The pottery series, La Tène I and La Tène II, justifies the conclusion that the Findon Park village site was occupied about 300–100 B.C.

The article is illustrated, and contains an appendix on the animal remains by C. Matheson and L. F. Cowley.

Bulletin de la Société préhistorique française, July–August 1928.—M. Henri Gadeau de Kerville writes on the Rock-pictures at Crechets, Hautes-Pyrénées. Apart from local legend attached to these petroglyphs, M. de Kerville points out that one at least has some phallic significance.

He assigns them a Neolithic origin.

September 1928 contains a note by M. G. Poisson on the finds at Fort-Harrouard. The two cultures represented are those of the Neolithic and the Bronze Ages. From the evidence of the material collected, it is thought that the Fort was then abandoned for a period of about four hundred years, that is, from La Tène I to La Tène III, pottery of the latter culture appearing in great profusion.

The Times, August 18, contains a report on the American Expedition in Mongolia. A considerable number of stone implements, dwelling-sites and ornaments of the Mesolithic Era have been found. The dwellings are always near sand dunes on the shores of lakes (long dry), and were made of skins supported by branches.

Evidences of a Mousterian culture were found, but in a very limited amount.

The Times, September 18, contains a report on the Meare Lake village excavations. It is now certain that in the part where the excavations are in progress there was a weaving establishment, for seventeen combs and twenty spindle-whorls, besides triangular loom-weights of different sizes, have been found. Of particular interest is the discovery this year of a bronze box some 12 in. in diameter, having a cover; the vessel is not ornamented, and is in a poor state of preservation.

The Times, September 24, contains a notice of the German Archæological Institute's (Dr. Welter) work at Balata (ancient Schechem) for this season. The main discovery was that of a large blunt-topped pyramid 131 ft. square built on a base 26 ft. high, apparently part of the foundations of a tower on the fortifications. Below the fortifications were found traces of an older city which dates from 1700 B.C. It was destroyed in 1300 B.C. and covered by the fortifications and tower described above.

ARTICLES

SOIL BACTERIA AND FERTILITY

By P. H. H. GRAY, M.A.
Rothamsted Experimental Station, Harpenden

In any population which can carry out work as an expression of energy, in addition to multiplying its numbers by the utilisation of food material, the number of individuals may be taken as an index of its capacity for producing a certain amount of work. Under ideal conditions, where of a number of individuals each produces an equal amount of work, the sum-total of the work produced is a function of the number working. Under normal conditions, however, in a heterogeneous population, the work produced is a function of only a fraction of the population. The same is true of the heterogeneous micro-population of the soil. This population is extremely varied, both in morphology and physiology. It is composed of animals (the protozoa) and plants (the algæ, the fungi including the ray-fungi, and the bacteria). In numbers the bacteria greatly predominate, and in addition most of the biochemical processes that take place in soil are carried out by these organisms. These processes are of great importance in rendering the soil fertile and capable of supporting the growth of the higher plants.

The soil bacteria live not only in the soil solution, but also in the colloidal material surrounding the mineral particles. Their food consists of carbon and nitrogen and certain essential minerals. These latter are obtained in the form of electrolytes from the solution, from the colloidal system, or directly from the mineral framework of the soil. Since some of them live only in the presence of a sufficient supply of air and others in the presence of a minimum amount of it, the colloidal systems, organic or inorganic, have a great influence on their activities; these systems control the water-holding capacity of the soil and thereby the amount of pore-space available for the organisms. The bacteria obtain their carbon from the organic colloidal complexes (collectively termed "humus"), from freshly added organic material, or from carbon dioxide. Nitrogen is derived from the complex proteins of organic residues, from mineral nitrogen, or from the air.

The bacteria are unable, through lack of chlorophyll, to

obtain energy from carbon dioxide of the air in the manner of green plants. They can be divided into two groups, based on their physiological requirements. The first group comprises those that are able to build up their protoplasm through the oxidation of simple inorganic compounds and carry out certain biochemical processes of first importance to plant life, namely, the production of nitrates and sulphates. The second physiological group is composed of those bacteria that depend upon organic compounds of carbon for obtaining their energy. These compounds enter the soil as plant residues, and are utilised in the form of celluloses, pentosans, sugars, and other carbon compounds. This group can be further considered under two headings: (i) those organisms that can build up their protoplasm by obtaining nitrogen from the air (nearly four-fifths of which, by volume, is nitrogen); and (ii) those that can only utilise soil nitrogen in organic or inorganic form. The former physiological group is generally termed the autotrophic, and the second group the heterotrophic, bacteria.

The earlier attempts to enumerate the soil micro-flora and to connect them with soil fertility were not fruitful of positive results, because the methods that were used were derived from medical bacteriology; indeed, the earliest attempts were made by medical workers whose chief interests lay in the isolation of pathogenic micro-organisms from various depths of soil. It was, however, early recognised that the soil was a suitable habitat for bacteria, and further that they were capable of active life therein. It has also been shown that the soil micro-flora is a distinct ecological group consisting of a multitude of forms with only a few outstanding numerically. It has been found, too, that different types of agricultural land possess micro-populations of varying magnitudes. The purpose of counting soil bacteria, therefore, is to discover what relationship there may be between soil fertility and the bacterial population; and for this purpose only the micro-organisms in the first few inches of soil have to be considered.

Methods

The ideal method of studying an environment for its bacterial population is to examine it directly. This unfortunately is not yet practicable, since by the methods so far advocated it is only possible to make a rough estimate of the number of organisms. The dispersed state of the soil particles, which may number many millions per gramme, severely interferes with accurate observations through the microscope, and the methods in use for staining the film of soil on the micro-slide do not completely differentiate between soil particles

and micro-organisms. The method in general use for counting has therefore been the plating method, first developed by R. Koch in 1881, the basis of which was gelatine mixed with complex organic nutrients. The principle of this method consists in diluting with sterile liquid a known weight of soil to a known degree. A small quantity, usually 1 cubic centimetre, of an appropriate dilution is mixed with the nutrient gel medium in sterile dishes ; after the gel has set, the dishes, covered to exclude dust, are incubated at a suitable temperature. Each organism in the original diluted suspension that is capable of growing on the medium grows until a visible "colony" is formed. The number of bacteria in the soil is estimated by taking the mean number of colonies on several plates and multiplying by the degree of dilution. The use of gelatine as a semi-solid substrate was later replaced to a large extent by agar-agar, since large groups of soil organisms were found rapidly to liquefy the gelatine. The agar is also less variable in its results, and has become firmly established on account of the ease with which it can be prepared. Bacteria that can utilise agar as a food material or that can liquefy it are extremely rare. The use of silicic acid as a gel substrate has not been favoured, except in the case of the nitrifying organisms, that flourish best in the absence of organic matter, on account of the difficulty in preparation. It was not, however, until Winogradsky discovered that certain bacteria would not grow in pure culture on a medium containing traces of organic matter that it was realised that the methods in use for counting soil bacteria were not suited to groups which do not appear on the plates, and which were of the greatest importance in a study of the biochemical processes taking place in the soil. These processes are of so varied a nature that it is clearly impossible for any one medium to be suited to the physiology of all the groups of organisms. The aim of workers in quantitative soil bacteriology has been therefore to search for a medium which will give constant results with a fraction only of the soil population. It should be possible with such a medium to study the fluctuations taking place in this fraction of the bacterial population at intervals either of space or of time.

Tests of Uniformity

An examination of the literature dealing with the methods in use was made by Wyant, and has shown what great lack of standardisation has existed. The elimination of highly complex organic compounds, such as peptone and meat extract, of recognised value in medical bacteriology, has been the first step towards a standard medium. Conn used pure mineral

salts with the addition of glucose and sodium asparaginate. Others have used an extract of soil as a basis, and this has proved to be not so variable as might be expected, even when made from different soils. It has been shown that an agar medium can be standardised in such a way that accurate conformity within the laws of random sampling may be obtained in the majority of cases. Given a standard method of diluting the sample for the purpose of making a set of plates and a medium on which constant results may be expected, the errors resulting from the random distribution of the bacteria in the diluted suspension (and therefore in the sample from which the suspension was made) can be calculated. The following figures illustrate the close conformity of results that can be obtained by four different workers using the same sample of soil and separate but standard manipulative technique.

TABLE SHOWING AGREEMENT BETWEEN FOUR SEPARATE WORKERS

Worker. / Plate No.	A.	B.	C.	D.
	Number of colonies counted.			
1	32	33	32	37
2	30	32	31	32
3	30	28	31	32
4	29	27	28	30
5	26	26	26	26
Mean	29·4	29·2	29·6	31·4

The agreement between the four sets is as close as that between the five plates in any one set. It is thus possible to standardise a method to a high degree of accuracy.

In making a study of the fluctuations of the soil micro-organisms care has to be taken that the plot chosen is uniform in character. In order to smooth out differences due to uneven fertility, it is the practice to take several samples of soil from different parts of the plot and to mix them; a composite sample is thus obtained representative of the whole area under observation. That this composition is necessary has been shown by Waksman, who made counts on fifty-one single samples from the same plot. The following figures are taken from his paper:

BACTERIAL COLONIES ON PLATES MADE FROM SINGLE SAMPLES

Sample No.		Sample No.	
18	177·8	22	101·3
19	189·8	23	226·8
20	111·5	24	167·6
21	125·5	25	111·2

The difference between two adjacent samples may be as much as 100 per cent. In studying the numbers of bacteria in soil at intervals of time, therefore, this factor of their uneven

distribution in space must be considered, since it may otherwise introduce large sources of error. If the plot is sufficiently uniform, the error from this source can be calculated. The following figures show that on a soil uniform in respect to numbers the variance between four samples taken simultaneously is the same as the value of the variance between plates in a single sample.

	Sample			
Plate No.	1	2	3	4
1	32	31	34	27
2	32	40	38	35
3	38	43	43	36
4	40	45	52	41
5	52	45	55	45
Mean	38·8	40·8	44·4	36·8

Variance of the 20 plates, 56·27.
Variance of the 4 sets, 56·97.

Fluctuations in Time

The most comprehensive series of counts of the soil population has been that made by Cutler, Crump, and Sandon at

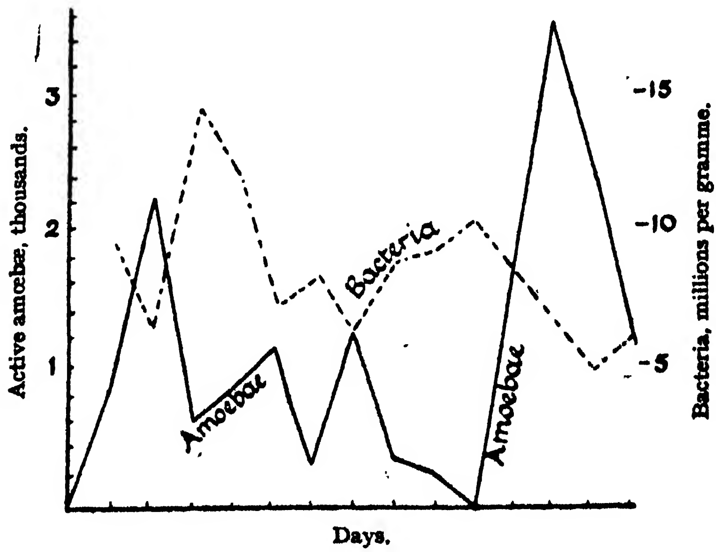


Fig. 1.—Daily changes in numbers of Bacteria and active Amœbæ in Broadbalk Field, Rothamsted.

intervals of 24 hours for 365 days. The samples were taken from a field plot at Rothamsted, which had received farmyard manure annually for many years. The simultaneous distribution of the bacteria over the plot was found on several occasions to be even; any fluctuations that occurred from

day to day could therefore only have been due to fluctuations in time. Counts of the protozoan population were made daily also, on the same sample. The results of this work, which were submitted to a statistical examination, clearly showed that daily fluctuations of a very high order take place both in the bacterial flora and in the microfauna. The curves in Fig. 1 bring out these day-to-day changes very clearly. The

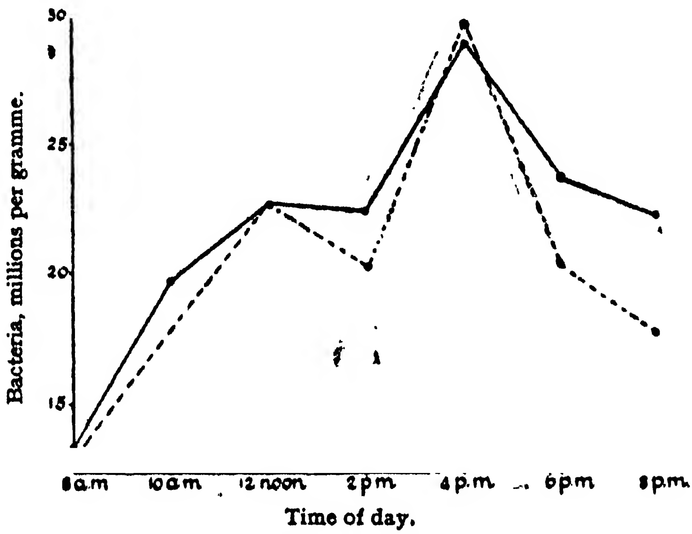


FIG. 2.—FLUCTUATIONS OF BACTERIAL NUMBERS AT INTERVALS OF TWO HOURS, IN HALVES OF A PLOT OF GARDEN SOIL (THORNTON).

degree of fluctuation in the bacterial numbers, often more than 100 per cent. on consecutive days, is so great as to cast doubt upon the value of many previous counts made at greater intervals, where the necessity for the precautions referred to in regard to sampling and technique may not have been realised. Such daily fluctuations have recently been found by Smith and Worden, in the United States of America, to occur in lawn soil. Their data were statistically analysed by Thornton and Fisher, and the fluctuations were shown not to be the result of sampling errors. It is possible, therefore, that such changes may be of universal occurrence in cultivated soils.

Since under favourable conditions an organism like a bacterium may by simple fission double its numbers in about 30 minutes, it is important to know whether the changes in total numbers of those that develop on the agar-plate may not occur within shorter intervals than 24 hours. Several series of experiments of this nature have been carried out at Rothamsted. In the earlier of these experiments samples

were taken at 2-hourly intervals for 60 or 80 hours. The bacterial numbers in a plot receiving annually a dressing of farm-yard manure fluctuated extensively, even reaching a difference of 100 per cent. in consecutive 2-hourly periods. Further evidence of these fluctuations was obtained from a specially prepared plot of garden soil, the counts being made only in the day-time at 2-hourly intervals. In the case of these later experiments similar large differences were found to occur between consecutive samples, and to be simultaneous on two halves of the plot (see Fig. 2). The result of these experiments thus establishes that very great changes take place in the number of soil bacteria within very short intervals of time, and that the changes may often be of the same order of difference, or greater, than differences previously found to occur.

Relationship to External Conditions

The most obvious of the fluctuating external conditions which could affect the whole of the soil population are those of temperature and moisture. The first of these two factors is not subjected to violent fluctuations except on the surface layer of soil. Except, indeed, as a contributing factor in the rapid drying of soil, temperature has not been shown to exercise a great influence on the biochemical activities of organisms in field soil. Russell and Appleyard, who made fortnightly counts on gelatine of samples from five different plots throughout twelve months, concluded that a spring rise in numbers was due to the initial rise of temperature and that the effect was soon masked by other factors. Fluctuations at 2-hourly intervals occur independently of temperature, though on one occasion a disturbance of the diurnal rhythm was connected with a mid-day temperature wave. These diurnal rhythms appear to take place both in summer and in early winter, but the numbers in summer varied, in the soil studied, from 32 to 24 millions, and in winter from 14 to 6 millions per gramme. Miss Crump, by taking counts at weekly intervals, has found that moisture and bacterial numbers often vary together. The figures given in the table below show this relationship.

Contingency Table showing Relationship between Bacterial Numbers and Soil Moisture

	State of the Variables, Moisture and Bacterial Numbers. May 1916—May 1917.		
Field.	Varying together.	One varying.	Varying inversely.
Broadbalk . . .	29	5	9
Great Harpenden . .	30	5	10

Although the agreement appears too close to be accidental in such a large number of cases, it must be remembered that

moisture is only one of the soil conditions likely to affect the micro-population. In the 365-daily count referred to no evidence was obtained that either temperature or moisture had any effect on bacterial numbers. It was, however, found, by taking fortnightly averages of the numbers of bacteria and protozoa, that distinct seasonal changes occurred ; the highest numbers of the whole population were found during the spring and autumn. In the daily count no relationship was found with the rainfall, although the conditions were such that the influence of dissolved oxygen (as suggested by Russell and Appleyard) in a heavy shower of rain should have been apparent. Since the majority of the daily counts were made during the drought of 1921, it is possible that greater fluctuations in moisture might have been operative during a period of different climatic conditions. It should be noted that the general level of numbers in the summer of 1920 is less than half that of the summer of 1921. A similar difference can be calculated from the weekly counts referred to above ; the numbers in May to July 1916 being twice those of the same period in 1917, a difference of the same order occurring in two separate fields.

Mean Level of Bacterial Flora in Two Years, during the Season May to July

	Bacteria in millions per gramme. Broadbalk.	Great Harpenden.
1916	18	15
1917	9·2	7·4

Rainfall, however, as distinct from the soil moisture resulting therefrom, may be correlated with changes in the bacterial numbers under different seasonal conditions. A very close correlation has indeed been found in a single case by Thornton.[1] On two plots of different manurial treatment, situated several yards apart, the bacterial numbers changed synchronously for a week at intervals of 24 hours, and the correlation observed between the bacteria and rainfall was 0·88.

Influence of Fertilisers

This experiment also gave evidence that the population that fluctuates in each plot was similar ; the numbers on one plot were double those of the other, while the amplitude of fluctuations was the same. The manurial treatment of the plot having the higher numbers (30 to 50 millions per gramme) consisted of an annual dressing of a complete

[1] I am much indebted to Mr. H. G. Thornton for permission to use these unpublished data.

mineral fertiliser, the effect of which has been to raise the yield of wheat to threefold that of the other (unmanured) plot, whose bacterial population ranged from 15 to 30 millions per gramme. The effect of the fertiliser can therefore be estimated as having also doubled the numbers of the non-fluctuating groups of bacteria.

This raises the question as to whether the numbers of soil bacteria can be taken as an index of soil fertility. Such, indeed, was the hope of workers in agricultural science after Caron suggested in 1895 that any treatment of the soil which increased the micro-organisms also leads to an increase in crop. Apart, however, from the considerations dealt with here, attempts to demonstrate this have not been successful. Neller recently obtained correlations between crop yield and three other variable, viz. bacterial numbers, oxidising power of the soil, and production of nitrate. He found, however, no correlation between crop yield and production of ammonia, a fact which suggested the unreliability of the usual counting media, designed primarily to enumerate ammonifying bacteria, as a method of obtaining a microbiological index of fertility. We have seen, however, that under carefully standardised sampling and counting methods the fluctuations that are found in time can be taken as real, and that fluctuations may occur synchronously on plots of different fertility. The cause of such rapid fluctuations still remains obscure; they may be inherent in the life-cycle of the bacteria whose numbers are determined, due to fluctuations in groups of bacteria which do not appear on the plates, or due to conditions set up by the protozoa.

The protozoa are a normal part of the soil population, and some of them have an influence in depressing the size of the bacteria population. They occur most numerously in the same layer of soil in which the highest numbers of bacteria are found, and greatly exceed them in bulk. Methods of estimating their numbers depend upon cultivating on organic media different dilutions of the soil suspension. The amœbæ are of the greatest importance in connection with bacterial numbers, though flagellates and ciliates are also known to feed on bacteria. Cutler has shown the feeding effect of amœbæ and flagellates on bacteria by inoculating cultures into sterile soil. In the absence of protozoa bacterial numbers reached 170 millions per gramme in two weeks and remained high, while in the presence of flagellates or amœbæ their numbers were depressed to about 50 millions.

In normal field soil the number of bacteria and active amœbæ (but no other type of protozoa) vary inversely, with a high degree of fluctuation from day to day (see Fig. 1).

These inverse fluctuations occurred in 86 per cent. of the day-to-day counts referred to. Some species of protozoa pass through daily rhythms which have no evident relationship to bacteria numbers. From the fact, however, that seasonal rhythms occur in the protozoan population as well as in the bacterial, there must be external causes affecting the whole population ; the numbers of protozoa may not wholly depend upon food supply. It is possible, however, that they also cause considerable fluctuations amongst the bacteria that do not appear on the plates. The experiments of Russell and Hutchinson clearly showed that incomplete sterilisation of soil, sufficient to destroy organisms like the protozoa, resulted in increased crops. These organisms may therefore be an important factor in depressing the level of fertility through their action on the bacteria.

From the physiological standpoint, the relation between productivity and certain of the more important biochemical processes has been clearly demonstrated. Waksman summarises the results of experiments with regard to nitrification. The following is condensed from this summary :

Level of fertility.	Relative nitrifying capacity of soil. Rothamsted.	Nevada.	Hawaii.
High . . .	93	54	21
Medium . . .	38	30	8
Poor . . .	26	4	4

Such tests have been applied by several workers and correlations found to exist where the soil is not of too acid reaction, but the numbers of the nitrifying bacteria (which are probably active in all normal cultivated soils) have not yet been estimated with certainty. The conditions of the soil at the time of making such tests may account for results of the opposite nature, since any disturbance of the soil by external influence will change the equilibrium of the micro-population for a time. Similarly, whenever there is a limiting factor to plant growth, such as insufficiency of an essential mineral, relationship between bacterial numbers in one group or another and fertility can also be demonstrated. The nitrogen-fixing organism *Azotobacter* requires an appreciable amount of phosphorus for its metabolism since over half of its ash content may consist of phosphoric acid. The insufficiency of phosphoric acid in soil therefore becomes a limiting factor for the growth of this organism as well, and the apparent relative abundance of *Azotobacter* can be utilised as an index of the soil's deficiency in this respect. In connection with this test, however, the actual numbers in fresh soil have not been estimated with any degree of certainty. We see, therefore, that through the

limitations of the plate counting methods, two at least of the groups of soil bacteria that carry out important biochemical processes are not included in the general count.

This limitation was early recognised in micro-biological analysis of soils, and in order to overcome it a method was devised by Hiltner and Störmer for counting the numbers in the different physiological groups of bacteria. This also consisted in diluting the soil with liquid, but a series of different degrees of dilution were inoculated into a liquid culture media of compositions suited to the nutritive requirements of the various known physiological groups. The number of organisms of each group was calculated by observing the degree of highest dilution at which growth occurs; between this and the next dilution (at which no growth occurred) the number of bacteria was supposed to lie. For various reasons, however, this method has not been favoured. The chief objection is that liquid culture media are not suited to the growth of many organisms, and there is no clear indication that a single organism will develop—it has been found in fact that some species will grow only if there is a certain number of cells together. The method, with duplication only, involves a large amount of material and space; and the results have been found to be too variable. This method has proved useful for indicating the presence or absence of certain groups in a soil at a particular time. It cannot be used to give any indication of bacterial fluctuations at short intervals, and without this the figures obtained as an index of seasonal changes may be of no value.

Several biochemical reactions of the soil have been found to have some bearing on fertility. These reactions may be considered as due to certain physiological groups of the microflora, though not of any one species of the bacterial group, for the actinomycetes and fungi are also effective in breaking down organic matter. Since it has long been known that the consumption of oxygen during the decomposition of organic matter in soils and the output of carbon dioxide therefrom were chiefly due to biological phenomena, these processes have received particular attention as a means of estimating microbiological activity. Amongst these processes are the decomposition of cellulose, pentosans, and sugars, mannitol decomposition, and the nitrogen-fixing capacity of soils. Many of such biological phenomena in soil result in a measurable production of carbon dioxide, and Russell has found that there is a relationship between oxygen consumption and the fertility of arable soils. It has been found, indeed, by Stoklasa that a fertile soil may produce four or five times as much CO_2 as a poor soil containing little organic matter. Similar results have been

obtained by Waksman and Starkey; their results for the respiratory index of soils are related to the crop yield as follows:

	Soil: Untreated.	Soil: Manured and fertilised.
CO_2	1	4·15
Yield	1	3·57

In work of this type less value can be attached to any single figures given for the numbers of bacteria, since the variation between these in different soils is usually not much more than might occur at short-time intervals even in the soil having the lowest numbers. In addition, it must be remembered that the phenomenon of CO_2 respiration is a function not of one group of the bacteria but of the total micro-organic population.

Direct Methods

It may prove possible to obtain an accurate estimate of the total flora of a soil by a direct method, first developed by Conn in 1918. In principle this consists in staining a thin film of a suspension of soil on a microscope slide; the organisms take the stain (a dye of the eosin series) and the soil particles and colloidal material for the most part remain unstained. From such preparations Conn found up to twenty times the number of organisms as are found by the plate method. Winogradsky has further developed this method, and considers that soils have a stable flora that inhabits the colloidal system and is chiefly concerned in slowly decomposing the humic matter, and a flora that he calls zymogenous, since it rapidly grows on the addition of certain organic or inorganic compounds. Similar conclusions are reached by the Russian workers A. A. and B.A. Richter, who group the soil flora under three descriptive headings: *cocci*, *Azotobacter* cells, and *bacilli*. The relative numbers of these groups are given by them, for loam soil at 10 centimetres depth, as 569, 184, and 106 millions respectively. The total found at the surface is given as 1,400 millions per gramme. The numbers agree approximately with those given by Conn, but at present no great reliability can be placed upon comparative figures, since errors in the technique have not been estimated and the amount of soil examined is not known with accuracy. A new technique for counting bacteria in stained films of soil (see *Nature*, vol. 122, p. 400) has recently been developed at Rothamsted. The method, which eliminates the need of having a known weight or volume of soil on the slide, allows of an accurate estimation of error, and it therefore becomes possible to count with certainty the number of bacteria in parallel samples of soil and also to estimate the

fluctuations of the population in time. In regard to the magnitude of the soil micro-flora, the results so far obtained emphasise the figures quoted above, for example, the number of colonies on Thornton's count medium indicated about 20 million bacteria per gramme, while the number found by this direct method was as high as 4,000 million. It is, however, impossible as yet to distinguish between dead and viable bacteria. The method allows of a certain reliance being placed upon qualitative counts, since the relative numbers of different morphological groups can be compared. When difficulties at present inseparable from the technique have been overcome such a method should open up a complete new field for exploration both in qualitative and quantitative soil microbiology.

THE WORK OF THE FISHERIES LABORATORY, LOWESTOFT

By E. S. RUSSELL, M.A., D.Sc., F.L.S.

Director of Fishery Investigations, Ministry of Agriculture and Fisheries

I. Historical

Fishery research is of comparatively recent date. It took its origin in the last quarter of the nineteenth century from two main sources—(1) the growing interest in marine biology and the science of the sea which culminated at the time in the famous *Challenger* expedition (1872–6), and (2) the pressing need for investigating the alleged decline in the productivity of the deep sea fisheries.

Although complaints of the damage done to the stocks by fishing operations were rife in the middle years of last century, the danger of "overfishing," so far as bottom-living fish are concerned, did not really exist before the introduction and spread of steam trawling in the '80s. We know now, as the result of research, that prior to the present period of intensive fishing, the earlier methods—lining and trawling by sailing boats—affected mainly the accumulated stocks of fish and did not at all reduce the possible annual yield. Huxley was right in 1883 when he asserted that the resources of the sea were practically inexhaustible by the methods of fishing then in vogue, and on the basis of this advocated the sweeping away of all restrictive legislation. Nowadays he would not have been so bold—the increase of fishing power has been so enormous and the effect upon the stocks of fish is so great and so definitely established that there is, at least in certain areas and for certain fish, some doubt as to whether we are not taking from the sea a greater weight of fish annually than can be replaced by the natural processes of reproduction and growth. There arises therefore the question of "overfishing."

It became acute in the '90s, particularly with regard to the flat-fishes of the Southern North Sea, and several attempts were then made to introduce legislative restrictions. The pressure was, however, relieved by the spread of the trawl-fishery to other regions—to Iceland and Faeroe, and later to the Barents Sea on the northern coast of Russia and the great hake grounds of the Atlantic Slope. At the present

day, British fishing vessels range from the Barents Sea in the north to the Morocco coast in the south, and from the North Sea west as far as Greenland. There is hardly a square mile of accessible and fishable ground inside 250 fathoms which is not visited at some time or other by a British trawler or steam liner. There is now little new ground left to explore and exploit, and the problem of over-fishing again recurs. The war gave the grounds a certain amount of rest and permitted a certain accumulation of stocks, but this temporary surplus was soon swept away when fishing got into full swing a year or so after the war.

In this country fishery research was started in the '80s under the auspices of the Fishery Board for Scotland; some time afterwards, several workers, among whom may be mentioned Cunningham and Holt, took up these problems in England, in connection with the recently founded Marine Biological Association. At the beginning of the present century, an important step in advance was taken by the foundation of the International Council for the Exploration of the Sea. To this organisation all the maritime nations of North-Western Europe gave their adherence, and henceforward marine and fishery investigations were in the main carried out on an international basis. The early programmes of the International Council show clearly the twofold interests which led to its formation. On the one hand there was the purely scientific interest in general oceanography and marine biology which led to extensive researches being carried out on hydrography, plankton and bottom-fauna; on the other, there was the more practical preoccupation with economic problems, which led to an intensive study of the food-fishes, and particularly to the elaboration on an adequate scale of fishery statistics concerning the quantities and sizes landed. The Council has very wisely always insisted on the need for a broad basis of general research on the conditions of life in the sea, in order that the results of the more directly economic work may be seen in their proper relations and interpreted scientifically. Much good has come of the close association and co-operation of the national workers: we in Great Britain owe much to the splendid work done by the Norwegians, the Swedes and the Danes.

Even a short account of the varied activities of the Council would go far beyond the scope of this article; the reader who is interested may be referred to the Anniversary Volume [1] recently published by the Council on its first twenty-five years' work.

The English share in the international investigations was up to 1910 undertaken mainly by the Marine Biological

Association, who maintained a laboratory at Lowestoft, and a trawler, the *Huxley*, for the sea work. During that period the investigations related chiefly to the plaice and the plaice fisheries, and very admirable and important work was carried out on migrations, growth-rate, transplantation, and so on. The general control was in the hands of Dr. Allen, while Prof. Garstang, ably assisted by Mr. J. O. Borley, was in actual charge of the investigations.

The Ministry (then the Board) of Agriculture and Fisheries took no direct part in the work at sea, but confined its attention to the very important task of establishing reliable and adequate fishery statistics, and of accumulating very extensive data as to the sizes of plaice and other fish landed at the markets. The credit for this achievement belongs in the main to two men, the late Mr. W. E. Archer, then in charge of the Fisheries Department, and Sir Henry Rew, then in control of the Statistical Branch of the Board. It may justly be claimed that the fishery statistics of England and Scotland are the finest in the world [see 2].

In 1910 the Board took over the complete control of the English international work; the laboratory and ship were given up, and the work continued under somewhat unsatisfactory conditions in London. Plans for reorganisation were maturing when war broke out and put a stop for nearly five years to all deep sea researches.

It was early realised that the necessary restriction of fishing operations during the war had brought about important changes in the stocks of fish, and that a large-scale experiment had thus been instituted, of which it was important to take prompt advantage. The opportunity was not lost; several men were at work on commercial trawlers in 1919 measuring the fish and establishing the changes in size that had occurred. These changes were particularly noticeable in the plaice, and a year's intensive study of the post-war plaice stocks was carried out in 1920–21 with the aid of a trawler chartered for the purpose. The results were striking—the average size of the plaice was found to have increased, while the growth-rate had fallen off, and curious changes had taken place in the distribution of the various sizes. A summary report on these investigations was published under the title, "The Plaice Fisheries and the War" [3].

During this period also, thanks largely to the energy of Mr. H. G. Maurice, Fisheries Secretary of the Ministry, and Prof. J. Stanley Gardiner, temporarily in charge of the research side, adequate provision was at last made for fishery investigations; the staff was largely augmented, laboratory accommodation was found at Lowestoft, and a powerful ship acquired.

This placing of the research work on a permanent and satisfactory footing was rendered possible by grants from the Development Commission made on the advice of a competent and enlightened Advisory Committee. At the same time adequate provision was made to subsidise the main marine biological stations in the country, particularly that at Plymouth, in order that they might continue to carry out that general work on marine biology and hydrography which is required to supplement the more specialised work of the Fishery Departments. The net result was a new impetus given to marine biology and fishery research in England and Scotland.

II. Organisation, Problems and Methods

The Laboratory at Lowestoft was not specially built for the purpose of fishery research, an existing building on the South Esplanade being adapted for the use of the staff. Part of the statistical staff of the Fisheries Department is also housed at the Laboratory—a convenient arrangement, since much of our work is statistical and in a sense all fishery research is based on fishery statistics. The ship, *George Bligh*, is a Mersey type trawler, built for the Admiralty during the war. Certain alterations were made to provide living and laboratory accommodation for the scientific staff, but the main lay-out and equipment is that of a standard trawler. Her length is 138 ft. overall and net tonnage 132. One is often asked the origin of the name *George Bligh*—she was one of a class of trawlers whose names were taken from the roll-call of Nelson's *Victory*. The ship is kept in commission all the year round, and carries out regular cruises at about fortnightly intervals. Normally the area of operations is the southern half of the North Sea, but on occasions she may go farther afield. Her first trip as a research ship was a voyage of exploration to certain little-known banks off the Western Isles, and an uncharted bank then discovered north of Rockall has been appropriately named *George Bligh Bank*. Occasionally cruises are made down Channel and to the hake grounds south and west of Ireland. Two or three members of the Laboratory staff accompany each cruise, and it is generally possible to accommodate also anyone who is interested in the work and desires to see how it is carried out.

The work at sea consists mainly in the collection of material —measurements of fish, otoliths and scales, plankton samples, water samples, and so on—which are dealt with later at the Laboratory. The general plan of the investigations is to make a thorough study of two or three of the principal species of fish in all their aspects—time and place of spawning, distribution of the young stages, rate of growth, food, migrations. The

study of the fish entails the study of its environment, both physical and biological, and hence fishery research must broaden out to include the study of bottom fauna and plankton, the physical nature of the bottom, tides, currents, and generally the physics and chemistry of the sea. Many different sciences must therefore be brought under contribution. This diversity of the work necessarily leads to specialisation among the staff —some devote themselves mainly to the direct study of the fish, some to plankton, others to hydrography. There are, however, no water-tight compartments ; all are engaged as a team on the same general problems, to the solution of which each contributes his part.

What, then, are these general problems ? Apart from the general acquisition of knowledge, which has a remarkable way of turning out in the long run to be of practical use, the main problems whose solution we seek may be said to be two—the rational and economical exploitation of the fisheries, and the prognostication of good and bad years.

By the rationa lexploitation of the fisheries we mean getting, at the least possible cost, the maximum annual yield of fish that is possible without prejudicing future supplies. This sounds simple, but is in reality an extremely complicated problem ; a full exposition of it would take much more space than is available here. Let me only say that towards its solution all our resources must be brought to bear—elaborate and long-continued statistics of the quantities and sizes landed, extensive measurements at sea on commercial vessels of the actual sizes caught (we have a staff of nine measurers constantly at work), age-determinations of typical samples, study of growth-rate in relation to available food and to the intensity of fishing, direct experiment with different kinds of fishing gear with the object of avoiding the capture of undersized and commercially valueless fish. It is necessary to make a rough census of the fish in respect of abundance, size, age, and growth-rate, every year, and to keep up this census over a long period, in order that the real significance of the changes in the landings may be understood. In fishery research there are no simple little problems which can be disposed of in a month or two and written up in neat little papers ; the problems are all of long range in time and relate to enormous stretches of sea. The means of tackling such big-scale problems are admittedly hardly adequate, but it is necessary to plan broadly and do one's best.

Closely connected with the problem of rational exploitation, both theoretically and in practical methods, is the question of studying the fluctuations in the fisheries. It has long been known in a general way that the stocks of fish may vary considerably from year to year in a manner quite independent

of the amount of fishing. The modern study of these fluctuations dates from the investigations of Johan Hjort and his collaborators on the Norwegian herring. They showed that a good fishery depends upon the occurrence of an exceptionally abundant year-group, which swells the stock and persists for some years, giving rise to a successful fishery. The classical case is that of the 1904 brood of Norwegian herring which predominated for many years and was still traceable after the war. Conversely, if a year-group is much below the average, the fishery in subsequent years is adversely affected. This phenomenon of big variations in the annual influx of new brood has been found to be common to most fish. Very striking results have been obtained by the Scottish investigators on the haddock, and the phenomenon has also been clearly established in the East Anglian herring fishery and in the plaice fisheries of the southern North Sea. For further details reference may be made to a comprehensive paper by Hjort [4]. As things are now, it is possible in some cases, by an age-analysis of the younger fish, to form a pretty good idea of the probable success of the fishery in the next year or two. The stage of actual prognostication has perhaps not yet been reached, but it is very near.

As regards actual methods and gear employed in these researches—it is impossible here to give any full account. The most important gear is undoubtedly the trawl of full commercial size, with a spread of approximately 100 feet. For the capture of small fish, the terminal bag of the net ("cod-end") is covered with fine meshed netting. Dredges and small trawls are occasionally used for special purposes, and fine seines are employed for the collection of tiny fish on beaches and in estuaries. Standard nets and water-bottles are used for the plankton and hydrographical samples, and the somewhat larger planktonic organisms and larval fish are taken by means of the Petersen Young Fish Trawl, a large net made of coarse sacking which can be towed at various depths above the bottom. For quantitative bottom-fauna work the standard instrument is the Petersen "grab," or bottom-sampler, which bites out a known area of bottom soil and brings it up with the animals therein contained. For the hydrographical work some special instruments have been evolved at the Laboratory, notably a surface-sampler which can be towed at full speed by liners (regular observations on temperature and salinity are made for us on several steam-ship routes and from several Light Vessels), and a current indicator, by means of which a regular tally has been kept for several years of the drift of water from the English Channel into the North Sea. (On oceanographical gear and methods generally see 5 and 6.)

Of methods peculiar to, or at least characteristic of, fishery research, may be mentioned the age-determination of fish by counting the rings or zones on the otoliths or scales, and the marking of fish by means of little ebonite or silver discs. Marking can be very successfully carried out on certain fish, particularly plaice, and gives invaluable information as to migrations and as to the toll taken by fishing operations. Marked fish are returned by fishermen who trawl them up, and in some cases a surprisingly high percentage may be returned in the course of a year—30 to 40 per cent. is not unusual in plaice. The use of drift-bottles, surface and bottom, for determining the trend of the currents—a most important factor in the life of all larval fish—is also a routine method extensively used, and recently bottles have been invented at Lowestoft which will float for a pre-determined time and then sink and anchor themselves.

III. Some Results of the Investigations

The results of the Lowestoft researches are published in a series of reports issued by the Stationery Office under the title of " Fishery Investigations," of which seven volumes have appeared since the war. In this series appear also the papers relating to the other branches of fishery research carried out by the Ministry, namely, the shell-fish work at Conway, and the fresh-water investigations based at Alresford and Cambridge. The " Fishery Investigations " series of reports is not very well known, nor is it to be found in many libraries—but this is not the fault of the Department itself. Of recent years many shorter papers and preliminary notes on the Lowestoft work have appeared in the *Journal du Conseil*, published at Copenhagen by the International Council for the Exploration of the Sea.

I shall not attempt to give a catalogue of results achieved, but shall mention merely a few typical instances. By arrangement with our Scottish colleagues, there is a certain division of labour in our fishery work, haddock problems being tackled by Scotland, plaice and cod by England, while both countries investigate the herring in common. Scotland has also, on a more local scale, her plaice problem, and we in England have recently taken up the hake.

Of the plaice it has been said that more is known of its general life-history and habits than of any other animal. Whether this be strictly true or not, it is certain that an immense amount of detailed information is now available about the plaice, the haddock, the herring, and some other fish. It is possible in fishery research, by making use of the facilities afforded by commercial vessels, to gain a really enormous amount of data as to quantities and sizes caught,

where and when they are most abundant, and to rough out the general movements from this source alone. Fish can be, and are, measured in hundreds of thousands, and scales and otoliths collected certainly in thousands. This material is supplemented by the more restricted but more valuable data obtained by the research vessel, and altogether the sampling of the stock is on a scale which is of a different order from that generally achieved in biological research. Not that, considering the vast areas to be covered and the rapid periodic and aperiodic changes in the stock, the scale of sampling can be regarded as really adequate, or as adequate as one could wish. Nevertheless, it remains true that of few animals is such a close census kept as of the plaice in the southern half of the North Sea—their numbers, age-composition, rate of growth, mortality rate, and so on. The object of the work is of course to establish what fluctuations occur in the stock and at what rate the stock is depleted by fishing, in order that measures may be taken to maintain and if possible increase the annual yield. The whole story is not yet known, and in particular the causes of the variations in growth-rate are not fully elucidated, but very considerable contributions have been made since the war by Mr. Borley and Miss Thursby-Pelham [see 7 and 8]. The egg-production of the plaice and the distribution of eggs and larvæ have been elaborately studied by Mr. Buchanan-Wollaston [9].

In connection with the study of the growth-rate of the plaice (and other fish) it is obviously important to investigate the amount and the fluctuations of the available fish-food, and work of this kind was taken up energetically shortly after the war. It was known from the pre-war work that the Dogger Bank was an exceptionally good ground for the growth of plaice, and accordingly several very thorough surveys of this area were made by means of the Petersen Grab. The results disclosed an unexpected and significant state of affairs, for it was found that the two principal bivalve shellfish on the Bank, on which the plaice and haddock fed, occurred in certain years in vast beds, which might be as much as fifty miles long and twenty broad. The distribution within such "patches" was so evenly graded that it was possible to contour them, and estimate with a fair degree of accuracy the total numbers present. One particularly large "patch" was found to have something like 4,500,000,000,000 individuals of the bivalve *Spisula subtruncata*. By repeated surveys it was possible to follow up the fate of some of these patches, and to gain valuable information as to the rate of growth, rate of mortality, and so on of these shellfish. It was conclusively shown that the amount of food on the Bank was subject to enormous fluc-

tuations from year to year. The subject is discussed by Mr. F. M. Davis in two papers [see 10] which form an important contribution to the study of animal ecology. Other work on fish-food, particularly the examination of stomach-contents, is carried on as a normal routine, and has yielded interesting results, especially for cod [11] and hake [12].

The life-cycle of the mature cod in the North Sea has been worked out by a judicious use of market-statistics, measurements, experimental trawling and marking [see Graham, 11] and the very difficult problem of the age-determination of the immature cod has been studied with some measure of success [13]. In connection with the cod investigations an interesting experiment was carried out in correlating the drift of eggs and larvæ with hydrographical conditions. Given the places and times of spawning in early spring, Dr. Carruthers estimated from a knowledge of the actual winds and small experiments with drift-bottles where the larvæ should be found in subsequent months. Concurrent fishing experiments were made at intervals for the larvæ and tiny fish, and good agreement was found between estimated and actual positions of the pelagic fry [see Graham and Carruthers, 14].

Space does not permit of more than a passing reference to the work on hydrography and plankton which is regularly carried on as an integral part of our programme. Since all larval fish are distributed at the mercy of the currents for at least a few weeks of their lives, it is necessary to know in great detail the current-system in the North Sea and its adjacent waters. This has been studied on a considerable scale by means of salinity observations, drift-bottle experiments, and current-meter readings, and the close relation of water-movements with meteorological conditions clearly established [see refs. under 15]. Since some fish, notably the herring, throughout their life, and all fish in their post-larval stages, feed on plankton, the importance of its study is obvious, and notable results have been obtained by Mr. Savage [16] and Mr. Hardy [17].

With regard to the herring—a great deal of solid work had to be carried out to establish the elementary facts regarding spawning places and times on the East Coast of England, the drift of the larvæ, the life-history at the whitebait stage and in the gap between this and the stage when they occur in the big shoals when they are fished for by drift-net and trawl. This was successfully accomplished by Dr. Wallace [18] and Mr. Hodgson, and a regular size and age census is now being kept, which enables us to follow the fluctuations in the yeargroups and to some extent to estimate the future course of the fishery [19].

Finally, I must mention an important practical line of work which is in course of development—the study of fishing

gear, especially the otter trawl and its modern development the Vigneron-Dahl trawl, with the object of establishing the effect of different sizes and shapes of cod-end mesh on the sizes of fish caught. It seems certain that by suitable modification of the mesh a part at least of the present useless destruction of undersized fish can be avoided, with consequent benefit to the yield of the fisheries.

IV. The Relation of Fishery Research to General Biology

It is, I hope, clear from the foregoing account of the methods and results of fishery research that we deal essentially with problems of animal ecology. It is the fish *in relation to its environment* that is the main object of study. Hence the importance of the ancillary studies such as hydrography; hence the need for organised team-work covering other branches of science besides zoology. That fishery research is in a position to contribute to the general science of animal ecology does not seem to be fully realised by the professional ecologist, and more use could undoubtedly be made of our results if they were more fully known to the academic worker. We are in the fortunate position of being able to collect material and data on a very large scale, and so to be of service to the ecologist if he wants our help. To the systematist we can supply, in respect of certain species, extensive data as to distribution, local races, and variation. The " race " problem is actually of considerable practical importance in fishery work, and has been extensively studied. A forthcoming publication of the International Council on this subject, of the nature of a symposium, provides a useful *mise-à-point* of the situation, and contributes towards the solution of the eternal " species-problem."

A word in conclusion about the fishery worker himself. Long experience of fishery investigations has led me to believe that rather special qualifications and mentality are necessary to make a good fishery naturalist. He must be prepared for a great deal of rather humdrum work of a laborious kind; he must be able to stand the rough conditions of work at sea and not be deterred from making his observations by conditions which are often extremely unpleasant; he should be of a practical turn of mind and able to handle men, especially fishermen; he should be able to visualise a problem in a broad way, having due regard to its practical bearings and its position in the scheme of research. These qualifications are not always possessed by men who do brilliantly at academic research—though there are exceptions—and common sense and determination are just as important as an exceptionally high order of intelligence. The fishery man has necessarily got to specialise

and is apt to get out of touch with academic work, but there is a certain fascination in the life, with its opportunities of direct contact with the sea and with the men who do business in great waters.

REFERENCES

1. "Conseil internat. pour l'Exploration de la Mer." *Rapports et Procès-verbaux*, xlvii, Rapport jubilaire, Copenhagen, 1928.
2. E. S. Russell and T. Edser. "The Fishery Statistics of England and Wales." *Journ. Roy. Stat. Soc.*, lxxxviii, pp. 221–50, 1925.
3. J. O. Borley and Collaborators. *Fishery Investigations*, Series II, vol. v, No. 3, 1923.
4. Johan Hjort. "Fluctuations in the Year Classes of Important Food Fishes." *Journal du Conseil, I*, pp. 5–38, 1926.
5. Sir John Murray and Johan Hjort. *The Depths of the Ocean*. London, 1912.
6. G. H. Fowler (Edited by). *Science of the Sea*, London, 1912. (New edition in preparation.)

The following references [7–19] are all to papers in *Fishery Investigations*, Series II.

7. J. O. Borley and D. E. Thursby-Pelham. "Report on English Plaice Investigations, 1921–23," vii, 6, 1925.
8. D. E. Thursby-Pelham. "Report in English Plaice Investigations, 1924–25," x, 3, 1928.
9. H. J. Buchanan-Wollaston. "Plaice-Egg Production in 1920–21," ix, 2, 1926.
10. F. M. Davis. "Quantitative Studies on the Fauna of the Sea Bottom; No. 1, Preliminary Investigation of the Dogger Bank," vi, 2, 1923; "No. 2, Results of Investigations in the Southern North Sea, 1921–24," viii, 4, 1925.
11. Michael Graham. "The Annual Cycle in the Life of the Mature Cod in the North Sea," vi, 6, 1924.
12. C. F. Hickling. "The Natural History of the Hake," Pts. I and II, x, 2, 1927.
13. Michael Graham. "Studies of Age Determination in Fish," Pt. I, xi, 2, 1928.
14. M. Graham and J. N. Carruthers. "The Distribution of Pelagic Stages of the Cod in the North Sea in 1924 in Relation to the System of Currents," viii, 6, 1925.
15. J. R. Lumby. "Salinity and Water Movements in the English Channel and Southern Bight, 1920–23," vii, 7, 1925.
    J. N. Carruthers. "Water Movements in the Southern North Sea," Pt. I, viii, 2, 1925; Pt. II, ix, 3, 1926.
    J. N. Carruthers. "The Flow of Water through the Straits of Dover," xi, 1, 1928.
16. R. E. Savage. "The Plankton of a Herring Ground," ix, 1, 1926.
17. A. C. Hardy. "The Herring in Relation to its Animate Environment," Pt. I, vii, 2, 1924; Pt. II, viii, 7, 1925.
18. W. Wallace. "Young Herring in the Southern North Sea and English Channel," vii, 4, 1924.
19. W. C. Hodgson. "Age, Length, and Maturity of Herring in the Southern North Sea," Pt. I, vii, 8, 1925; Pt. II, viii, 5, 1925.

A very useful guide to the literature on marine and fishery research is Dr. E. J. Allen's "Selected Bibliography of Marine Bionomics and Fishery Investigation," published in *Journal du Conseil*, I, 1926, and also separately, by A. F. Høst, Copenhagen. A current bibliography appears in each number of the *Journal du Conseil*. "The Seas" (London, 1928) by F. S. Russell and C. M. Yonge is an admirable introduction to marine research.

EXPERIMENTAL GRAPHOLOGY

By ROBERT SAUDEK, Ph.D.

For seventy years graphology was known only as an amateurish hobby of intellectual society—it was practised by such eminent persons as Madame de Staël, Disraeli, Sir Walter Scott, Robert Browning, Paul Verlaine, and others—or as a "fake" science practised by professional readers of character from the handwriting.

The scientific treatment of the subject does not date back farther than twenty years. It is true that a German physiologist (of English extraction), Prof. Preyer, wrote a book on the psychology of handwriting as far back as 1897, but his theories have since then been conclusively refuted.

It is only to-day, when we have succeeded experimentally in isolating the various factors of individual letter-formations, that a scientific treatment of the many connected problems relating to the technique, physiology, and psychology of handwriting has become possible.

Before this isolation had been effected the graphologists proceeded from the assumption that since anybody obviously writes "his own hand," his writing must in some way or another be typical of his psychology, or, as one used to say, of his "character." Hence the theory that any alteration, whether in development or degeneration, of a man's "character," must give rise to a corresponding alteration in his handwriting.

Ridiculous as this theory may seem, there still is a grain of truth in it. Were this not so, how could we explain the fact that the amateur graphologists do sometimes succeed in drawing an almost incredibly accurate sketch of a writer's character?

Recent research-work has established the fact that there are at least twelve factors which co-operate in the formation of the individual handwriting. In extreme cases a single one of these twelve factors may be so predominant as to paralyse the influence of all the rest, and we know now that it was only when the amateur graphologists were confronted with some of these rare border-cases that they guessed rightly.

These twelve factors are :

1. *The writing-instruments* (pen, ink, pencil, paper). All the problems arising from these factors have been definitely recognised and solved. We owe our knowledge of the technical structure of handwriting to the microscope.

In all cases of forensic graphology where experts have to identify the authorship of a disputed handwriting this knowledge is of the utmost importance, since it has been established that bad writing-materials may distort the actual form of any handwriting in such a degree as to make its identification practically impossible.

2. *The degree of graphic maturity.* Complete maturity is found only in a handwriting in which the attention is wholly concentrated on the content of what is written ; that is, when the handwriting is not in any way impeded by doubts as to the form which should be given to the letters, or the spelling of the words.

The child, on the occasion of his first writing-lesson, is taught only to make strokes ; a separate impulse is required for the production of each stroke. Then, at a stage somewhat more advanced, he perceives a whole letter as a unit, and writes under a letter-impulse, and later still under a word-impulse ; but generally speaking he does not until his fifteenth year reach a maturity which enables him to write under the impulse of complete phrases.

Every writer can arbitrarily reduce the maturity of his handwriting to a lower stage. That is to say, a person whose handwriting is naturally excellent can produce an illiterate handwriting ; but an illiterate person can never produce a perfectly mature handwriting.

If we have to discover the writer of an anonymous letter, and if a number of persons lie under the suspicion of having written it, and if this anonymous letter is written by a maturely-writing person, we can exclude from our list of suspected persons all those whose ordinary handwriting is immature.

One sure sign of graphic maturity is what is known as " tendency to the right."

Since we write in lines running from left to right our haste is revealed by our endeavouring to arrive as quickly as possible at the end of the actual sentence (or line) of our writing.

" The tendency to the right " may be described as the outward and visible sign of inward and invisible haste ; haste to express the writer's thoughts on paper, unhindered by self-consciousness, vanity, exhibitionism, or æsthetic considerations. The " tendency to the left " betrays hesitation, reluctance, reserve ; here the execution is almost ahead of the thought. Fig. 1 shows the signs of " tendency to the right " and " tendency

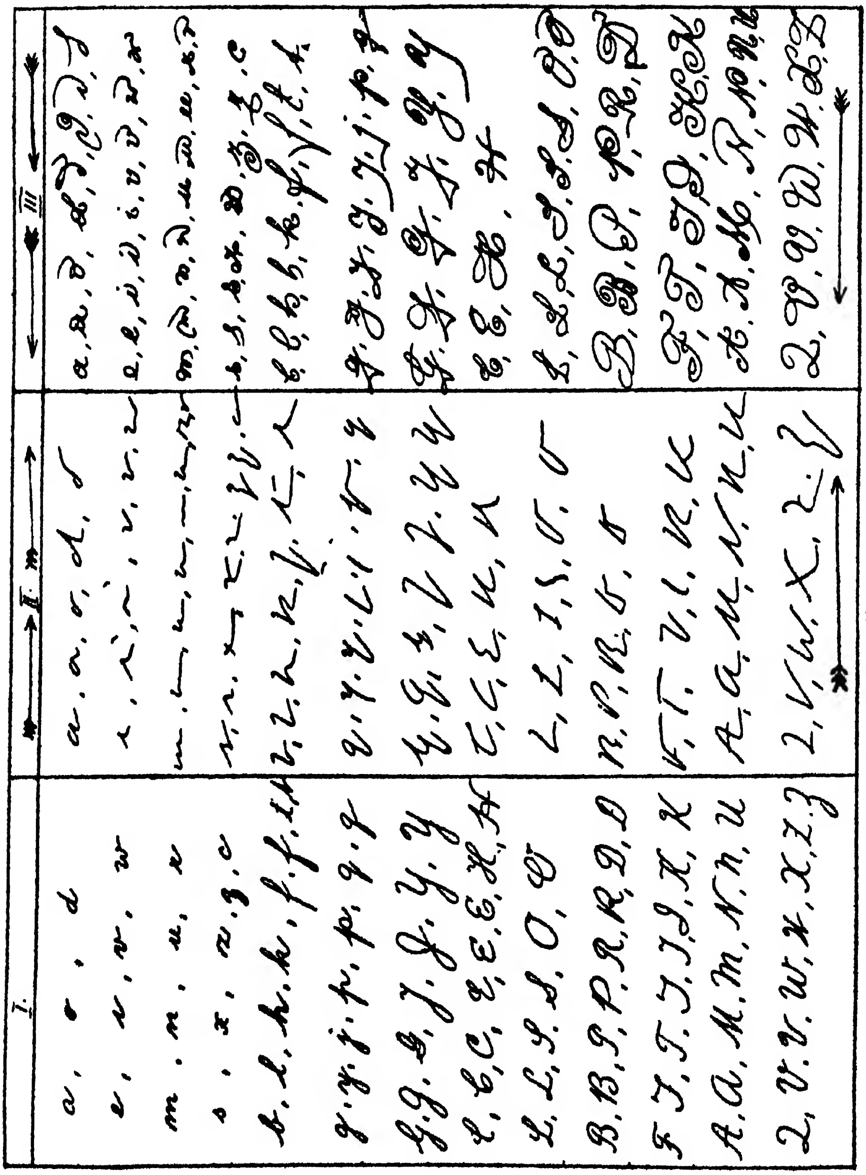


Fig. 1.

to the left"; in the first column are normal letters; in the second column are these letters as modified by "tendency to the right"; in the third column we see them modified and often distorted by "tendency to the left"—and also, in most cases, by a puerile exhibitionism which frequently goes with "tendency to the left," since both phenomena are evidence

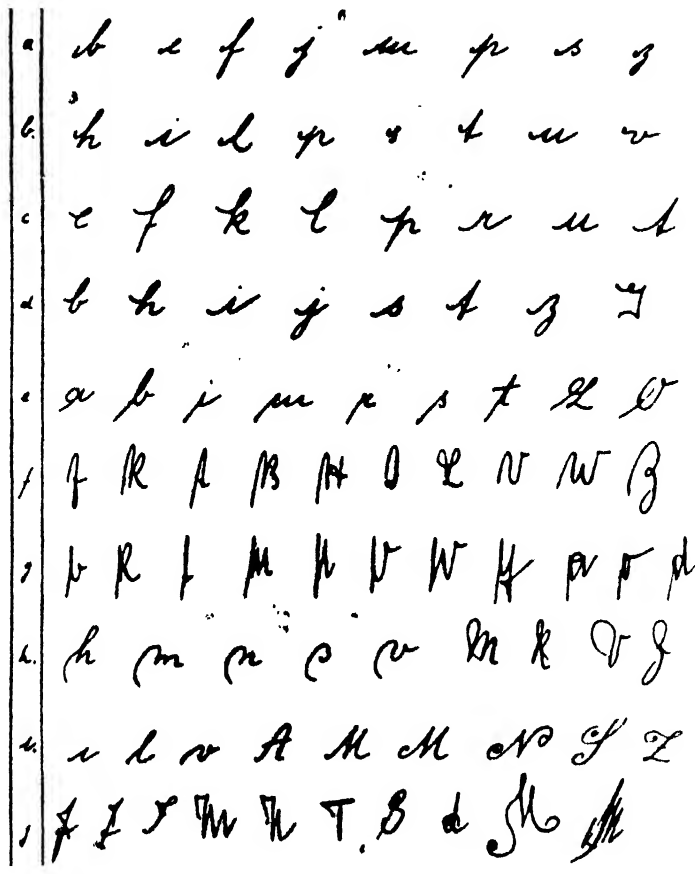


FIG. 2.

either of immaturity, or of a complete absence of sentence-impulse, of spontaneity, of expressive momentum, so to speak, which tempts the writer to mark time by senseless spirals and flourishes.

The prevalence of either of these two tendencies is most plainly revealed by the traces which the nib produces on the paper either when starting a stroke (initial adjustment) or when lifted from the paper (concluding adjustment).

Fig. 2 is a summary table of initial adjustments, arranged

in order of the degree of speed and the difficulty of the adjustment in question. The movement of the nib in the air above the writing surface is traced in dotted lines.

The table begins with the easiest (*a*) and ends with the most difficult initial adjustments (*j*).

(*a*) The movement begins with a pressureless centripetal, slanting down-stroke. (In this case a pre-adjustment was made by a movement of attack in the air above the paper.)

(*b*) The movement begins with a centripetal, slanting, emphasised down-stroke. (In this case there was no preliminary movement in the air, but the writing-movement began when the point of the pen touched the paper.)

(*c*) A pressureless downstroke running from left to right. (In this movement there is usually a preliminary movement in the air.)

(*d*) A downstroke, written with pressure, running from left to right (with no preliminary movement in the air; the writing-movement begins as the point of the pen touches the paper.)

(*e*) A slanting approach-stroke, written with little pressure, produced by a movement of extension.

(*f*) A vertical approach-stroke, written with little pressure, which passes into a curve at the top.

(*g*) A vertical approach-stroke, written with little pressure, which describes an angle at the top; and part of it, or all of it, forms a covering-stroke over the down-stroke.

(*h*) A rounded approach-stroke running upwards from right to left (the leftward-tending movement of attack is contrary to the tendency to the right of a spontaneous act of writing). The mental resistance against the tendency to the right is plainly apparent in the adjustment—that is, where the act of writing is most subject to the caprice of the writer.

(*i*) Adjustment in the form of a dot (there was either a pause for consideration before the beginning of the act of writing, or a hesitating attack, and the apparent dot represents only the result of marking time on the same spot).

(*j*) Examples of complex variations of adjustment.

Fig. 3 is a summary table of concluding adjustments, arranged in order of the degree of speed and the difficulty with which they are produced, so that the last-mentioned kind of adjustment (*k*) is the most difficult.

(*a*) On the left is a handwriting with a high degree of connection. No final adjustment, because the concluding stroke becomes the initial stroke of the following word; on the right we see the same phenomenon, except that here the path from the end of one word to the beginning of the next passes through the air above the surface of the paper.

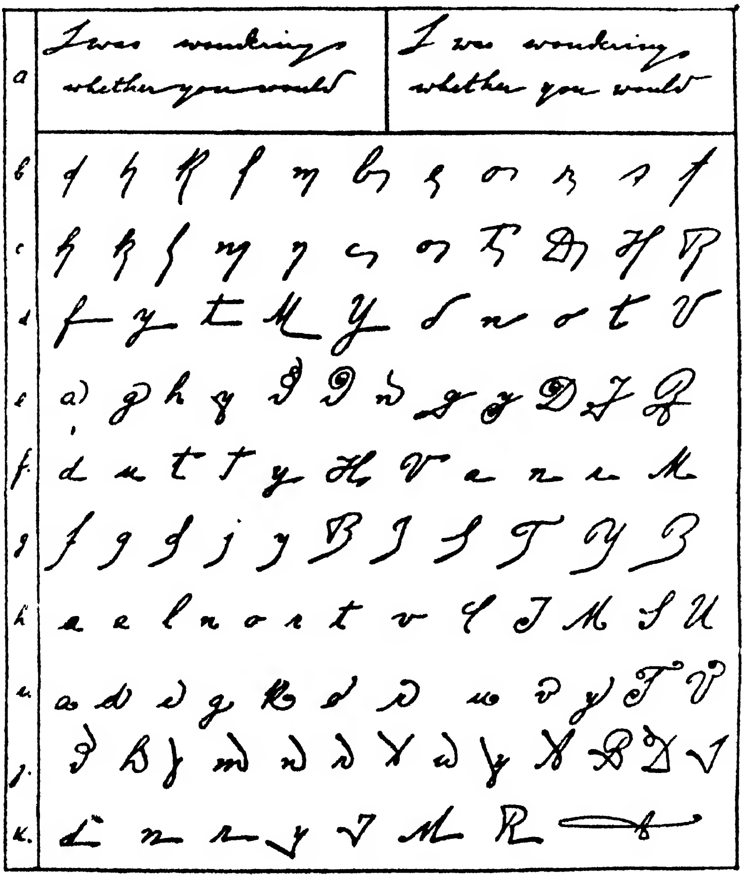


FIG. 3.

(*b*) The movement concludes with a pressureless prolongation downwards of the final stroke.

(*c*) The movement ends with a strongly emphasised prolongation downwards of the final stroke.

(*d*) The movement ends with a strongly emphasised prolongation of the final stroke towards the right. (Persistent affectation.)

(*e*) One or many pressureless and freely terminating

final strokes, bending round towards the left. (Reaction felt during the writing of a word—that is, intellectual indolence, which protests against rapid thinking.)

(*f*) Final strokes turning with a sharp angle to the left. (So-called harpoon-strokes; unwavering perseverance; inflexibility.)

(*g*) Cudgel-shaped, thickened final strokes, running to the left. (Affected final emphasis; ostentation and disputatiousness.)

(*h*) The writing-movement is brought to a standstill by a resting-point. (Need of frequent pauses; inertia; indolence.)

(*i*) Combination of the adjustments described under (*e*) and (*h*). (Extreme inertia, which makes itself felt after each individual word-impulse.)

(*j*) Concluding points after preceding prolongation of the final stroke in the most difficult of all directions—upwards and to the left—with simultaneous increase of pressure (Handwriting of this kind presupposes the overcoming of threefold physical impediments, is possible only in connection with a letter-impulse, and borders on the pathological.)

(*k*) A complicated instance of concluding adjustments; a twofold deflection by sharp angles, with harpoons or concluding points after an angular deflection and an extreme leftward-tending movement. (The significance of this adjustment is that of the momentarily co-operating components, which have been separately dealt with above.)

3. *The acute physiological condition of the writer.* We are able to determine the physiological condition of the writer only by comparing his handwriting at different periods. But if we are provided with such material our general diagnosis is infallible.

Schoolmasters may conclude that the health of their pupils is affected if several recent manuscripts show a maturity inferior to that of previous manuscripts, provided that factor 1 is not responsible for this deviation.

Although this general diagnosis is infallible, we are as yet unable to make differential diagnoses except in certain extreme cases.

4. *Chronic physiological impediments.* We may distinguish between chronic impediments and those which are only acute. The central nervous system adapts itself, after a time, to the loss of the extremity (the hand) with which we have been in the habit of writing, and also to the loss of any sense which normally plays its part in the act of writing.

This is why any acute impediment distorts the handwriting far more than a chronic impediment (and even more than the loss of the sense of sight).

5. *The degree of speed of the act of writing.* (Actual intensity of stroke-impulse, letter-impulse, word-impulse, phrase-impulse.) I have succeeded in discovering certain signs hitherto unrecognised, and in classifying all the signs (several of which have been known for decades) which inform us of the degree of rapidity with which a manuscript was actually produced.

The isolation of these signs has been facilitated:

(*a*) By methodical mass experiments with "normal" and with physiopathological writers;

(*b*) By the use of special apparatus;

(*c*) By the slow-motion cinematograph.

TABLE I

PRIMARY SIGNS OF THE SPEED OF WRITING

Primary Plus Signs (Speed)	*Primary Minus Signs (Slowness)*
1. Smooth and unbroken strokes and rounded forms.	1. Wavering forms and broken strokes.
2. Frequent signs of "tendency to the right" all through the manuscript, alternating with "tendency to the left" at the ends of lines where the end of the sentence occurs in one of the following lines.	2. Frequent signs of "tendency to the left."
(For examples of "tendency to the right" and "tendency to the left" see Fig. 1)	
3. Great uncertainty of aim after temporary interruptions of the act of writing—that is, after syllable- or word-impulses.	3. Conspicuous certainty of aim with scarcely perceptible deviations from the intended direction of motion.
4. Increased continuity of execution; for example, the connection of diacritical signs with the following letter, the joining together of words, the joining together of numerals in the same group of figures, etc.	4. Frequent pauses during execution, recognisable by meaningless blobs, blobs due to readjustment, angles, divided letters and unrhythmical separations within the word itself (English script excepted), and "touching up" of letters.
5. Letters curtailed and degenerated almost to illegibility towards the end of words.	5. Careful execution of significant details of letter-forms and amplification of strokes towards the ends of words.
6. "Primarily wide" script (especially in the case of vertical handwriting).	6. "Primarily narrow" script (especially in slanting handwriting).
7. Great difference of emphasis between up-strokes and down-strokes.	7. Hardly any difference in strength of up-strokes and down-strokes; writing produced with very little pressure, or "pasty" writing.
8. Widening of the left-hand margin as the writing proceeds.	8. Ornamental or flourishing connections.

TABLE II

Secondary Plus Signs (Speed)	*Secondary Minus Signs (Slowness)*
(Secondary signs increase or diminish the significance of the primary signs. They render possible a graduated estimate of the *tempo* and the spontaneity of the act of writing.)	
1. (*a*) Increasing obliquity as compared with normal angle of school-copy. (*b*) Increasing tendency to reversed angle (over 90 per cent.) when the school-copy was vertical, and to a lateral grip of the pen.	1. Down-strokes parallel almost as in school-copy.
2. "Secondarily wide" handwriting (especially if the script is at the same time "primarily narrow").	2. "Secondarily narrow" handwriting (especially if the script is at the same time "primarily wide").
3. Rising lines with paper and pen at normal angles.	3. Sinking of the lines (well marked from the beginning of the line, not only during the course of the line).
4. Infrequent changes of angle of writing.	4. Frequent changes of angle of writing.

There are eight primary signs of speed and eight primary signs of slowness:

Four secondary signs of speed and four secondary signs of slowness; and

Five equivocal signs.

The sixteen primary signs are valid for any handwriting.

The eight secondary signs are valid only if they have not been deliberately produced (for example, we may produce a rising line by altering the position of our paper).

The five equivocal signs must be employed only in order to reinforce the significance of the primary signs which occur in a given handwriting.

The reader who is familiar with the general literature of graphology but knows nothing as yet of experimental graphology may be surprised to find that the usual symmetry in the arrangement of the signs of rapid and slow handwriting is not observed; that whereas the eighth plus sign is "widening of the left-hand margin," the eighth minus sign is not "narrowing of the left-hand margin," but something quite different, namely, "ornamental or flourishing connections." We very often encounter a similar lack of symmetry, the explanation being that experimental graphology proceeds from approved and demonstrated facts and not from dialectical conceptions by which all the phenomena of life may be easily and conveniently divided, like sheep and goats, into two contrasted groups.

It will be seen that *width* is one of the principal signs of speed. As we have already seen, the more powerful the forward impetus of our thoughts while we are writing (or, in our terminology, the stronger the sentence-impulse) the more vigorous is our movement from left to right, to the end of the word, the end of the line, the end of the sentence. Thereby the so-called width of our script is increased—that is, the distance between the individual downstrokes of the small letters *n, m, u, w*, is increased, as they tend to shift to the right. We call this "primary width." But if we control this movement to the right for the sake of greater definiteness in the formation of our letters, we write these small letters in a narrow script, though in other respects, if the legibility of our script is not diminished thereby, we surrender to our impulse to the right, and make the connecting-strokes between our letters considerably wider; and this we call "secondary width." Primary width is a primary sign of speed; secondary width a secondary sign of speed. Primary narrowness is a primary sign of slow writing; secondary narrowness is a secondary sign of slow writing.

This rule, however, is subject to a twofold limitation. We must in every case examine whether these signs of primary and secondary width or primary and secondary narrowness occur in vertical or in slanting handwriting. It is an essential characteristic of vertical handwriting that it is normally narrow. If it is nevertheless abnormally wide, this width is an individual sign of a particularly intense sentence-impulse. If it is normally narrow, then, in our terminology, it is not a narrow handwriting at all, for narrow handwriting occurs only when abnormal narrowness appears in a slanting hand, this being contrary to the normal character of the hand.

To so-called "script," and to disconnected, typographical, upright handwriting, none of these rules apply, because in these the impulse to the right is not decisive; not only do æsthetic considerations and the graphic significance of the word constitute deciding factors, but the vertical tendency of the script counteracts the tendency to the right.

Further, we may lay down the following doctrine:

> A comparatively rare alteration of the pen-hold is always a sign of rapid writing; a comparatively frequent alteration of the pen-hold is always a sign of slow writing.

(The change of pen-hold may be noted in the varying adjustments of the pen on its application to the paper [initial adjustments] and in the application of pressure in shadow-strokes. If the student of graphology will place under his writing-paper a sheet of carbon tissue, and under that another

sheet of paper, he will find that the carbon copy, provided he writes with a broad, soft nib, will teach him more about "insertions" and adjustments than pages of description.)

Nevertheless, we have placed both these signs not in the table of the primary signs, but in that of the secondary signs; and the reason for this is that the problem is complicated by another consideration of a physical and mechanical nature.

When we begin to write a line we adopt a particular individual attitude of the body; we sit at our writing-table in a particular manner, and adjust the position of our forearm on the table. If we now write in great haste, or with unusually strong emotion, connected with the content of our writing—that is, with a most intense sentence-impulse—then we do not, in normal fashion, take time to move the arm farther to the right, but the position of the elbow remains fixed; and we write (apart from secondary movements) in a wide curve, in the segment of a circle whose radius is equal to the distance from the point of the pen on the paper to the point where the elbow rests. The movements to either side of the main direction—that is, those which form the letters—are principally or exclusively conditioned by the movements of the fingers and the turning of the hand.

If we write in this manner in the normal position on a sheet of narrow letter-paper (of about 5 inches in width) we obtain a rising line; if we write on paper of double width the line rises till about the middle, and then falls.

For this reason, in rising lines on a narrow writing-surface alterations of the hold are always very rarely observed. The only alteration that does occur is due, not to a readjustment, but to a continual pronation of the hand during the writing of the line.

It may also happen that the hold is very rarely adjusted, yet, nevertheless, there are no rising lines. This is the case when the impulse from the central nervous system, and the physical reaction of the muscles to the impulse (not having as yet degenerated), function with complete simultaneity, so that there is no feeling of impediment during the over-hasty writing of the lines, and so that no need is felt of dispelling any possible incipient fatigue by a change of hold.

It follows, therefore, that infrequent changes of hold, together with rising lines, constitute an irrefutable proof of rapid writing, and are indeed equivalent to a primary sign of speed; while either of these two signs individually, when it does not occur simultaneously with the other, has merely the significance of a secondary sign.

This doctrine, however, is subject to a reservation if rising lines only are in question, since these can be produced in an

artificial manner, by placing the paper at an angle with the edge of the desk or table. And here yet another reservation must be noted. Persons who habitually write an *extremely* sloping hand may place the paper so far to the right that the line begins to fall from the first, or almost from the first. The simplest way of correcting this fall is a pronation of the hand which has the effect of still further reducing the writing-angle. In the case of an intense sentence-impulse this accommodation may be neglected. If, therefore, we find falling lines in an extremely slanting handwriting without a considerable increase of the slope towards the end of the line, we may regard the falling line *in this case* as a sign of speed.

Mass experiments have proved that in the rapid production of slanting handwriting the writing-angle is *never* increased towards the end of words and during the act of writing, but that it is frequently diminished—that is, the obliquity is increased.

On the other hand, vertical writing (with a writing-angle of 90°) frequently changes, when written quickly, towards the end of words, and during the act of writing, into a slanting hand with an angle of 80°, while backhand writing (with an angle of more than 90°), when written quickly, assumes a more obtuse angle.

Both appearances must therefore be reckoned as secondary signs of speed.

On the other hand, an approximate parallelism of the down-strokes can be produced only by exceptionally careful "copper-plate" calligraphy. We shall, therefore, reckon this sign to be a secondary sign of slowness.

It is characteristic of the writing-movement from left to right, and, as we have once more seen, of the functioning of all the organs which take part in the act of writing, that under normal conditions, in rapid writing, if the position of the paper is more or less normal, the lines cannot sink from the very beginning. With the exception already noted, a sinking line in rapid writing can only be produced artificially by deliberately placing the paper at a quite unaccustomed angle and then writing lines which will really be "straight," but which will be sinking lines when considered in relation to the top of the paper. Such a case may be regarded as non-existent.

A line sinking from the beginning is almost indubitably a sign of slow execution, but there are two causes beyond that already noted which may be responsible for this appearance: namely, physical impediments, as a result of which the forearm is pressed closer and closer to the body, whereby on the one hand a sinking line may be automatically produced, and on the other hand the position of the body will frequently

be readjusted; and mental factors—for example, severe mental depression—which may occur with a temporary inhibition of the natural *tonus* of the muscles.

Since the true cause of the sinking line can be ascertained only by means of concomitant appearances, we are not justified in regarding this sign as absolutely decisive—that is, as primary. We shall therefore note it down—except in the case of an extremely slanting hand—as a secondary sign of slowness.

It is inherent in the essential character of the natural play of our muscles—that is, their reaction to the nervous impulse—that our writing is easiest and quickest when there is a rhythmical alternation of contraction and relaxation. But since every contraction produces a writing-pressure, by which the two elastic points of the nib or quill are forced to yield and separate, thus facilitating a greater flow of ink, and producing a broader furrow, so that the track of the pen on the paper takes the form of a shadow-stroke, which, by the following muscular relaxation and lessened pressure, changes into a hair-stroke, we must reckon a rhythmical alternation of thick and thin strokes as a primary sign of speed. And correspondingly, a handwriting in which this difference of emphasis in strokes running in different directions does not occur must be regarded as having been produced by constant contraction or constant extension, but not by the alternation of the two; it is a slow and restrained writing. But a complete extension of the fingers lessens the angle of inclination between pen and paper; whereby not merely the point of the pen, but a larger surface is brought into contact with the paper, and a greater flow of ink is produced—not merely greater, but more regular. A handwriting thus produced with a great flow of ink, in which the difference of shading between hair- and shadow-strokes disappears, we will call a *pasty* handwriting. It is more slowly produced than a script with a regular alternation of emphasis. Its pastiness is increased by a so-called "long hold" of the pen, that is, a hold in which the tips of the fingers are farther than usual from the point of the pen, so that the angle inclination is further diminished and a correspondingly greater flow of ink produced.

6. *The school-copy from which we learned to write.* This factor has been isolated by two methods:

(*a*) By mass experiments in English schools which employ the typographic system of handwriting (script), in which children of ten years, although they had been writing a cursive hand for four years, had to employ the typographic system. The statistics in question comprise more than 50,000 cases.

(*b*) By experiments facilitated by the Governments of several great nations, whose consulates collected the manuscripts of nationals who received their primary education abroad in an alphabet other than their national alphabet.

7. *The nationality of the writer and his present national environment.* Almost all persons who emigrate to the United States when they have already attained complete graphic maturity adopt, after a few years, the characteristic peculiarities of the American alphabet (see also factors 8 and 9).

8. *The degree of visual sensibility.* The intensity of factor 7 depends on the intensity of this factor 8.

The isolation of this factor was possible only after the Great War, which has, alas! provided us with many clinical cases of soldiers who have lost their sight, and whose former "sighted" handwriting can be compared with their present "blind" handwriting.

The loss of the eyesight makes it impossible to receive fresh visual impressions. This is why persons who have lost their sight never subsequently modify the formation of their letters. Their visual memory represents their former handwriting more and more distinctly, until it crystallises in their imagination into a permanent image.

9. *The faculty of graphic expression.* This depends on the visual memory, the æsthetic culture, and the manual dexterity of the writer.

10. *The intensity of the writer's vanity, affectation, and imitativeness on the one hand, and his naturalness, simplicity, and sincerity on the other.*

Only completely natural persons write with a completely unrestricted sentence-impulse. Every trace of "giving oneself airs," every degree of vanity, beginning with the exhibition of æsthetic culture and ending with an almost pathological self-importance, is revealed by the deviations from the natural handwriting.

We recognise the simplicity and "sincerity" of the handwriting on the one hand, or the expression of the writer's self-importance on the other, by four general signs:

(*a*) The speed of the act of writing.

The more quickly we write, the less capable are we of giving attention to our calligraphy:

(*b*) The fact that there are several primary signs of speed and of slowness contradicting one another in the same manuscript. This proves that the handwriting has not been produced in a natural way, but with deliberate concentration on its calligraphic virtues.

(*c*) Initial accentuation.

The writer's attention to his calligraphy diminishes in proportion to the speed of writing.

Since a writer who has attained graphic maturity perceives whole words as units, and since his mental concentration diminishes as he is writing each of these units, it follows that in general he can only write the beginnings of words carefully, and not whole words.

(*d*) The same principle applies to all portions of the handwriting which impress themselves on the writer's attention.

The facility with which he gives artificial forms to his letters depends absolutely on the singularity of these formations. Those portions of the handwriting which do not in any way attract the writer's attention are *never* written in an ostentatious manner.

It has been possible to determine precisely the intensity with which each integral component of the handwriting draws attention to itself.

The method of this experiment consists in slowly and progressively approaching the handwriting to the observer, whose function it is to describe the handwriting. We distinguish between :

(*a*) Its appearance at a distance (before its characteristics can be distinguished in detail).

(*b*) The appearance of the manuscript at a shorter distance, but still without reading the text.

(*c*) The description of all the peculiarities of the handwriting after the manuscript has been read.

The degree of singularity or conspicuousness of any trait is determined by the stage of the experiment at which this peculiarity has been perceived and described by the observer.

The peculiarities which have not been perceived and described are classed as peculiarities which appear negligible and which for this very reason are *always* neglected, that is to say, are regarded as having been written in a natural and habitual manner.

In the case of forged manuscripts, the assumption of an alien style may be perfectly successful in the conspicuous features, but less so in the inconspicuous features, or these will inevitably reveal some or all of the characteristics of the writer's natural hand, by which his identity can be established by the expert graphologist.

11. *Intellectual culture, knowledge of foreign languages, foreign countries, and foreign systems of handwriting.* The writer's sensibility for and his visual memory of alien fashions

cannot be responsible for the imitation of exotic handwritings unless the writer has had the opportunity of assimilating their appearance during a sojourn abroad.

12. *The actual position of a letter in the manuscript.* The actual formation of a letter varies accordingly as this letter stands at the beginning or in the middle or at the end of a word, or quite alone. Its form, then, depends on the direction of two movements: the movement preceding and the movement following its formation.

This classification of twelve factors has been made without special consideration of psychopathological factors.

Fifteen laws of handwriting have been definitely established with the aid of the slow-motion cinematograph and other means.

We must distinguish between:

(*a*) Three laws which were known previously, but which have been confirmed by the cinematograph (1–3).

(*b*) Two laws whose existence was suspected, but which were discovered and confirmed only by the cinematograph.

(*c*) Six laws which were quite unknown, but which were revealed and definitely established by the cinematograph (6–11).

(*d*) Four laws which were revealed and established by other means (12–15).

The Fifteen Laws of the Writing-movement

1. No one can make angular strokes without checking the pen for an instant (generally for not less than $\frac{3}{10}$ second—Freeman, Chicago, 1923) on passing from one direction to another (Preyer, Wiesbaden, 1895).

2. When the speed increases the obliquity of the shadow-strokes increases also up to 10° (Meyer, Berlin, 1900; confirmed by McAllister, Yale, 1913).

3. We all write with a speed which changes during every microscopic portion of the stroke. The speed accelerates towards the middle of a stroke and is retarded towards the end (Binet and Courtier, Paris, 1893).

4. A technical obstacle (for example, a paper-fibre in the pen) retards the speed only by reason of visual irritation. The speed of the writing of blind persons is not retarded by such obstacles (Saudek).

5. The movement is retarded when we increase the pressure of the fingers on the penholder. This retardation, however, is not due to the increased friction of the pen on the paper (as

was mistakenly believed) but to a fresh impulse and a fresh adjustment of the fingers (Saudek).

6. *We write long strokes always more rapidly than short ones.* (Our movements are subject not to the laws of physics but to the laws of physiology.) Moreover, this acceleration is distributed over the writing-track of the whole letter ; for example, we write the letters *a*, *d*, *q* in precisely the same time, although the letters *d* and *q* contain a longer writing-track than the letter *a* ; and the acceleration has already commenced in the oval (Freeman, Chicago, 1923).

7. Each change of direction of a movement causes a retardation of this movement (Freeman, Chicago, 1923).

8. This retardation due to the change of direction is not absolutely habitual to the writer, but depends partly on the form of the preceding or the following letter (see the twelve factors which co-operate in the formation of the handwriting) or on the fact that the letter in question stands alone (Saudek).

9. Directly our pen begins to move the impulse of a fresh movement is manifested. This fact causes a loss of time necessary for the fresh adjustment (Saudek). Nobody is able immediately to start a fresh writing-movement without a certain loss of time necessary for the new adjustment of his pen on the paper. This is why in a rapid handwriting one avoids interruptions of the writing-movement as far as possible.

In a rapid handwriting the pen will move directly from the end of one word or group of letters to the beginning of the next word or group of letters : not in a straight line, but in such a way as to attach the first letter of the new word or group without loss of time. The adjustment of the pen to the first movement of this letter may take place wholly or partly in the air ; but as a general thing it leaves indications of its movement in the form of traces which we call end-adjustments and initial adjustments (see Figs. 2 and 3). If the writer is not writing under a full sentence-impulse he may rest or hesitate between his words, and this hesitation or deliberation may betray itself in a modification of the adjustments. But the adjustment is particularly useful to the graphologist in betraying preoccupation with æsthetic considerations, servile adherence to a school-copy, traces of an earlier style of writing, not consistent with the present style, and above all affectation, vanity, self-importance, and the desire to make an impression. And if an apparently mature character contains elements of puerility, this puerility is certain to reveal itself in the adjustments. Some adjustments are pathological symptoms ; they betray the writer's difficulty in applying his pen lightly and steadily to the desired spot ; others betray boredom, abstraction, or mental exhaustion ; the writer begins

to write before he is really certain of what he intends to write; such adjustments are of the same nature as resting-points. Whatever the symptomatic value of the adjustment, it must of course be considered in relation to all the other features of the handwriting under examination.

In Figs. 2 and 3 we see a variety of initial and end-adjustments respectively arranged in the order of the difficulty of production. Some of these have a special symptomatic significance; for example, the left-ward-tending tail to the *y* and *z* in line *g* of Fig. 3 is typical of shy and reclusive persons who shrink from life and are not very certain of what they are about to say; and the long flourish and hooked ending of the last letter of this table are indicative of vanity and a desire to make an impression. The principal value of such an arrangement of adjustments in order of difficulty is that it gives us a valuable means of measuring the degree of speed of the writing-movement in different passages—that is, the prevalence of a stroke-impulse, letter-impulse, word-impulse or sentence-impulse, as the case may be.

In considering the significance of an adjustment we must pay attention to its position in the sentence and on the page. The end-adjustment of the *g* in the word *wondering* at the top of Fig. 3 is justified by the fact that the pen had to be brought quickly downwards to the left for the beginning of the next line. In the middle of a line it would obviously have a different value. And the flourishing end-adjustment of the A at the end of line *k* would be innocuous enough if the writer were signing a letter with his initial; it would then express satisfaction at the completion of a task, or the desire to give an æsthetic finish to the close of the document—to write, so to speak, a sort of ornamental full stop.

10. More time is required for writing a dot than for writing a short stroke—for example, a comma (Saudek). In order to make an actual dot we must arrest the movement of the pen, otherwise we write an accent. This is why in a rapid handwriting we always find "dots" which are written as accents (Crépieux-Jamin).

The uncertainty of aim which is one symptom of speed is revealed principally in the i-dots and t-bars. In rapid writing these are placed too high or too low, and usually to the right of their respective letters if placed during the writing of the word, revealing the "tendency to the right" characteristic of the sentence-impulse. The movement of the pen in placing the i-dots is revealed by the shape of the dots, which are not really points, but, as we have seen, accents. It must be remembered that some writers place their i-dots after writing the whole lines, and even in reverse order—*i.e.* from right to left.

11. "Covering-strokes" can be written only with a retarded movement. The distance between two shadow-strokes (for example, in the letter *n*) is reduced to zero, which means that the dextrogyric movement ("tendency to the right") has been completely arrested (Saudek).

12. A slanting handwriting (75°–55°) is produced more easily and rapidly than a reversive handwriting (91°–125°) (Judd and McAllister, Yale).

13. The lower projections (as in *g*) are written more easily than the upper projections (as in *b*) (Judd and McAllister, Yale).

14. A rhythmical distinction of the shading in the hair-strokes and shadow-strokes of the whole manuscript is possible only if all the muscles of the hand are functioning with their proper vigour and stability (Saudek).

15. Every completely mature writer exerts a pressure which is personally typical and which can be registered by a pen specially devised for the purpose. Although the curve inscribed with such a pen is absolutely individual, it changes within certain limits in proportion to the speed of the handwriting at the moment (Drever, Edinburgh; but not Kraepelin). Kraepelin has measured only the static pressure (in writing a point), but Drever the dynamic pressure (in writing a whole sentence).

The characterological analysis of a handwriting must proceed in accordance with a method of analysis and synthesis which is too elaborate to describe within the scope of this paper.[1] The forensic analysis of a handwriting proceeds on different lines, the aim of the graphologist being (*a*) to discover whether the document in question is the work of its reputed writer, and (*b*) if it is not, to prove the identity of the handwriting in its inconspicuous characters with that of the suspected person, or to discover which one of a number of suspected persons has produced the forgery. In all cases the expert relies on the fact that the inconspicuous features of handwriting cannot be consistently disguised in a manuscript of any length, and very often they are not consistently disguised even during the writing of a single word.

[1] This method is described at length in my "Psychology of Handwriting" (1925), and in "Experiments with Handwriting" (1928)—both published by G. Allen & Unwin, London, and Doubleday, Page, New York.

ESSAY

WEEDS IN THE ECONOMY OF AGRICULTURE (H. C. Long, B.Sc.).

THE harm done by weeds to farm and garden crops has been recognised from ancient times, and has been touched on very extensively in our literature, agricultural and otherwise, for several hundred years. For example, in his *Five Hundreth Points of Good Husbandry*, first published in 1557, Thomas Tusser says:

> " Slack never thy weeding, for dearth nor for cheap,
> The corn shall reward it, ere ever ye reap."

Again he observes:

> " In May get a weedhook, a crotch and a glove,
> And weed out such weeds, as the corn doth not love:
> For weeding of winter corn, now it is best;
> But June is the better for weeding the rest."

Shakespeare (*Richard II*) says:

> " I will go root away
> The noisome weeds, that without profit suck
> The soil's fertility from wholesome flowers."

In 1731 Jethro Tull, in *The Horse Hoeing Husbandry*, writes:

> " It is needless to go about to compute the value of the damage weeds do, since all experienced husbandmen know it to be very great, and would unanimously agree to extirpate their whole race as entirely as in England they have done the wolves, though much more innocent and less rapacious than weeds."

In 1756 Thomas Hale, in *A Compleat Body of Husbandry*, observes that—

> " Everything that grows without being sown or planted, among a Crop that has been sown or planted, is in that Place a Weed. The whole Benefit of the Tillage was intended for the Crop, and this robs it of a Part."

These quotations make it abundantly clear that there was no lack of recognition on the part of certain individuals that much loss of agricultural crops was due to weed infestation. Nevertheless, it is observed in the introduction to Benjamin

Holdich's *Essay on the Weeds of Agriculture*, published early last century, that "It has happened (it seems very strangely) that an Essay on Weeds and their destruction has never been published." This claim is apparently due to the fact that Pitt's essay, published in the fifth volume of the *Communications to the Board of Agriculture* (1806), was regarded as containing "A pretty long catalogue, but with many important omissions and without any practical arrangement." Pitt's list appears to enumerate fifty-five species of weeds, but it would be no great difficulty to name some three hundred. It is, however, the fact that the vast majority of the said three hundred must be regarded as of small importance except in a few local instances, and perhaps most farmers having experience in only one district might find it difficult to name a score which cause them any serious trouble.

Since Holdich's essay was published a considerable number of articles and papers on weeds have appeared in the agricultural annuals, and at the present time there are available two or three privately published books as well as official publications of the Ministry of Agriculture and Fisheries. There are, further, publications dealing with poisonous plants from the standpoint of the farmer and his live stock.

We may now usefully consider just in what way weeds are injurious and the extent of the damage they do. It may be said that in the first place weeds occupy the soil at the expense of the cultivated crop, and to that extent do not afford the crop adequate space for its development. In this connection the effect is that the cultivated crop is deprived of air, and the sun's light and heat. This, of course, has the effect of reducing the ability of the plant to develop the chlorophyll required in the process of photosynthesis. It also has the effect of delaying ripening of crops, particularly cereals, which take longer to dry, when cut and stooked with many weeds. In addition, it needs to be borne in mind that the weeds transpire soil moisture in exactly the same way as the crop, and, therefore, tend to exhaust the soil more rapidly of moisture. It has been found, for example, that cultivated crops may transpire during the course of their growth to maturity from 300 to perhaps 1,000 tons of water per acre. Still further, soluble plant food which might otherwise be utilised by the crop is stolen by the weeds, and is at any rate lost for the time being.

Taking everything into consideration, therefore, the crop may suffer very badly, and the weeds have only to be present in sufficient quantity to cause very severe loss. For example, many years ago at Rothamsted a plot of wheat was left uncultivated and untreated and was allowed to seed and reproduce itself from year to year. In the second year it produced only

about half a crop, and the yield was reduced so rapidly that only two or three stunted plants appeared in the fourth season, while in the following year the wheat had entirely disappeared. A number of investigations during the past twenty years have shown that the percentage loss of crop or financial loss due to the presence of weeds may be 50 per cent. of the whole. This has been ascertained by comparisons between plots of the same crop treated to keep down weeds and others left untreated. As a case in point it may be said that at the University College Farm, Reading, a trial made twenty years ago showed that a crop of mangolds left unweeded after the plants were singled yielded only 16½ tons per acre on a two-year average, whereas on an adjoining plot a single hoeing resulted in a yield of 31 tons, and with two hoeings the yield rose to 37½ tons.

It was estimated before the war that the loss due to weeds in Britain was probably not less than 16 millions sterling per annum, partly owing to direct loss of crop and partly to the extra cultivations and consequent expenditure in combating the weeds. This loss is almost certainly now much greater. It may be suggested that a cost of at least *7s. 6d.* per acre on grassland and *20s.* per acre on arable land is not excessive.

Investigation in the direction of weed eradication has taken several forms. In the first place advance in agricultural machinery and implements with a special view to weed eradication have led to considerable improvements. The influence of fertilisers and lime in reducing weeds has also received close attention, while the possibility of destroying weeds by applications of liquid sprays or dry powders has been widely investigated. Tillage and tillage implements can hardly be touched upon here.

As regards lime, it is well known to botanists that many plants occur most extensively on the chalk, whereas others are seldom so found. So considerable, in fact, is the influence of lime on certain weeds that some of them, such as spurrey, corn marigold, bracken and sheep's sorrel, are likely to be greatly reduced within a comparatively short period—say from one to five years—consequent upon a heavy application of lime. Where such weeds occur, therefore, liming is one of the first steps to be adopted in order to reduce them.

The influence of fertilisers on grassland in particular has received attention for thirty years past. Where complete fertilisers—nitrogen, phosphates and potash—are used, considerable improvement in the herbage is practically certain, provided the management and system of grazing are sound. Phosphates have a very considerable effect in encouraging the growth of clover, which not only increases the value of the

herbage for stock but tends to reduce inferior or weed plants, but by the introduction of nitrogen from the air through the root nodules tends to the accumulation of nitrogen and consequent increased fertility. The use of nitrogenous fertilisers on grassland is unsatisfactory without minerals, as it tends to encourage inferior herbage, such as *Agrostis* and ribwort plantain. Potash has been found to reduce the growth of *Agrostis*, as also has superphosphate. Although, as stated above, lime consistently reduces the amount of bracken and sheep's sorrel, as it also does *Agrostis* and Yorkshire fog, it nevertheless increases the percentage of ribwort plantain.

As regards the influence of spraying on weeds, a 2 to 4 per cent. solution of copper sulphate or a 15 per cent. solution of iron sulphate has long been used to destroy charlock in corn crops, the charlock being destroyed while the cereal is practically undamaged. Such spraying also has a considerable effect in reducing certain other weeds, some of which, like persicaria and spurrey, it may destroy, whilst others, like thistles, it will severely damage and perhaps inhibit their production of seed. In recent years it has been discovered that an application of very finely powdered kainit, applied in the dry state when the dew is on the leaf, is very effective in destroying charlock in corn crops. Both sulphate of ammonia and calcium cyanamide are also to some extent effective against the same weed. Apart from their direct action, all these fertilisers also have a valuable effect in reducing weeds by serving as a stimulant to the corn crop and enabling it by its more vigorous growth to suppress at least the more lowly weed species.

When one considers the weeds which grow on paths and drives, there is somewhat more scope in using plant poisons, and various applications—of salt, copper sulphate, iron sulphate, arsenical preparations, hydrochloric acid, sulphuric acid, washing soda, and carbolic acid—have all been found more or less effective. A recent investigation at Aberdeen has shown that an application of a 4 per cent. solution of copper sulphate may be expected to keep paths and drives free from weeds for a year or two.

NOTES

Catalytic Action in the Mercury Break (A. P.)

The " make and break " in an induction coil is often brought about by a very fine stream of mercury impinging on vanes of such a good electrical conductor as copper, rotating at high speed, in an inert atmosphere. Usually, coal gas is used. Owing to the speed of the vanes the mercury is broken up into extremely small particles, but collects on the bottom of the apparatus as a finely divided, heavy, black powder. This powder has a very penetrating and somewhat objectionable odour and is also greasy to the touch. Careful distillation up to 180 C. yields a heavy, yellow oil with an extremely pungent smell not unlike that of acetylene. Extraction of the powder with carbon tetra-chloride, with subsequent evaporation of the solvent, yields a very similar product and this is perhaps the best way for obtaining the oil.

Careful distillation of the oil shows that it is not a simple compound, but probably a mixture of two or more liquids. A solution of the substance in carbon tetra-chloride decolorises bromine, thus suggesting unsaturated compounds and the odour suggests the presence of higher acetylenes. Sufficient material has so far not been available to actually identify these, and there are no references to the action in books on organic chemistry.

After removal of the oil there is left a black mass of mercury and carbon. The mercury may be distilled off, but all trace of the oil must be removed by solvents if the mercury is desired in an odourless condition.

The mechanism of the action in the commutator may be somewhat as follows. The coal gas is decomposed by the electric sparks into finely divided carbon and the mercury particles, also extremely small, are adsorbed at the carbon surface. Owing to the large surface area of the carbon-mercury phase, catalytic action is very probable, which results in the formation of the liquid referred to above.

Information about the above phenomenon should prove interesting.

Czechoslovak Scientific and Contemporary Culture (Gerald Druce)

This being the tenth year of Czechoslovakia's independence, the educational authorities have held at Brno, in Moravia, an

Exhibition of Contemporary Culture to emphasise the scientific and general progress made during these ten years. The promoters of the Exhibition were strongly of the opinion that general culture, technical and industrial developments, and the organisation of State Departments (*e.g.* postal, rail, and air communications, health services, etc.) were very dependent upon the application of scientific knowledge.

Science exhibits therefore predominated and were arranged by the various university faculties, technical institutes, and even secondary and primary schools (*e.g.* those from Ruthenia in the far east of Czechoslovakia, where peasant arts and crafts are taught). The intention was to show the relation of man to inanimate and animate nature and how science in all its branches meets the needs of body and mind. The interdependence of the sciences and the manner in which specific knowledge is applied in daily life, in industry, transport, and in assisting general cultural development are emphasised by the continuous way in which exhibits merged and passed from one branch to another.

Man and his generation were illustrated with prehistoric and anthropological collections. The steps taken in maintaining health were shown in exhibits of the medical faculties and hospital schools, showing what was being done to eliminate or minimise suffering and to promote better health generally. The " animate nature " research section was dedicated to Mendel and Purkyně, whose respective discoveries still inspire their fellow-countrymen working in this field.

That much has been accomplished in consolidating the state services and that the present liberal educational policy is sound and bearing fruit is unquestionable. The authorities recognise, however, that much remains to be done and schemes projected or in hand but not yet completed are aptly portrayed in a symbolical manner on easels recalling an artist's unfinished picture.

Wireless Observations during the Eclipse of the Sun, June 29th, 1927 (S. K. Lewer)

The work described in this report (Radio-Research : Special Report No. 7, H.M. Stationery Office ; price 1*s.* 3*d.* net.) was supervised by the Radio Research Board of the Department of Scientific and Industrial Research. The report is divided into three sections :

1. Long-wave observations (12,000–14,000 metres),
2. Observations on medium waves (300–500 metres),
3. Direction-finding observations,

and a note is added on some observations made at Liverpool University not under the direct supervision of the Board.

The experiments were not extended to short-waves, since comparatively precise quantitative results were considered essential.

The intersection of the moon's shadow with the surface concentric with the earth, and at a height of 60 miles, calculated by Dr. J. L. Comrie, was taken into account in arranging the positions of the various transmitting and receiving stations.

1. *Long-wave Observations*

These transmissions were planned for observations on signals

(*a*) crossing the band of totality at a large angle;
(*b*) remaining outside the band of totality altogether; and
(*c*) travelling, as nearly as possible, along the path of totality.

Curves of signal-strength variations are reproduced, showing that the effects of the eclipse were very marked. Observations made on June 28, 29, and 30 indicated that the effect of sunrise had passed off altogether before the eclipse began.

The factors which appear to be involved are:

(i) Attenuation.
(ii) Phase relation between the downcoming and ground-waves.
(iii) Abnormal polarisation of the downcoming waves.

A special frame-aerial system used at the Radial Research Station at Slough was capable of giving these factors separately with considerable accuracy.

The results obtained at Slough showed an increase in reflection coefficient of the ionised layer from 0·13 to 0·22, which is consistent with the results obtained during a normal sunset.

The effects observed at the two other receiving stations, Aberdeen and Exeter, are described, but their interpretation is difficult and no definite explanation can be given.

The results of long-wave observations indicate that the effect of the eclipse was that of a minor sunset in which only the preliminary phenomena of a normal sunset occurred, totality not lasting long enough for the complete effects to occur. Further, the effects were not confined to the totality band.

2. *Observations on Medium Waves*

These observations were made on special transmissions from the stations of the British Broadcasting Corporation. The transmissions were of two types:

(*a*) Transmissions from Newcastle and Birmingham: unmodulated carrier wave, the wave-length of which could be changed continuously through a small range. From photographic records of the received signal, the relative amplitudes of the downcoming and ground-waves, and the equivalent height of the ionised layer could be deduced.

(*b*) Transmissions from London and Manchester: unmodulated carrier wave of constant wave-length.

Graphs are given showing the ratio of the intensities of the downcoming and ground-waves, and also the number of fringes per metre change of wave-length of the signals from Birmingham received at Peterborough. The observations at Liverpool on the Newcastle transmissions were rendered impossible by interference.

The observations at Nottingham and Giggleswick on the London transmissions showed a very large increase in signal-strength about the period of totality, together with large fluctuations. There is a marked difference between the graphs obtained at these two receiving stations, which is explained by the absence of a ground-wave from London at Giggleswick. Graphs of the signal intensity of the Manchester transmissions received at Slough on June 27, 28, 29, and 30 are shown. An increase in signal-strength with marked fluctuations lasting for about ten minutes on either side of totality was observed.

The summarised results show a very definite effect on the properties of the ionised layer responsible for the deflection back to the ground of waves of 300–400 metres. The most striking effect, a large increase in the intensity, is ascribed to an increase in the height of the layer, and to a rapid removal of ionisation in the lower atmosphere. The eclipse effect lasted for a period from 20 to 50 minutes at the different stations, while the passage of the moon's shadow lasted nearly two hours. This means that quite an appreciable fraction of the sun's radiation may be cut off before the effect can be detected by wireless methods. The records of all stations show that on the morning after the eclipse the night conditions seemed to persist for an exceptionally long time after sunrise.

3. *Direction-finding Observations*

Each receiving station made observations on only one transmitting station throughout the week of the eclipse. This work was divided into two sections:

(*a*) *Long-wave Observations* ($\lambda = 18{,}750$ metres).—The results obtained at Teddington on signals from Rugby indicated that any effect due to the eclipse was completely masked, and therefore no curves are reproduced.

(*b*) ***Medium-wave Observations*** ($\lambda = 361$ and 385 metres).—Curves are shown giving a complete record of the experiments. The curves show the error in the apparent bearing plotted with respect to time.

It is concluded that the eclipse produced a very definite effect. At all stations a very large increase in the variations was observed, coinciding roughly with the time of totality. The variations may be ascribed to the observed changes in the effective height and the reflection coefficient of the ionised layer. This is further proof that the normal night conditions of the Heaviside layer were temporarily restored.

Observations on Intensity of Signals from Stavanger (Norway) received at Liverpool University

Both Liverpool and Stavanger were very close to the central line of the total eclipse.

Graphs are shown giving the variations in the signal current on June 29 and 30, and July 1. Photographic recording was used during the eclipse. A definite peak was observed during totality at Liverpool, and another of greater magnitude when totality existed approximately mid-way between Liverpool and Stavanger. These results, similar to the sunrise and sunset effects, show that the effect of sunlight on atmospheric ionisation must occur quite rapidly. The variations observed may be regarded as being due to a movement of the Heaviside layer. The existence of a series of peaks in the signal intensity curve is probably due to the varying combination of the direct wave and the wave reflected from the layer.

Centenary of the Foundation of King's College, London

The need for the creation of a University in London was first publicly mooted by the poet Thomas Campbell in a letter to *The Times* in 1825, and, as a result of the efforts of Campbell and his friends, University College in Gower Street was opened in October 1828. Unlike the older Universities, it was open to members of all religious denominations, and, in consequence, the Governors found it impossible to give any kind of theological instruction to the students. To many people in London such broad-mindedness (or atheism) was most distasteful ; religious instruction was deemed to be far too important for total exclusion from the university curriculum. The decision to found a denominational college was first made at a meeting held at the Freemason's Tavern on June 21, 1828, under the chairmanship of the Duke of Wellington, there being also present, among others, the Lord Mayor, the Speaker of the House of Commons, the two Archbishops and seven Bishops. With such weighty consciences stirred into activity funds were

soon obtained, and, the King having signified his gracious patronage of the institution, a royal charter was sought and granted on August 14, 1829. The College was formally opened on October 8, 1831.

Before the war the full-time day students in attendance at the College numbered about 700. To-day there are more than 1,200 undergraduate students in the six Faculties of Arts, Science, Engineering, Medicine, Laws, and Theology; over 300 postgraduate students are reading for higher degrees or are engaged in research work; and some 500 students who would otherwise be unable to obtain a university education are reading for degrees and diplomas in the Evening Department specially provided for those unable to attend during the day. Another new and important development since 1913 has been the annual scheme of free public lectures, by means of which the latest discoveries in science and learning have been made accessible to large popular audiences.

This increase in numbers and the modern requirements of teaching and research are making new demands which must be met. Among the most pressing needs are the construction of a new Anatomy Building in proximity to the physiological laboratories and the reconstruction and extension of the chemical laboratories in the block of buildings overlooking the Temple Station.

Further, the work of the College is seriously hampered by lack of endowments. Indeed, the proportion of income derived from permanent endowments is smaller than that of any other university institution in the British Isles. Of a total annual expenditure of over £103,000 not more than £4,800 is at present derived from endowments from Chairs, Scholarships, and Research.

A total sum of £350,000 is asked for. Of this £125,000 is required for the new buildings for science and medicine; £15,000 for a new wing of the Hostel in Vincent Square; £125,000 for the endowment of Chairs of English Language and Literature, Physics, Physical Chemistry, Electrical Engineering, and Physiology; £25,000 for Chairs in the Theological Department; and £60,000 for Scholarships and Bursaries.

Donations, large or small, can be sent to the College Bankers, Messrs. Coutts & Co., at 440, Strand, W.C.2, for the credit of King's College Centenary Appeal Account. Copies of a detailed statement of the more urgent needs of the College may be obtained upon application to the Secretary, King's College.

Notes and News

The Council of the Royal Society has, with the approval of H.M. the King, awarded Royal Medals to Prof. A. S. Edding-

ton for his contributions to astrophysics and to Prof. A. Broom for his work on the origin of mammals. The Council has also made the following awards: the Copley Medal to Sir Charles Parsons for his engineering work; the Rumford Medal to Prof. F. Paschen for his contributions to our knowledge of spectra; the Davy Medal to Prof. F. G. Donnan for his work in physical chemistry, especially for his theory of membrane equilibrium; the Darwin Medal to Dr. L. Cockayne for his contributions to ecological botany; the Sylvester Medal to Prof. W. H. Young for his work on the theory of functions of a real variable; the Hughes Medal to M. le Duc de Broglie, for his work on X-ray spectra.

Sir Ernest Rutherford has been re-elected President of the Royal Society for the current session. The Secretaries are Sir James Jeans and Dr. H. H. Dale, with Sir Henry Lyons as Foreign Secretary.

We regret to have noted the announcements of the deaths of the following well-known men of science during the past quarter: Sir Hugh Anderson, F.R.S., physiologist; Prof. R. A. Berry, agricultural chemist; Prof. G. H. Bryan, F.R.S., mathematician; Sir Horace Darwin, F.R.S., founder and chairman of the Cambridge Scientific Instrument Co.; Lord Haldane; Sir Alexander Kennedy, F.R.S., engineer; Dr. Robert Knox, radiologist; Dr. D. Murray, archæologist; Prof. D. N. Paton, F.R.S., physiologist; Dr. B. W. Snow of Wisconsin, physicist; Sir Charles Tomes, dental surgeon; Prof. Wilhelm Wien, physicist.

Sir Ernest Rutherford has been elected Honorary Foreign Member of the Vienna Academy of Sciences.

Sir Alfred Ewing has been re-elected President of the Royal Society of Scotland.

Prof. E. A. Milne has been appointed to the new Rouse Ball Chair of Mathematics at Oxford.

Mr. F. J. M. Stratton, of Caius College, has been chosen to succeed Prof. H. F. Newall as Professor of Astrophysics in the University of Cambridge.

Dr. G. T. Prior, Mr. G. W. Yule, and Prof. E. T. Whittaker have been elected Presidents of the Mineralogical Society, the Cambridge Philosophical Society, and the London Mathematical Society respectively.

Sir David Milne-Watson and Mr. Robert Whyte Reid have been appointed to be members of the Advisory Council to the Committee of the Privy Council for Scientific and Industrial Research in place of members who have retired on completion of their terms of office. Sir J. H. Jeans has been reappointed member of the Advisory Council for a further period of one year.

The centenaries of the deaths of Thomas Young and Sir Humphry Davy both occur in May this year.

It has been arranged to hold the first meeting of the British Association in South Africa on July 22 in Capetown, when the President, Sir Thomas Holland, will take over the duties of his office. The members of the Association will call at Kimberley on July 29 and the Presidential Address will be delivered at Johannesburg on July 31. Further sectional meetings will be held at Pretoria and after August 7 the members will disperse for tours through the Union. L'Association française pour l'Avancement des Sciences has invited those members of the British Association who are unable to travel to South Africa to attend its meeting at Le Havre. Owing to the continued illness of Dr. E. H. Griffiths, the general treasurer of the Association, the Council has decided, with great regret, to accept his resignation and has appointed Sir Josiah Stamp to be his successor.

The Nineteenth Annual Exhibition arranged by the Physical and Optical Societies will be held at the Imperial College of Science and Technology on January 8, 9, and 10.

The total disbursements of the Rockefeller Foundation during the year 1927 just exceeded eleven million dollars, of which two million dollars were given to the University of London on account of the purchase of the Bloomsbury site.

The Regent House of the University of Cambridge has definitely accepted the offer of the International Education Board to contribute £700,000 towards the cost of the new University Library and to the faculties of Biology and Physics. The payments made by the Board will be in proportion to the amounts raised by other means, the full University quota being £229,000.

The Council of the Institute of Marine Engineers (85/88, The Minories, London, E.1) offers the Herbert Akroyd Stuart Award, value about £56, for the best paper on *The Origin and Development of Heavy Oil Engines* delivered to them by April 30, 1929. The competition is open to all, and the marks will be awarded as to 30 per cent. for historical interest, 50 per cent. for technical interest, and 20 per cent. for literary merit.

The International Education Board of New York is providing the funds for a 200-inch reflecting telescope to be erected in California, possibly at Mount Wilson. Dr. J. A. Anderson, of the Mount Wilson Observatory, will be in charge of the work of manufacture and erection, and Dr. Elihu Thomson, of the General Electric Co., will undertake the production of the mirror. Fused quartz is to be tried first, and, in the event of failure with that substance, pyrex glass next. The telescope will be under the control of the Astrophysical Observatory of

the California Institute, which will co-operate with the staff at Mount Wilson.

The new Safety in Mines Research Laboratories in Sheffield, opened officially by the Prime Minister on October 11 last, are described in detail in Safety in Mines Research Board Paper No. 44 (H.M. Stationery Office, price 6*d.* net). The opening of this new research station is the last step in the efforts which have been made for more than a century to apply the systematic methods of science to the solution of problems of the dangers of coal mining. The laboratories are intended for small-scale experimental work, the large-scale experiments being carried out at the Buxton station, opened eighteen months ago. The staffs of the two stations work as one unit, investigators being transferred from one to the other as the nature and progress of their work require. Both are paid for by grants voted by the Miners' Welfare Committee from their fund. The new building has four floors and a basement. The rooms are small, but separated by light movable partitions, so that larger rooms to house several workers can be formed if future requirements make it necessary. Steam is supplied at 50 lb. per sq. inch from a boiler on the second floor and " vacuum " from a pump in the basement capable of exhausting 40 cubic feet per minute to a vacuum of nearly 30 inches of mercury. Alternating current is supplied from the town mains and direct current from a 40-kilowatt converter, the wiring being carried out in cab-tyre sheathed cable run bare on the walls to enable modifications to be made with a minimum of disturbance. There is a small library of books and periodicals.

The paper contains brief accounts of the investigations now in progress in the laboratories and eleven full-page plates illustrating rooms and experimental arrangements.

We recommend to the attention of our readers a lecture entitled *Anthropomorphism and Physics* delivered by Prof. T. P. Nunn to the British Academy in 1926 (London: Humphrey Milford, price 2*s.* net). It contains a philosophical review of the present position of physics and an expression of the somewhat heterodox opinions of its distinguished author, who believes that " music and laughter, the coloured beauty of living creatures and of the sky, sea, mountain, and plain, even in some way the goodly savour of food, wine, and tobacco, are realities—not phantasms dwelling only in the mind "—a belief which he maintains is shared by even " the hardest-shelled man of science " after business hours! Prof. Nunn maintains the doctrine of the objectivity of sensa. " ' It is not in virtue of a substance contained in them that things are; they are, when they are qualified to produce an appearance of there being a substance in them ' " (Lotze). The latter part of the address

contains arguments against the existence of the electron. One—the metaphysical argument—is summed up thus: "For if the only shapes, sizes, and positions we know are those of sensa, how can an electron, into whose constitution, by hypothesis, no sensa enter, have position, size, or shape?" The other "aims at discrediting all scientific objects by showing that in the history of science they have been constantly found out and discarded," *e.g.* caloric, electric fluids in current-bearing wires, and the æther. It is not impossible, even, indeed, probable that Prof. Nunn may be right about the electron; but the arguments employed can carry but little conviction to the experimentalist. In conclusion, there is a brief discussion of the wave-group theory of the electron. The question is asked "Waves of what?" and answered in terms of Whitehead's "ether of events." Whether one agrees with Prof. Nunn or not one must agree that his lecture is interesting, provocative, and worthy to be read.

The *Transactions of the Royal Society of Canada* (Vol. XXII, section iii), for January 1928, contains a number of papers of considerable interest to physicists. Prof. Boyle and Dr. Rawlinson contribute a paper showing details of a solution of the problem of the passage of sound through three contiguous homogeneous media with plane parallel interfaces. The special case of the transmission through a partition in an infinite medium is of importance in ultra-sonic wave research. One conclusion is that all the incident energy will be transmitted by the partition if $\cos i/\cos r = \rho_1 v_1/\rho_2 v_2$, where ρ and v indicate density and velocity respectively (the product ρv being known as the acoustic resistance of the medium). In another paper Lane and Bieler describe experiments on the magnetic susceptibility of sodium and potassium, the results ($0{\cdot}65 \times 10^{-6}$ and $0{\cdot}54 \times 10^{-6}$ respectively for mass susceptibility) being in good agreement with Pauli's calculations. Henderson shows that tourmaline crystals can be used as piezo-electric resonators. Messrs. Shipley and Goodeve give an account of their work on the effect of high voltage arcs under water, finding that the decomposition effected is much in excess of that indicated by Faraday's law—probably as a result of thermal decomposition. Finally, Prof. J. C. McLennan and his collaborators contribute a number of papers of a spectroscopic character.

We have received a number of copies of the Bell Laboratories Record—a monthly magazine printed by the Bell Telephone Laboratories, Inc., N.Y., for the information of the laboratory staff. It is quite the best staff magazine we have yet seen, and the pages devoted to club matters, insurance, and personalia show that those in control of the organisation really

take a keen and practical interest in the welfare of the staff. Two-thirds of each number is devoted to articles, one of which is usually of general scientific interest, while the others refer to matters of special importance in telephony. For example, the October number (Vol. 7, No. 2) contains an account of the work of the American Nobel Laureates in physics, by K. K. Darrow; an article on the acoustics of the auditorium, by E. C. Wente; a description of a few of the relays used in the Bell system, and another describing some of the keys used on manual telephone switchboards. All these articles are illustrated by diagrams and photographs of quite exceptional merit.

The Bell Laboratories have recently prepared a new magnetic alloy, which they call Permivar—containing 45 per cent. nickel, 25 per cent. copper, and 30 per cent. iron. The properties of the alloy depend very much on its heat treatment, and when this is carried out in a prescribed manner the hysteresis loss per cycle per c.c. of metal with a maximum induction of 100 gauss is only $2{\cdot}5 \times 10^{-6}$ ergs as compared with 1 erg for iron and $3{\cdot}3 \times 10^{-2}$ ergs for permalloy. The magnetic properties of permivar make it very suitable for the cores of telephone circuit loading coils.

In an account in *Nature* (Oct. 20, 1928) of the proceedings of the World Fuel Conference held during the period September 23–October 6, attention is drawn to the extraordinary neglect (? exploitation) of the domestic user in this country. His total consumption of coal is about 23 per cent. of the total production —more than that of all the railways, gas, and electrical companies combined. He pays twice as much per ton as the industrial user, and obtains at this extravagant cost an inferior product sold under a fancy name without the exact specification (such as calorific value, ash or stone, content, etc.), which forms the basis of coal purchases in industry. It is indeed time that an official body should be constituted to define and maintain a standard of quality in the different classes of coal supplied for domestic use.

Vol. 1, No. 2 of the *Journal of the Council for Scientific and Industrial Research, Commonwealth of Australia*, contains a brief note on the "flying fox" problem in Queensland and New South Wales. The so-called "flying fox" is really a large bat, *Pteropus poliocephalus* being the commonest species. "Nocturnal in habit and very gregarious, these animals live in large camps of hundreds of thousands of individuals. They migrate according to season and food supply, but usually return to the same camps in successive seasons. In the day-time they cling to the branches of trees in dense numbers; they are restless and alert, and a single gun-shot will put the whole camp in flight. At night they depart in search of food

such as fruit, berries, eucalyptus flowers, and honey. They are particularly fond of cultivated fruit, and the damage they can do in an orchard in one night is appalling. The amount of fruit eaten is relatively small, but the ground is strewn with material which has been merely nibbled or claw-marked." Many obvious methods of destruction have been tried, but in every case the scale of possible operations has proved to be hopelessly inadequate. It has at last been decided to appoint a special investigator for a period of two years at a salary of £600 per annum, whose chief duty it will be to study the biology of the animal with a view to its possible control. Wholesale slaughter is necessary even to keep the pest from increasing; even now it constitutes a most serious menace to fruit-growers in the affected States.

A longer and more important paper in this number of the *Journal* deals with the wastage of Australian fruit during the voyage to England and after the fruit reaches the market. Bitter pit in Cox's Orange Pippin and Cleopatra form the most serious source of loss on the voyage, but in 1927 internal break-down caused large losses during distribution. Thus with three boxes of 2½ in. apples the following results were obtained:

Box 1. Cox's Orange Pippin, Collinsdale:				
Days from discharge	5	10	18	30
Internal breakdown	0·7%	12·8%	25·0%	33·8%
Box 2. Ribston Pippin, Latrobe:				
Days from discharge	3	9	18	
Internal breakdown	4·0%	20·5%	38·5%	
Box 3. Ribston Pippin, Tasmania:				
Days from discharge	6		19	
Internal breakdown	2·5%		16·5%	

Further investigation of the conditions of picking, packing and transit is required to ascertain the cause of such rapid deterioration. Losses on pears were chiefly due to over-ripeness when gathered. The condition of grapes on arrival in England is very uncertain, and further investigation is required. The importance of the work both to the Australian grower and to the European consumer is obvious. The data given above were obtained by the British Food Investigation Board.

Further investigations of the Kimberley Horse Disease confirm the already published conclusion that it occurs when horses are allowed to eat the leaves of *Atalaya hemiglauca* (Whitewood), a tall shrub which is to be found in all parts of North Australia. It seems extremely probable that the ill effects produced by the *Atalaya* are due to its saponin content (from 3·5–4 per cent.). Pathological examinations show changes in the liver similar to those which occur in Winton disease (New Zea-

land) and dunziekte (S. Africa). Winton disease has been shown to be due to ingestion of *Senecio Jacobæa* (ragwort) which the investigators of Kimberley disease have now tested and shown to be fairly rich in saponin. It is evident that a fertile line of inquiry into various obscure stock diseases has been opened up. (Commonwealth of Australia: Council for Scientific and Industrial Research, Bulletin No. 36).

Antiquity (edited by O. G. S. Crawford, F.S.A., and published by John Bellows, Gloucester) is, perhaps, the only journal devoted solely to archæology which may occasionally be seen on a station bookstall. To all who are especially concerned with that subject it is, no doubt, familiar already, but an appeal is made to a wider circle. Its articles comprise both summaries of technical reports which have been published elsewhere and original researches. In No. 6, Vol. II, June 1928, Randall-MacIver contributes the second part of the "Forerunners of the Romans," which deals with the pre-Etruscan Iron Age peoples of Italy. Two of the largest dolmens in Central France, near Tours, are described and figured by Vice-Admiral Boyle Somerville. The capstone of one of them is estimated to weigh 86 tons, and the organising of the army of over 3,000 men required to place it in position must be accounted one of the most amazing feats performed by prehistoric man. An article by Eric Thompson entitled "The 'Children of the Sun' and Central America" is somewhat violently antagonistic to the ideas of the "Manchester School." Illustrating his point by the archæological history of Cranborne Chase and Grovely Forest in Wiltshire, the editor contends that there was a definite break between the civilisations of Roman and Saxon times in England. Other articles deal with the evidence of ancient cultivations at Grassington in Yorkshire and the library of Alexandria. There are notes on numerous recent excavations carried out in many different parts of the world, and 29 reviews. In its matter and style *Antiquity* is fully maintaining the high standard of its earlier parts. Although frankly opinionated in its attitude towards certain theories, this quarterly serves a most useful purpose in keeping all interested in its subject in touch with many varied aspects of the most recent research.

Sir Ronald Ross's Archives, containing many original papers and letters, notebooks and other documents, connected with his work on Malaria and Mosquitoes, have been sold to Lady Houston for £2,000. On the other hand sixteen mahogany chairs have recently been sold at Christie's for 1,450 guineas. Evidently modern Britons think as much of their seats as of their senses.

CORRESPONDENCE

To the Editor of SCIENCE PROGRESS

SUNLIGHT IN INDUSTRY—A CORRECTION

From L. V. DODDS

DEAR SIR,—My Essay entitled "Sunlight in Industry," published in SCIENCE PROGRESS, April 1928, contains the following paragraphs :

"The Medical Officer of the Packard Motor Company has stated that, in his opinion, the use of ultra-violet energy at the Packard works has reduced the amount of amputated fingers at least 80 per cent. and the period of disability 40 per cent. Dr. L. Donnelly, M.D., records how the General Electric Company have made very extensive investigations regarding the effect of ultra-violet energy on the efficiency of employees, and they found that during the winter months their employees made many more mistakes, and their output per hour was less than in the summer months. Ultra-violet burners were installed at one end of a big room, and suitably screened to prevent injury to the eyes by the shorter wave-lengths. At the end of a month's time it was found that the employees under the ultra-violet lights were producing considerably more work than those at the other end, and that the percentage of errors was greatly reduced."

The authority for these statements was an article by L. Donnelly, M.D., in the *American Journal of Physical Therapy*, June 1925, p. 127. It appears, however, that they are quite incorrect. A. W. George, M.D., the Chief Surgeon of the Packard Motor Co., writes :

"I am glad to have this opportunity of flatly denying the statement which Dr. Donnelly has attributed to me. We have never been able to reduce the number of amputated fingers 80 per cent. by the use of ultra-violet energy, safety campaigns, or by any other means. . . . I am using ultra-violet light in the treatment of wounds and their complications many times every day and look upon it as a valuable therapeutic agent ; but I do not attribute to it the miraculous qualities that Dr. Donnelly and a few other enthusiasts claim for it."

Similarly, the General Electric Co. of Schenectady, N.Y., denies any knowledge of the investigations referred to in the second paragraph quoted above.

I regret that the errors in the original article were repeated in SCIENCE PROGRESS and am very much indebted to Mr. E. E. Slosson, the Director of SCIENCE SERVICE, Washington, D.C., for the trouble he has taken in drawing my attention to them and in sending copies of letters of denial from the Packard and General Electric Companies.

Yours faithfully,
L. V. DODDS.

SUTTON-ON-HULL,
YORKSHIRE.
Nov. 13, 1928.

RE CELOTEX

From A. C. KEMP

DEAR SIR,—To an architect—or indeed anyone interested in the various ramifications of the building question—the recent article by Mr. Gordon Temple upon the utilisation of waste is most interesting.

May I, however, be permitted to point out that the statement that less than two years have elapsed since its introduction into this country is scarcely accurate, since Celotex sheeting—in exactly the same form as now used—was being sold and used in many parts of England in 1924, considerably earlier than your correspondent indicates.

Yours faithfully,
ARTHUR C. KEMP, F.F.A.S.

CHALFONT ST. GILES,
BUCKS.
Nov. 15, 1928.

ESSAY-REVIEWS

ANTS AS SOCIAL INSECTS. By HORACE ST. J. K. DONISTHORPE, F.Z.S., F.E.S., etc., being a review of **The Social World of the Ants compared with that of Man,** by AUGUSTE FOREL, translated by C. K. OGDEN. [Vol. I, pp. xlv + 551, plates i–ix, figs. 1–95; Vol. II, pp. xx + 445, plates x–xxiv, figs. 96–128.] (London and New York: G. H. Putman's Sons, 1928. Price 3 guineas net.)

BEFORE dealing with the body of the work it may be as well first of all to say a few words about the translation. The original edition was published in five small volumes (1921–3), but the translation appears in two handsome, but rather ponderous, ones. Volume I consists of the Translator's Foreword, Table of Contents, Explanation of Special Terms, Explanation of Plates and Figures, Preface by the Author, and Three Parts corresponding to the first three volumes of the original edition. Volume II consists of Table of Contents, Explanation of Plates and Figures, Two Parts (Parts IV and V) corresponding to the last two volumes of the original edition, An Appendix, "The War between the Ants and the Termites, a Study of the Origin of Instinct," by Edouard Bugnion (which appeared at the end of the original third volume), and an Index of 38 pages. The work is most admirably translated, very well printed with very clear type, on very good paper, and nicely bound. It is undoubtedly a fact that the translation is far superior to the original work, in usefulness as a book of reference, and in every way. The excellent Index at the end of Volume II increases the value of the work enormously. A thorough explanation of the plates, and figures, is given in each volume, and furthermore the latter are arranged in their proper positions and sequence and not dotted about the book indiscriminately. The credit is also now given to all sources whence the figures were obtained, or copied. Some of the worst mistakes in the original work have been altered in the text itself, and in a footnote to the translator's admirable foreword (p. viii) a certain number of our criticisms and corrections (from our reviews of the original volumes) are referred to. We will take this opportunity most heartily to congratulate the gifted translator on his very fine achievement.

In reviewing this undoubtedly great work, the final and

magnum opus of the veteran Swiss myrmecologist and scientist, Dr. Auguste Forel, we are personally handicapped by a too hypercritical sense, the result of an intimate knowledge of the habits of, and works on, what are admittedly the most fascinating of all insects. This has been brought about by the close study of ants and their guests during the last forty years, not only in nature and the laboratory, but also in all the works on the subject, both ancient and modern. It were better to be a general scientist, entomologist, or student of Natural History, instead of a "myrmecophilous" specialist, when the great interest of the whole subject, the grandeur of the theme, and the fine achievement in treating of every detail in every branch in the study of ants would not be depreciated by a too ready consciousness of every small error and inaccuracy, or the realisation of the fact that certain parts of the work are not up to date.

The great Charles Darwin wrote, in 1863: "Accuracy is the soul of Natural History. . . . Absolute accuracy is the hardest merit to attain, and the highest merit. Any deviation is ruin." If, therefore, we feel it a duty to point out any such inaccuracies which appear to us to exist here and there throughout the book, it is not with any intention of belittling the really fine character of the work, nor to detract from the praise due to the distinguished author, for whom we possess nothing but admiration and respect, but for the benefit of any serious student of myrmecology who might otherwise be misinformed on certain points. In the preface we learn that Forel, who was born on September 1, 1848, passed his early childhood until he was eight and a half years old in the small village of Lonay-près-de-Morges, on the shores of Lake Geneva. There he made his first acquaintance with, and indeed began his studies on, ants, and actually witnessed, what he afterwards came to realise was a slave raid, made by some red ants on his black ant friends living on the terrace of his father's garden. When he was eleven years of age his grandmother presented him with a copy of Pierre Huber's *Recherches sur les mœurs des fourmis indigènes* (1810) [a work which, together with Lubbock's *Ants, Bees, and Wasps* (1882), and Forel's *Fourmis de la Suisse* (1874), formed our own original myrmecological library], which he describes as a revelation to him. He then and there vowed to himself he would be an ant-historian like Huber all the days of his life, and he has ever since kept his word.

His writings have been very prolific, and he has helped to advance the study of ants in all its branches.

Forel gives a list of his myrmecological friends and colleagues to whom he says science, like himself, also owes much. It is with regret that we notice the name of only one Englishman mentioned—he being more of a big game hunter than a myrme-

cologist. That he should have omitted the name of the late Lord Avebury (Sir John Lubbock), who did so much to encourage the study of ants, and made so many original discoveries and experiments, is alone deplorable. We ourselves are amply compensated by the generous tribute in the translator's foreword.

We will now endeavour briefly to follow the author through the work, touching as far as possible on the different subjects with which he deals.

We first read about the phylogeny of ants, which teaches one to understand the ancestral origin of these insects ; ontogeny, their individual development and the reason for their polymorphism, external anatomy, their structure as seen by the naked eye or through the magnifying-glass, internal anatomy, the structure of the vital organs of their wonderful bodies ; and classification which makes one realise the manner in which to distinguish their genera, species, races, and varieties from one another, taking into consideration as far as possible their phylogenetic descent from the earliest times.

Forel considers that the ants must be derived from non-social insects very closely akin to the Mutillids, whose females probably had no wings. It is true that the male in one of the most primitive ants belonging to the subfamily *Ponerinæ* might be mistaken for a Mutillid even to-day. Wheeler, however, considers that ants are most closely related to the *Scoliidæ*, since they must be traced to ancestors which were winged in both sexes. Escherich also has pointed out that it is unlikely that a wingless form which had once acquired wings would again lose them, as in the case of wingless females.

It is evident that Forel is of the opinion that the sex of an ant is already determined in the egg—as is the case with Termites. He writes : "The differentiation must begin at a very early stage in the larva, if not actually in the egg, which appears to me much more probable" ; but previously he definitely states : "Polymorphism in ants takes place in the egg !" We, however, from our experiments with a living colony of *Myrmecina graminicola*, which has been in our possession for over eighteen years, consider that it is caused by the feeding of the ant larvæ by their nurses. Wheeler also, in one of his latest papers (1928), writes : "The fact that the mithergates of such ants as Pheidole often exhibit an anomalous mixture of traits characterising all three of the female castes affords valuable confirmation of the theory that the fertilised eggs all have essentially the same genetic constitution and that the resulting larvæ develop into workers, soldiers, and fertile females or queens, because they are subjected to three different trophic conditions by their worker nurses."

One of the reproductive organs in the female ant is known

as the receptaculum seminis. This is a small pocket which is filled with the male element during copulation. The eggs are fertilised during their passage through the uterus by sperm ejected from the receptaculum. Forel alone calls this "the female seminal vesicle," and although his figure No. 19 is said to be after Adam and Escherich, both these authorities call it the receptaculum seminis. Escherich and Wheeler both use Janet's figure of a female *Myrmica* to illustrate the ♀ reproductive organs and, as does Janet, call this pocket the receptaculum seminis.

Forel states that at present we divide the ants into five subfamilies; but in 1920 Wheeler created two more—the *Pseudomyrminæ* and the *Cerapachyinæ*—bringing the number up to seven. It was Wheeler also in 1920, not Emery in 1922, who replaced the name *Camponotinæ* by that of *Formicinæ*.

For the geographical distribution of ants Forel gives nine major faunas. Of these the ant fauna of the Palæarctic region proper (*i.e.* not including the Mediterranean region) is, relative to its large size, the poorest in the world. The largest ant fauna is the Neotropical, and next in numbers comes the Indo-Malayan. A close relationship exists between the ants of the Nearctic and Palæarctic regions, an equally great difference being found between those of the Nearctic and the Neotropical, the Neotropical and the Æthiopian, etc. It is curious that some species in the extreme northern and southern faunas are very much alike; this is not on account of relationship, but has been brought about by convergence, through the effects of a similar climate, etc. Many species of ants have or are becoming cosmopolitan through the influence of navigation and commerce—one of the examples given by Forel is the notorious "Argentine Ant" *Iridomyrmex humilis*, which in recent times has reached Europe. Forel records it from Portugal and the South of France. We discovered it in Sicily in 1926, it has been found in Guernsey, and we note it from Belfast, Edinburgh, Eastbourne, Enfield, Plymouth, Broadbottom (Cheshire), Manchester, Tring, and Windsor.

Numerous fossil ants have been discovered, especially in the Baltic and Sicilian ambers, but no species is known to have existed before the Tertiary Epoch. Still the remains of ants are so numerous in the early Tertiary, where the males, females, and workers are as sharply differentiated as at the present day, that it seems probable they must have existed as far back as the Trias, though they have not left any remains. We have recently described and enumerated all the known British fossil ants.

As Forel points out, physiology is the study of the functions of the body. Psychology is the study of the mind and the

physiology of the brain. The sensations of ants give them information about the external world. They combine these data by means of their movements, and their brain.

Ants possess the senses of smell and touch (partly combined), taste, sight, and probably hearing in some form (Forel does not deny a certain kind of hearing in ants, but does not consider their audition is the same as ours). They are thus able to find their way, communicate with each other, and know each other.

Brun, Cornetz, Miss Fielde, Forel, Lubbock, Wasmann, and others, have all contributed to our knowledge of the Psychology of ants.

Symbiosis is the more or less constant and intimate union of one species of living beings with another species. Forel distinguishes three kinds of symbiosis in ants: (1) between two different species of ants; (2) between a species of ants, and other animals; (3) between ants and plants. Partial symbioses between ants and plants are very numerous—they inhabit the gall-like roots of *Myrmecodiæ*, etc.; cut leaves of plants for their fungus gardens; inhabit the stems of *Triparis* and *Cecropia* and spines of acacias, etc.; several species amicably inhabit the same plant; collect certain seeds (myrmecochorous seeds); plant ant rice (*Aristida*), and other grain; are digested in pitchers of *Nepenthes*, etc.; and certain moss (*Polytrichium*) grows over and obliterates their nests, etc.

Those animals which live with ants are known as myrmecophiles, and Forel recapitulates Wasmann's four divisions—the Symphiles (True Guests), Synœketes (Indifferently Tolerated Lodgers), Synechthvans (Hostile Persecuted Lodgers), and Parasites (Ecto- and Ento-). Some of the different means utilised by these creatures to achieve their ends are mimicry—imitation of form, colour, and movement of one living creature by another (Forel gives as an illustration green insects which live among green leaves, etc. This is more usually called "protective resemblance")—trichomes—attractive patches of hair; porous canals; offensive and aggressive measures, etc. Various examples are given of guests in the four different divisions.

The food of the larvæ of *Microdon* (certain Diptera) is said to be unknown; it is perhaps as well to mention that in 1911 we demonstrated by experiment that these larvæ feed on the pellets ejected by the ants from their infrabuccal pockets.

Ectoparasites are creatures which fasten themselves on to the bodies of their hosts. A good example given by Forel is the little fly (*Metopina pachycondylæ*) whose larva clings to the neck of an ant larva (*Pachycondyla harpax*) in Texas by means of a sucker, and encircles its host like a collar. To speak of the

larva of *Clythra*, which encloses itself in a case made of earth mixed with its own excreta, and walks about independently inside the ants' nest, as an ectoparasite is clearly inaccurate. Moreover, we worked out the life-history of this beetle in 1902, and have shown that its larva feeds partly on vegetable refuse in the nest, and also on the droppings and pellets of the ants —not on the ants' eggs, as stated by Forel.

The descriptions of a certain number of Hermaphrodites are given—these are more usually called Gynandromorphs, Ergatandromorphs, and Dinergatandromorphs, according to whether the individual has male and female, male and worker, or male and soldier characters combined, as the case may be. Up to the present time some 49 such individuals have been recorded in ants.

As to what is the origin of hermaphrodites Forel suggests it is probable that primordial anomalies of the egg after its fertilisation, or even before, may play a large part in the matter. In the case of von Sommern's experiments, reported by Poulton, several gynandromorphs of the butterfly *Papilio dardanus* were produced by simple mechanical shock *at the time during which the larval skin is being cast and the pupa still soft and unset.*

True monstrosities also occur in ants, which may possess an extra number of legs, or deformed joints, etc.; but these belong to quite different phenomena from those of gynandromorphism.

Ants build their nests everywhere—they are nearly all irregular, variable, and adaptable to circumstances, whether in the earth, in wood, or trunks of trees, in pre-existing cavities; being woven, or constructed of carton, etc. Forel describes nearly all the different kinds of nests, and figures many of them.

The greater part of the rest of the work deals with the habits of ants both within doors and without, including their specialised habits. This is, of course, the most interesting part of the study of ants, as indeed of all insects. Space will not allow us to follow the work any further in detail, but we can recommend its careful perusal both by the student, as well as the general reader. We must, however, call attention to the following points:—

When describing the different observation nests used by various myrmecologists to study live colonies of ants in the laboratory, Forel suggests that Gould (1747) only studied the habits of ants in a state of nature, and did not use any artificial apparatus to observe them in. Gould himself, however (p. 25), speaks of placing in a box with moist earth "three or four cells of ants" with their young and also a queen, etc. Pierre Huber was not the first, as stated by the author, to use observa-

tion nests to study ants in; since Swammerdam described, in 1737, the apparatus used by himself for this purpose. It was Lubbock (in 1876) who first actually demonstrated that females after the marriage flight are capable of bringing up their brood to maturity unassisted.

It is not the *Ponerinæ* alone who feed their larvæ with bits of cut-up prey. In the subfamily *Myrmicinæ*, we have frequently seen, for example, the ants in our nests of *Myrmecina graminicola* and *Stenamma westwoodi* place bits of cut-up insects, etc., on their larvæ, which are eagerly devoured by the latter.

Wheeler has shown that ant larvæ also supply food to the ants their nurses, as is the case with wasps, and this he calls trophallaxis; therefore it cannot be said that " the ant alone is active."

Lubbock was the first to prove that the eggs of aphides were carried by ants into their nests for the winter, and when hatched in the spring, taken out and placed on their proper food-plants.

In the relations of ants to caterpillars of the Lycænid butterflies, no mention is made of Chapman and Frohawk. To ignore the work of these two authors is to leave out the most important observations and discoveries made on the subject.

Silvestri did not find that the larva of a lady-bird sucked the secretion of *Tettigometra* larvæ living with *Tapinoma*, but that it devoured them.

With reference to mutual parasitism of ants, we should consider *Formicoxenus nitidulus* to be a commensal, or myrmecophilous species rather than a parasite.

It is true in the main that with every ant the sentiments of fear and hostility are wakened by the odour of an ant which she has not met individually. As an exception we can quote a number of workers of *A.* (*C*) *umbratus* from Weybridge, which we had under observation, and having introduced to them individuals of the same species from Woking, Richmond Park, and Deal, the latter were at once accepted without the slightest hesitation.

Our experience with *Myrmecina graminicola* proves this ant to be by no means " very peaceable." Not only have we never been able to get two colonies to amalgamate; but in the colony which has been in our possession for over eighteen years very severe fighting has more than once taken place.

Forel expresses a doubt about Green's observations on *Dorylus orientalis* eating various tubers, such as potatoes, etc., but Mr. E. Green tells us he has observed it himself over and over again in Ceylon.

In connection with the interesting habits of those ants

which use their larvæ to spin silk to fasten together the leaves of which their nests are constructed, it may be pointed out that the first mention of the actual nests in the literature is to be found in *Captain Cook's Voyages*. In 1770 he observed the nests of *Œcophylla virescens* in the branches of the mangroves in North Queensland. The discovery of how these nests were constructed was first made (as stated by Forel) by Ridley in 1890. He was followed by Saville Kent in 1891, and Green in 1896. All three of these observers made the discovery independently, without knowing about the work done by the others.

It is stated that no genus of *Ponerinæ* is able to rear aphides or coccids, or any other cattle. We, however, have found the coccid *Ripersia donisthorpei*, the colembolid *Cyphodeirus albinos*, and several beetles, including the regular guest of this ant, *Bythinus glabratus*, with *Ponera coarctata* in England.

Wheeler not only separated the *Pseudomyrminæ* as a subfamily on the basis of his studies of the gizzard and the larvæ of ants, and also of their nutrition, but also because the shape of the head, especially the clypeus and frontal carinæ, is unique; the eyes are very large, the construction of the petiole, postpetiole and spurs is peculiar; and that the antennæ are 12-jointed in the ♂ as well as in the ♀ and ☿.

We regret that throughout the work, and especially in the Epilogue, the author has taken the opportunity to air his socialistic views. In our opinion an entomological work is not the appropriate means for the introduction of political theories of any kind, still less for their glaring advertisement.

AN ENGLISH DISCIPLE OF MENDEL. By β_6. Being a review of **William Bateson, F.R.S., Naturalist; his Essays and Addresses, together with a Short Account of his Life,** by BEATRICE BATESON. [Pp. ix + 473.] (Cambridge: at the University Press, 1928. Price 21*s*. net.)

WILLIAM BATESON was born in August 1861, less than two years after the publication of the *Origin of Species*. Twenty-two years later, after completing the Natural Sciences Tripos at Cambridge, he was himself setting to work on the investigation of biological problems. His first attempt was along the then conventional lines of descriptive embryology: in studying the life-history of *Balanoglossus* he sought to throw light on the ancestry and evolution of the Vertebrata. This very attempt, however, left him with the feeling that it was not by the methods of comparative anatomy and embryology that the problems of evolution were to be solved. It seemed to him that in working so exclusively along these lines the zoologists of the last two decades, although professing Evolution as their faith and the author of the *Origin of Species* as their prophet,

had in reality departed widely from the methods and interests of Darwin himself. Forgetting that animals are living organisms, they were content with studying and describing the "types" of the lecture theatre and laboratory, and indulging in far too academic discussions as to the nature and course of evolution. Bateson felt that the problems which arose out of Darwin's work could only be solved by Darwin's methods. Evolution being dependent on the occurrence of variations, it could best be elucidated by a study of variation and its mode of occurrence in living organisms, and henceforward we find him making such studies the main interest of his life. For the next seven years he busied himself principally with the collection of facts: "he ransacked museums, libraries, and private collections; he attended every sort of 'show,' mixing freely with gardeners, shepherds, and drovers, learning all they had to teach him." The results of these labours were published in 1894 in his book, *Materials for the Study of Variation*. Not content, however, with simply amassing the observations of other people, he was already during this period commencing, on his own, breeding experiments on plants and animals in order that he might see for himself the way in which variations arise and the effects produced by intercrossing. This breeding work gradually absorbed more and more of his time and energies.

From 1890 onwards we find him urging on senior zoologists and university authorities the importance of his methods to the modern student of biology. He appears to have received but scant sympathy, however, at any rate where university appointments were in question. It may have been that the general opinion with regard to his work was that expressed to him by Sedgwick: that he had gone too far afield and that his was a "fancy" subject. Furthermore, though not without devoted students, he seems to have alienated seniors and contemporaries by a somewhat too aggressive assurance as to the merits of his own methods and the futility of all others. With the help of a college stewardship, however, grants from the "Evolution Committee" of the Royal Society and from the British Association, and other adventitious means, he managed to keep up and greatly extend his breeding experiments, and to carry on with them until, in 1908, a suitable appointment at last fell to him: the new Professorship of Biology at Cambridge, created for the advancement of Genetics, a science at last accepted as worthy of university recognition. This chair he held for two years only; in 1910 he was offered the Directorship of the John Innes Horticultural Institute at Merton, and he accepted because, financial reasons apart, it afforded him the much-longed-for facilities for unlimited experimental plant-

breeding. Pursuing such experiments with untiring energy he remained at Merton until his death in 1926.

Bateson thus had the rare good fortune to be able to devote practically his whole scientific life to the investigation of those problems which especially attracted him. Shortage of funds for the complete realisation of his experimental ambitions may at times have checked him, but he seems to have been usually little hampered by the routine teaching or the administrative drudgery which are apt to claim the lion's share of the time and energy of so many scientific workers

Beatrice Bateson, in a charmingly written Memoir, which she intersperses with numerous extracts from her husband's writings and letters, fills in these bare outlines of his biological career with many details of scientific and personal interest. She paints us an inspiring picture of the Hero as Man of Science : passionately absorbed in his investigations, cautious and critical yet productive, working early and late with infinite patience and unflagging enthusiasm, inspirer of an active band of students, still finding time for university interests such as controversies on the woman's question or compulsory Greek, and gifted, moreover, with a wide general culture and a keen and informed appreciation of art. Finally, we may venture perhaps to add, fortunate in a wife no less energetic than himself, who, while keeping domestic affairs running smoothly, could share his intellectual interests and actively assist him in his work. To her the younger generation of biologists, commencing on their own investigations, can only be grateful for this vivid picture of what Bateson strove to be ; they are likely to find it of greater value than any more critical account of the man himself.

The series of essays and addresses which follow the Memoir are scattered over the years 1899 to 1925. They are addressed not to the genetic specialist but to a wider audience, assumed, for the most part, to be equipped with sufficient scientific education to enable them to appreciate the more general conclusions to be derived from the specialist's work. Collected into a book, many of them will be found to be largely repetitions of those which have gone before, since each new audience, whether of horticulturists, agriculturists, neurologists, eugenists, educationists, economists, politicians, had of necessity to be instructed in the elementary principles of genetics before they could be shown how these might throw new light on their own particular problems. Again and again are repeated the same criticisms of the older methods of biological investigation and argument, and again and again we find the same insistence on the importance of the results of the experimental cross-breeder. The posterity of each individual must be traced separately. The gross statistical method confuses totally

different phenomena under the term "variation." There is no sanction for the view that evolution occurs through the gradual transformation of masses of individuals varying almost imperceptibly from one another.

Bateson held the view that a hard-and-fast line could be drawn between definite variations or "nascent varieties" and indefinite variations, or "fluctuations." The latter he believed to be merely "ephemeral," not capable of transmission, the latter to be transmissible and to follow the Mendelian laws of inheritance. He ignores the fact that the biometrician is quite conscious of the very wide range of variation in many living organisms and quite ready to recognise that the rare extremes of his statistical array—with which he identifies Bateson's definite variations—may play an important rôle in evolution.

For biological students perhaps the main interest of these essays will lie in the historical account which they yield, when chronologically synthesised, of the development that Mendel's law of heredity underwent at the hands of Bateson and his school. The first of the addresses—to the Royal Horticultural Society—was delivered in 1899 before Mendel's work was rediscovered and made known to the scientific world by De Vries and Correns. Less than a year later, while in the train on his way to give the second address to the same society, Bateson read Mendel's paper of 1865, seized upon it at once as the key to the true understanding of the laws of variation and heredity, and incorporated an account of it in his lecture. Along these lines, he felt, must the genetical work of the future be developed; here at last lay the path along which attempts to solve the problem of the origin of species must be directed. Henceforth we find that "Mendelian Inheritance" is the keynote of all the essays. At first Mendel's law is accepted in the simple form deduced by Mendel himself from his experiments on crossing different races of peas; but as breeding experiments multiplied, and embraced an ever wider field, the simple law was not found to suffice, and one elaboration after another, undreamt of by Mendel himself, had to be added to explain the complex results. Dominance is first claimed as universal, then thrown overboard. The conception of "determiners" is introduced, then the conception of "coupling." "Quantitative" characters are differentiated from "qualitative," and it is admitted that the laws governing the descent of the former are still not understood. Even qualitative characters, though usually behaving as integral factors, may on occasion undergo quantitative disintegration with the production of a graduated series of varieties intermediate between the two parent varieties that were crossed together. Such apparently non-Mendelian

results as are produced by the cross between negro and white man may be accounted for *either* by imperfect segregation *or* by a multiplicity of cumulative factors.

Through more than twenty years we thus trace the fate of Mendel's simple conception of heredity as, snowball-like, it gathers complexity in Bateson's mind ; never allowed to prove unequal to the occasion, a new clause was promptly added to it whenever the need arose. It must be left to the future to decide how far this elaborate superstructure rests upon rock or upon sand, but, as we find Bateson himself proclaiming in an address on " Progress in Biology " in 1924, Mendelian research has assuredly failed to reach the goal which it first aimed at : the origin of species. As regards the problem of evolution it " has finally closed off a wrong road."

In later life Bateson appears to have grown more willing to take an interest in the work that contemporary biologists were doing in other lines than his own. Thus, in his address to the American Association at Toronto in 1922, he confesses that he has been converted by the demonstrations of T. H. Morgan and his colleagues to a belief in the Chromosome Theory. In 1924 he is keenly interested in what D. M. S. Watson has to tell him of the evolutionary data slowly but surely accumulating at the hands of the palæontologist, and of the attempts that have already been made to interpret it. Perhaps, had he come into touch with the palæontological as with the cytological material that New York has to offer, before his Toronto address, he might have modified the statements that caused such rejoicings in American anti-evolutionist circles. Had he spent a day among the rich stores of the American Museum of Natural History, critically examining the superb series of fossil horses, and thus had for the first time a true record of evolution actually before his eyes, he could hardly have been less impressed with these data than with the microscopical preparations of the cytologist. How would they have affected his views as to the discontinuity of evolution, the unimportance of minute variations, the futility of the statistical method of dealing with variation as a mass phenomenon, the lack of evidence as to adaptation in living organisms, and the minor part played by natural selection ?

Readers, both scientific and lay, are likely to find much to interest them in this book, even though they hold critically aloof from its dogmata. Those for whom the more strictly genetical side has less appeal may yet find much to stimulate thought in the essays dealing with social and racial problems as viewed by the biologist, and in those, such as " The Place of Science in Education," in the educational series at the end of the book.

MARINE ZOOLOGY ON THE PACIFIC COAST OF NORTH AMERICA. By CHAS. H. O'DONOGHUE, D.Sc. Being a review of **Seashore Animals of the Pacific Coast.** By MYRTLE ELIZABETH JOHNSON, Ph.D., and HARRY JAMES SNOOK, M.S. [Pp. xiv + 659, with 9 coloured plates and 700 figures.] (New York: The Macmillan Co., 1927. Price 32*s*. net.)

THE investigation of the rich and varied organisms on the shores of the Pacific Coast of North America began with the voyage of the Russian Captain von Kotzebue in 1824. The collections then made were reported upon by F. Eschscholtz in his *Zoologischer Atlas*, 1829–33. This was all in advance of what might be expected from the white inhabitants of that region at that time, and consequently there was a pause until the middle of nineteenth century, when the California Academy of Sciences started to publish their Proceedings, and keen naturalists like Cooper, Trask, and Stearns began to study the local littoral fauna of California, but more particularly in and around San Francisco. Almost at the same time W. K. Lord was appointed naturalist to the boundary commission, the first official British naturalist in these regions. In 1858 gold was discovered in the Fraser River Valley, and consequent upon this arose the need for accurately mapping the forty-ninth parallel. Lord was attached to the commission set up to do this. He was a man of acute powers of observation and catholic interests, and the list of animals he collected was published as an appendix to his most readable book, *The Naturalist in Vancouver Island*, in 1866. The work he so ably began was carried on, though not without a break, by a small group of enthusiasts in Victoria and Vancouver Island, among whom the names of the Rev. G. W. Taylor and Dr. Newcombe call for mention.

Meanwhile, however, the work was being attacked along other lines and a series of biological stations were opened along the coast. The first of these was a small building put up in Chinatown, Pacific Grove, California, by some of the staff of Stanford University in 1892. Soon after a tract of land in Pacific Grove and a building were provided by T. Hopkins, and thus came into being the Hopkins Marine Laboratory, the director of which was, and is, Prof. W. K. Fisher. At the same time the University of California made a start in a tent at Pacific Grove, but this did not lead directly to anything permanent, and after various moves and experiments the Scripps Institution was erected in La Jolla in 1905, and two years later Prof. W. Ritter was appointed full-time director. In 1913 Pomona College built a station at Laguna Beach, California, with Prof. W. A. Hilton in charge. Before the last of these Californian laboratories was built, however,

a start was made 900 miles to the north in the Vancouver Island region. The first was a station at Departure Bay, near Nanaimo, on Vancouver Island, erected by the Biological Board of Canada, and of this the first curator was the late Rev. G. W. Taylor. Two years later the University of Washington established a laboratory at Friday Harbour, San Juan Islands, the director being Prof. C. T. Frye. The latest recruit is a station founded in 1926 at Prince Rupert by the Biological Board of Canada, with Mr. D. B. Finn as director.

In more or less close connection with these stations, which on account of their connections with universities or government may be regarded as professional institutions, the greater part of the biology on the Pacific Coast has been done. The remainder was the work of what may perhaps be termed the San Francisco and Vancouver Island groups of amateurs, and some of these were in large measure collectors whose material was actually worked up by professional specialists. This stands in marked contrast to the condition in Great Britain during the first half of the nineteenth century, when by far the greater part of the work was done by the amateur, and if any consultation were necessary, it was usually the professional biologist who had to call in the aid of the naturalist.

Marine biology on the Pacific Coast has now reached a new stage, for there are a number of amateur naturalists, of teachers in schools, students in high schools and universities and professional workers, who in these days are often little more than amateurs outside their own particular field. Over and above these there is a large and ever-growing number of persons who would scarcely regard themselves as naturalists, but who, nevertheless, are keenly interested in the multitudinous variety of animal forms to be found on the shores. The book under consideration meets the requirements of all these in a most satisfactory manner. It fills a need that has long been felt by workers on the Pacific Coast.

It is possible to point to a few slips that will probably be corrected in a new edition which we feel sure will be called for before long, *e.g.* " ajority " for " majority " on p. 130 ; " Æolididæ " p. 490, and " Æolidiadæ," p. 502, for " Æolidiidæ " ; " Styel " for " Styela," p. 6 ; " 1997 " for " 1897," p. 330, etc. And, on the whole, it would be preferable to give the measurements all in the metric scale or in feet and inches, and not to use both, as on p. 443, *inter alia*. These matters are easily remedied, but there are three other criticisms that seem pertinent. While recognising the need for brevity and other reasons that led the authors to omit the vertebrates, we cannot help thinking that a short account of the very interesting intertidal and shore fishes would have added materially to the value of

the book. As an appendix there is a list of original publications arranged under phyla. This in itself is good, but it is a pity that the latest reference given is one paper in 1923, whereas the book was not published until August 1927, and in the intervening period some useful work was done; to mention only one, we have Oldroyd's monograph on the shells of the coast. Moreover, there is plenty of internal evidence to show that more recent publications were actually consulted. Lastly, we cannot help feeling that Canadian work has not received the attention it merits. Nanaimo is not mentioned in the collecting grounds, whereas it and the Vancouver Island region are the classical and type localities for a number of the coast species. Fortunately the organisms themselves pay no attention to the imaginary line forming the international boundary, and fortunately also we can say without fear of contradiction that that line plays no part in the exchange of hospitality, courtesy and good fellowship which is so marked among the biologists of all classes on the coast.

Apart from this, we have nothing but praise for the book; its arrangement, its execution, its illustrations, and general get-up are excellent. In the preface it states "such a book as this is, of necessity, a compilation from many sources." This may be strictly accurate in some senses, but the authors are far from doing themselves justice. It would be in a sense as true to say that a Beethoven sonata was just a compilation of musical notes or that a painting was a compilation of colours. By their judicious selection of facts, the successful arrangement of them, and their pleasing presentation the authors have limned a picture of a wonderful fauna that is more than a mere compilation. Moreover, the book is full of observations and has numerous original illustrations that bespeak long experience in the field and close observation of the animals themselves. In particular, the illustrations are excellently done, and in such a work this is a very important point.

The book will be invaluable to biologists in the widest sense on the Pacific Coast of North America, and, moreover, to those elsewhere it will provide a source of reference not otherwise available. It may well mark the beginning of a new wave of enthusiasm among those to whom the inaccessibility of many of the highly technical original papers have proved too formidable a barrier to be overcome. Personally, we can say that it brought back the recollection of many happy hours spent at various points from Prince Rupert to Los Angeles in a most vivid and pleasant way. We should like to congratulate the authors and all concerned and to wish the book the high degree of success that its excellence fully warrants.

CONTAGION AND CALCULUS. By HILDA P HUDSON, Sc.D., being a review of **A Contribution to the Mathematical Theory of Epidemics**, by W. O. KERMACK and A. G. MCKENDRICK. Proceedings of the Royal Society, A, Vol. 115, pp. 700-721, May 1927.

IN a mathematical treatment of epidemics, we can always write down a list of variables to represent as many of the features as we fancy, and arbitrary functions to represent their relations; the differential equations then follow. In order to be able to carry the analysis far enough to test it by actual records, we have to take few variables and simple functions. Any particular choice of assumptions is open to objection, for it cannot possibly represent the whole truth; it will be guided by the kind of application in view, and will be other for a short and highly fatal disease than for a protracted one from which people usually recover.

It is in such respects that this investigation differs from the earlier one, cited by the authors, due to Sir Ronald Ross [1]; it is satisfactory to find that similar results follow from the two sets of assumptions. Both account for the usual course of an epidemic by the decrease in the number of susceptible people, without having to assume any rise and fall of the infectivity; both lead to functions having the familiar bell-shaped graphs, that of the case-incidence rising more steeply than it falls.

The first main result of the paper under review is as follows. At any time after the outbreak of an epidemic, out of an original population N per unit area, let there be x persons still liable to infection, and y suffering from the disease. The epidemic is stationary if, on any day, the number of fresh infections is equal to the number of cases removed by recovery or death. The first of these two numbers is presumably dependent on, and the second independent of, the density x of the susceptible population; both depend on the density y of sources of infection, and on certain characteristics of the particular disease: its rates of infection, recovery, and mortality. If these rates are constant, and the two numbers simply proportional to xy and y respectively, there is a certain fixed value of x, here called the *threshold* density, for which y is stationary. If x exceeds this value, y increases and x is thereby decreased till it falls to the threshold value, after which the epidemic declines again.

The paper contains an interesting extension to a disease transmitted through an intermediate host, to which nearly the same analysis applies; and some way is made with the far more difficult case of variable rates of infectivity, etc.,

[1] *Proceedings of the R. Society*, Part I, A, Vol. 92, 1916. Parts II and III with Hilda P. Hudson, *ibid.*, A, Vol. 93, 1917.

34

provided that the epidemic is small, so that certain approximations can be used.

Of the assumptions here made in order to keep the equations simple, the one most open to query is the neglect of the birth-rate. Births tend to aggravate an epidemic by continually adding to the number of susceptible persons. In the example on p. 714, of an outbreak of plague in Bombay, the number of births is certainly of the same order as other numbers taken into consideration. This numerical example is of doubtful value. A comparison with statistics would be very useful if it told us something about the numerical values of the characteristics of the disease; but this is expressly disclaimed, and would in fact require an epidemic on a very large scale, with the beginning and end of each case accurately noted; few, if any, such records exist. This is the great difficulty in testing any mathematical theory of epidemics.

We add a few captious criticisms of details. The authors themselves remark on p. 717 that the number y_0 of initial cases, being an integer, can never be small in the mathematical sense, though y_0/N can. It is exasperating that they did not therefore correct some inaccuracies on the previous pages; the meaning is, however, clear. A minor exasperation is the use of $\sqrt{-q}$ for a real constant.

More serious is the omission of any discussion of the relative order of the quantities assumed to be small. Two of these are:

$\xi \equiv \frac{\kappa}{l} x_0 - 1$, depending on the excess of x_0 the original susceptible population above the threshold value, κ and l being constant rates of infection and removal, and

$\equiv y_0/N$, depending on the original number of cases.

One of the constants of integration is ϕ where $\coth^2 \phi = 1 + 2a\eta/\xi^2$, where $a \equiv N\kappa/l$, a constant nearly equal to unity. The value of ϕ depends on the order of η/ξ^2, and if this ratio is finite, a slight variation of x_0 can very greatly affect the value of ϕ and of the integrated function. This is the point where the importance of the threshold density emerges from the differential equation, and it is a pity that it is obscured by the way the analysis is set out.

On p. 715 we have the amazing statement that the epidemic can take a certain course if y_0 is neglected, which, if it meant anything definite at all, would mean that there never were any cases, and therefore no epidemic. This is just the case when $\eta/\xi^2 \equiv 0$, however small ξ may be, and ϕ is infinite.

The main argument, however, can be based on the differential equation (29), which is valid for all values of the constants,

just as well as on the integrated form (30), which may become illusory.

The authors have made a definite addition to our knowledge of a subject on which too little work has been done ; it is much to be hoped that they and others will carry these researches further.

NORMALS OF WORLD WEATHER. By E. V. Newnham, B.Sc. Being a review of Volume II of **The Manual of Meteorology**, by Sir Napier Shaw. [Pp. xxxix + 445, with 225 figures.] (Cambridge: The University Press, 1928. Price 36*s*. net.)

Meteorology owes a considerable debt to Sir Napier Shaw for the energetic way in which he furthered the study of British weather during the years of his directorship of the Meteorological Office. Since his retirement he has devoted his energy to completing the task of writing a meteorological textbook on a more comprehensive scale than has been attempted hitherto. Urgent need for facts about the wind led to the appearance of the fourth, and concluding, volume of this work during the war before the first three volumes had been written. The first volume, entitled *Meteorology in History*, which appeared two years ago, was the subject of an essay-review in Science Progress for April 1928, p. 686. This year gives us the second volume, entitled *Comparative Meteorology*, which passes on to a consideration mainly of our present knowledge of the atmosphere over the whole globe, at high levels as well as near the earth's surface.

It is difficult to select a single item for notice out of an almost encyclopædic volume like the one under review. In considering only one aspect of the work, the most obvious one has been chosen. A glance through the book shows that "normal" or "average" states of portions of the whole atmospheric circulation form the subject of a large proportion of the two hundred and twenty-five beautifully executed figures, while the pages are strewn with tabular matter of a like kind. In a certain sense it may be said that averages smooth out and concel the essential facts about the atmosphere, presenting situations which never have and probably never will be realised, but although the compilation of averages should not be the principal aim of meteorology, they are of immense value. Sir Napier has been at pains to collect statistics with regard to items not commonly included in the subject, for example the distribution of earthquakes, active volcanoes, the limits of polar ice in the oceans and the frequency of aurora borealis. For more ordinary matters, such as cloudiness, rainfall, pressure at sea-level, at 2,000, 4,000, and 8,000 metres, temperature of the air near ground-level, temperature of sea surface, there are

generally very clear maps showing the elements as isopleths, one for the northern and one for the southern hemisphere. For the first three items, and for surface temperature, each month is considered separately.

Although this work will probably be consulted most often for the immense number of facts recorded in it, theoretical matters of great importance are dealt with in the author's characteristically clear way. Thus, cyclones and anticyclones, and various theories about them, are discussed towards the end of the book. The statistical material is evidently of no avail in bringing to light the true causes of these phenomena, for antagonistic theories can be held by the different schools, and which are true and which false can only be found out when we possess far more detailed knowledge of the condition of the atmosphere at particular instants, and up to considerable heights, than we possess even with regard to average states. Even the question as to whether some or all of the cyclonic depressions that pass across the British Isles so frequently are symmetrical whirls at heights of 4,000 metres or more cannot be answered at present.

In the last chapter, entitled " Foundations of Theory," the author seeks to reduce the complexities of the upper atmosphere into some kind of order by means of forty-one articles dealing with vorticity, resilience, convexion, and so forth.

The physical ideas involved are in some instances not easy ones to grasp, and the significance of certain of the articles is not immediately apparent; but because parts of the work can be followed only by the most advanced students of meteorology it should not be thought for a moment that it is not suitable for every class of student. The amount that will be learned will naturally vary with the mental calibre of the individual and his knowledge of physics; the only demand that will be made from all alike is the possession of opportunities for prolonged quiet study without which the magnitude of the work will make it prove overwhelming.

REVIEWS

MATHEMATICS.

The Foundations of Euclidean Geometry. By HENRY GEORGE FORDER. [Pp. xii + 349.] (Cambridge: At the University Press, 1928. Price 25*s*. net.)

MR. FORDER has written a textbook of Euclidean geometry developed logically and with great care from fundamental axioms. It was well worth doing. No such systematic and complete treatment has been attempted before, though there are several corresponding works, *e.g.* Veblen and Young, on the wider and perhaps more interesting subject of projective geometry. The author has naturally made much use of the great work of Hilbert and his followers; and doubtless of the report by Enriques in the *Encyklopädie der mathematischen Wissenschaften*, III A, B 1, though curiously enough I have not discovered a reference to this last. The absence of any attempt at an author index makes it difficult to be quite sure.

The treatment, after an introductory summary of the logical development of non-geometrical work to be used in the sequel, is the familiar one of introducing axioms, a group at a time, and seeing how far they lead us. We begin with axioms of order, then add axioms of congruence, then a parallel axiom, and finally a continuity axiom. Then the edifice may be completed. It is also shown that all Euclidean geometry follows from the axioms of order, a parallel axiom and a continuity axiom, or alternatively from axioms of order, a weaker parallel axiom, a continuity axiom and certain axioms of congruence, and that both sets of axioms are consistent and complete.

There are chapters on proportion, on coordinates, on polygons, and polyhedra. Finally, there is a sketch of a treatment, due to Pieri, in which congruence can be taken as the only undefined relationship between points and a last chapter on the angle-sum of a triangle and non-Euclidean areas.

This is emphatically, as the author says, not a book for a beginner, but any serious student of geometry will find it of great value as a complete logical scheme, though, save for the less formal notes, printed in smaller type, it is rather heavy reading.

But what does the author really mean when he says on p. 309: "We have thus succeeded in erecting Euclidean geometry on an excessively (*sic*) narrow basis"?

F. P. W.

The Cipher of Roger Bacon. By WILLIAM ROMAINE NEWBOLD, edited by ROLAND GRUBB KENT. [Pp. xxxii + 224, with 32 plates.] (London: Oxford University Press, 1928. Price 17*s*.)

THE evidence that Roger Bacon discovered gunpowder rests upon a phrase in his *Epistola de Secretis*, which contains the sentence, "Sed tamen sal petrae *luru vopo vir can utriet* sulpuris; et sic facies tonitruum et coruscationem, si scias artifium." It has been concluded that the words *luru vopo vir can utriet* are in cipher, and Col. Hime decoded them so that the sentence read, "But take of saltpetre 10 parts, 7 of charcoal, and 7 of sulphur." There is evidence that Bacon wrote other tracts in cipher and the Voynich Manuscript is one of these.

In 1912 this cipher manuscript of 116 quarto leaves came into the hands of Mr. W. M. Voynich, who was able to trace some of its early history. It was probably written during Bacon's imprisonment (1277–92), and may have remained hidden until the dissolution of the monasteries, after which it came, perhaps as early as 1547, into the possession of Dr. John Dee. Its cipher would have baffled Dee, who took it with him on his continental journeys and made a present of it to Rudolph II at Prague about 1585. The Emperor, although well versed in alchemy and medieval science, was also unable to understand the work, and lent or gave it to the director of the Prague Botanical Gardens, one Hořčicky, or in Latin, Jacobus Sinapius. It was probably given to Hořčicky on account of the botanical and biological illustrations it contained. As Hořčicky inscribed in it the signature *de Tepenec*, a title he received from Rudolph in 1608, it may be presumed that the manuscript was given to him between that year and 1612, when Rudolph died.

Marcus Marci, a scientist of repute and rector of the Prague University, most likely secured the volume from Hořčicky, for there is a letter attached to the manuscript, dated from Prague, August 19, 1665, written by Marcus and addressed to Athanasius Kircher, giving the latter the manuscript with the hope that he would be able to make known its contents. In this Kircher failed, and the subsequent history of the work is not told.

When Mr. Voynich obtained the manuscript he supplied photographs of some of the leaves to French experts and other scholars, but only the late Prof. Newbold made any progress in deciphering the work. His clue was obtained from a phrase on the last page, namely, *michi dabas multos portas*, from which Prof. Newbold inferred that the "many gates" indicated that the cipher was to be derived from a simplified alphabet of 22 or 23 letters. By combining these two at a time 484, or 529, symbols are obtained and they, according to Prof. Newbold, constitute Bacon's cipher symbols. Thus, *fi-de-li-or-um* would mean *tonus*. As this method made the text twice as long as the original Bacon omitted doubled letters, so that *Unius* became *oritur* and not *orriittuur*, as would be the case if this elision were not practised. To meet the difficulty of writing grammatical cipher Bacon had to assign two, three, four, or even more values to his symbols, the needs of the context determining which one should be selected.

From this Prof. Newbold has been able to arrive at the nature of the contents of the Voynich manuscript. The first and largest section is botanical, and in addition to the text includes 125 drawings of specimens. The second section, with 26 pages of drawings, is astronomical, the third part contains Bacon's original biological ideas, and the fourth contains drawings of flowers, fruits, leaves, roots, and pharmacists' receptacles for drugs.

An examination of the writing under the microscope led Prof. Newbold to advance the view that some drawings and letters are made up of separate elements, cryptogramic characters, sometimes resembling Chinese and including Greek shorthand signs. There seems, however, very little evidence in favour of this view, for it requires much imagination to see these characters in individual letters, which are too small to contain all the strokes that Prof. Newbold has read into them. These marks are more likely to be incomplete letters from which some of the pigment of the ink has peeled off in the course of time.

But whilst the shorthand cipher may be considered improbable, the letter symbols devised by Prof. Newbold may well constitute the code which will unravel the meaning of the Voynich manuscript. *The Cipher of Roger Bacon* has been edited by Prof. Kent from material left by Prof. Newbold, and Prof. Kent has evidently devoted much time and thought to its arrangement and has also presented an able account of the labours and views of Prof. Newbold in attempting to elucidate the meaning of the Voynich manuscript.

J. G. F. Druce.

Operational Methods in Mathematical Physics. Being No. 23 of the Cambridge Tracts on Mathematics and Mathematical Physics. By HAROLD JEFFREYS, F.R.S. [Pp. vii + 99, with 8 figures.] (Cambridge: at the University Press. Price 6*s*. 6*d*.)

THIS little book will be welcomed for several reasons. It again brings to the fore the power and directness of the operational methods devised by Heaviside. It is surprising as well as unfortunate that the subject should have suffered neglect at the hands of pure and applied mathematicians alike. Why objection should be taken to studying the rules of Q and p in differential equations any more than the rules of the differential calculus itself is difficult to understand. Doubtless the justification of the method presented too many difficulties at the time of its inception. If so it surely comes within the province of the pure mathematician to rectify the matter.

In this connection we cannot wish for any more than is given us in the first two chapters of this tract. The first confines its attention to the interpretation of functions of the operator which involve only integral powers of it. The author finds it convenient to use the operator Q in formal proofs where $Q(y) = \int_0^x y\,dx$. He then justifies its treatment as an algebraic quantity. Applying this result to a system of differential equations of the first order with constant coefficients, such as commonly occurs in mathematical physics, Heaviside's rule of solution is deduced and also the expansion theorem here called the partial fraction rule. Compared with this treatment the ordinary method is clumsy and inefficient. Instead of going by definite steps the old method guesses at a solution, substitutes the guesses, gets often far more in the way of complementary functions, etc., than is necessary, and finally becomes very complicated when equal or zero roots occur in the determinant formed from the equations.

The form of solution obtained in the linear equation of the second order with constant coefficients is, besides being easily obtained, of far more use for the purposes of actual calculation than anything the ordinary method ever produced. A very useful operation, namely $e^{hp} f(x) = f(x + h)$, is next introduced from which, as Heaviside showed, can be deduced a method of expressing any function in operational form.

In order to interpret such things as fractional powers of the operator recourse must be had to two rules due to Bromwich which reduce the problem to one of contour integration. The previous results are derived from these rules giving results, with $\Gamma(n + 1)$ replacing $n\,!$ etc., as would be expected. One other result is obtained, the value of e^{-dq} where $q = p^{\frac{1}{2}}$.

This completes the machinery of the methods. The rest of the book is a series of standard problems worked out by means of them. There is also a chapter on dispersion, in which the method of steepest gradients is used, and one on Bessel functions. After perusing these one is left with no doubt as to the advantages of the method. The necessity of having to assume solutions almost disappears, so that if the methods were generally adopted it might become possible to teach dynamics without mystifying beginners.

A. MORRIS CASSIE.

La Géométrie non-euclidienne. Par P. BARBARIN. Troisième édition suivie de Notes sur: La Géométrie non-euclidienne dans ses rapports avec la Physique mathématique. Par A. BUHL. [Pp. 176, with figures in the text and 7 plates.] (Paris: Gauthier Villars, 1928. Price, 15 frs.)

[In] this book M. Barbarin gives an interesting and instructive account of the [es]tablishment of Lobatschewsky's (or Bolyai's) and of Riemann's geometries. There follows a preliminary discussion of a general geometry, based on

that of Euclid, but retaining only those propositions that are true (or can be modified so as to be true) in all three systems. The development of the idea of this general geometry is continued in an introduction to the later analytical study of constructions, trigonometry, and mensuration in the three systems.

Finally, the chief arguments brought against the new systems are reviewed and some account given of the representations of the non-Euclidean systems in Euclidean space and vice versa. Ample references to more detailed treatments are given in footnotes, and the interest of the book is enhanced by the portraits of Lobatschewsky, Riemann, Legendre, De Tilly and Wolfgang Bolyai, which are inserted in the sections of the text dealing with their respective works.

In the main part of the book the treatment is elementary, differential geometry is absent, and but little indication is given of the projective point of view, in which the metrical properties of elements in any one of three systems appear as relations to a certain fixed quadric (or absolute). The latter idea appears, however, in M. Buhl's notes, for he devotes a section to showing the connection between Maxwell's electromagnetic equations and Cayley's theory of the transformations which leave a quadric form invariant.

Other notes give a brief sketch of the symbolism used in the developments of non-Euclidean *differential* geometry founded on Riemann's famous memoir of 1854, and of results that have followed from attempts to find analytical theories for phenomena of gravitation, light, and electro-magnetism. The importance of Stokes's formulæ in this connection is clearly emphasised.

F. B.

Sir Isaac Newton, 1727–1927. A Bicentenary Evaluation of his Work. [Pp. x + 352.] (London: Baillière, Tindall & Cox. Price 22*s.* 6*d.* net.)

THIS book consists of a series of fourteen papers prepared under the auspices of the History of Science Society, in collaboration with other American learned societies.

The majority of the papers are written on a particular branch of Newton's work, and its effect on the development of science, by well-known scientists in America, *e.g.* "Newton and Optics," by Miller; "Newton's Influence upon the development of Astrophysics," by Campbell; "Newton's work in Alchemy and Chemistry," by Newell, etc. There are also papers on Newton's Place in the History of Religious Thought, his work at the Mint, and an account of the work of John Winthrop, Newton's first critical disciple in America.

The work is beautifully got up, and the print is clear, and restful to the eyes. In an excellent paper on "Newton in the Light of Modern Criticism," Prof. David Smith raises some very interesting questions, *e.g.*, "What were Newton's real contributions to world knowledge?"

"Was he the hero when he closed the oak at Trinity to open the gate of fortune at the Mint?"

"Why was he so slow in announcing his scientific discoveries?" and many others.

This fills one with eager anticipation to go on with the book to find the answers, but, on the whole, one is disappointed, and that, perhaps, because one expects too much.

That Newton betrayed his trust is voiced once more: "That Newton, of all men born upon British soil, should have passed his later years in looking after clippers of coin, or in acting as a gatherer in of taxes seems to me particularly pathetic" (p. 7).

Contrasts in his character are well brought out, *e.g.*, he loved the solitude of a student, and yet abandoned it for a life in the social atmosphere of the metropolis. He challenged a royal decree and later accepted a knighthood.

He dreaded controversy, and yet, when forced to it, would valiantly assert his rights.

Prof. David Smith's estimate of him reads: "Here was a very human being, with human virtues, and human frailties, and yet a succession of pilgrims continually stand in silent awe before his monument in the Valhalla of the British World, and may we not unite in saying that they are justified in so doing?"

The paths of Newton's discoveries have been so well trodden during the past two centuries that the bulk of the material found in these papers is already familiar ground to all students of the history of science; the matter has been so well put together, however, that it is worthy of the perusal of all serious readers, and its directness and lucidity should make its meaning quite clear to the non-scientific reader.

New light is thrown on certain points; in particular, I would mention the paper on "Newton's Twenty-years' Delay in Announcing the Law of Gravitation," by Prof. Florian Cajori. The accepted value of the earth's radius in England in Newton's time is gone into in great detail, and the evidence for thinking that Newton's delay was due to a wrong estimate of this is analysed with great care; his conclusion is that the delay was due to theoretical difficulties in the earth-moon test.

The paper on Newton's Chemistry gives a fair amount of scattered information; the conclusion is interesting: "His chief purpose does not seem to have been the transmutation of base metals into fine gold, but rather a diligent search for a great principle which would transform disconnected phenomena into a philosophic system." There is a certain amount of overlapping in these papers, but no more than one would expect from the nature of the case. This book is a sane appreciation of a man of genius by men capable of judging his great work.

J. R. Morgan.

ASTRONOMY.

Astronomy and Cosmogony. By J. H. Jeans, M.A., D.Sc., LL.D., F.R.S. [Pp. x + 420.] (Cambridge: at the University Press, 1928. Price 31*s.* 6*d.*)

Since the publication ten years ago of *Problems of Cosmogony and Stellar Dynamics*, Sir James Jeans has been continuously engaged in probing still more deeply into the mysteries surrounding the constitution and evolution of the various classes of astronomical bodies and systems; these researches he has woven into a connected story in the handsome and imposing volume under review. The book opens with a chapter on "the astronomical survey of the universe," which for clearness, compactness, and completeness could hardly be surpassed. The second chapter on the light from the stars is an admirable account of the physical and mathematical principles required in interpreting the observations of the radiation emitted by the stars. The next chapter deals with gaseous stars partly following the lines developed by Eddington. It will be remembered that in Eddington's theory of gaseous stars, the product of the opacity coefficient and the average rate of generation of energy within the star are taken to be constant; this Jeans regards as a special case in a wider survey of the problem. The author abandons the concept of a purely gaseous star in which the pressure is given by Boyle's law, for his calculations lead him to the conclusion that the capacity of the stellar atoms for stopping radiation demands that the atomic weights of the stellar elements should be some hundreds or thousands of times the atomic weight of hydrogen—clearly very improbable values. Reconciliation between the opacity and reasonable values of the atomic weights is effected by supposing that the deviations from Boyle's law are so great that the stellar material forming the core of the star is in a liquid or semi-liquid

state. In Chapter IV the author considers the source of stellar energy and shows that the supply must result from the annihilation of the stellar atoms themselves. The stability of stellar structures is examined; two conditions for stability are necessary—the first, that the atoms must liberate their energy spontaneously, not only by transformation into other atoms, as in the case of the radioactive elements, but by complete annihilation as well; the second, that as terrestrial atoms do not behave in the way just indicated the stellar atoms forming the core must be "super-radioactive" with atomic numbers greater than 92 (the atomic number of uranium). In Chapter V—on Liquid Stars—Jeans develops his arguments and compares his theoretical results with observation. Briefly, the white dwarfs are regarded as stars in which the atoms are stripped bare of electrons right down to the nuclei; the stars of the main series have their atoms reduced so that only the K electronic ring remains; in the giant stars the atoms maintain a greater complexity as ionisation still leaves the K and L rings intact. His theory allows him to partition off a Russell diagram into regions of stable and unstable configurations, and it is this theoretical diagram that is superposed over the observed Russell diagram displaying the absolute magnitudes and spectral types of 2,100 stars of the Mount Wilson spectroscopic parallax programme. The fit is made as close as possible by assigning definite values to certain constants in the equations; from these values, Jeans concludes that the stellar atoms have atomic numbers of about 95. Chapter VI contains Jeans's theory of stellar evolution. Perhaps it is more profitable to quote one of the author's many delightful descriptive touches. "The evolution of the stars must no longer be compared to the steady march of an army through a perfectly flat featureless plain, but rather to the movements of an army scrambling down, and possibly at times up, a succession of terraces. The different terraces are the bands of stable configurations [in the Russell diagram already mentioned] which correspond to the jamming of atoms ionised down to different rings of electrons. A star stands on the terrace corresponding to one ring, and then, stepping on to the slippery unstable region between this terrace and the next, drops down to the next lower terrace—from the giant branch to the main sequence, let us say." The next three chapters are devoted to the difficult mathematical theory of the configurations of liquid and compressible gravitating masses rotating as rigid bodies. Chapter X—"The Rotation and Fission of Stars"—contains Jeans's beautiful theory of the effect of radiation within the star on the rotation of its different layers. As he puts it: "The transmission of radiation has so braked the rotation of the outer layers that they have been reduced nearly to rest and may rightly be regarded as slowly rotating veils drawn round the stars, concealing the more energetic rotations within." The following six chapters deal with a diversity of cosmogonic problems too numerous to mention; but some idea of the subjects discussed may be discerned from the chapter headings: Evolution of Binary Systems; The Ages of the Stars; The Great Nebulæ; Variable Stars; The Solar System. The concluding chapter is a masterly summing-up of the subject-matter of the book. It may be mentioned that the only important class of celestial object not dealt with in the book is the class known as "Novæ," or temporary stars.

Sir James Jeans's volume is a magnificent survey of the great problems of the universe, and he deserves the warmest congratulations of the scientific world on his achievement. It is perhaps not to be expected that all his theories—in particular, those dealing with liquid stars, stellar evolution, and Cepheid variation—will rally the whole of the astronomical world around his banner, for rival theories are also strongly in the field. The author, of course, discusses the latter in relation to his own theories, and defends his own points of view with great vigour. The descriptive parts of the

book are delightfully written and should commend themselves to those who will prefer to pass over the mathematical analysis. The book, which is beautifully illustrated with astronomical photographs, must assuredly act as a powerful influence in stimulating research in the paths so skilfully trodden by its gifted and distinguished author. W. M. SMART.

A Guide to the Constellations. By SAMUEL G. BARTON, Ph.D., and WM. H. BARTON, Jr., C.E., M.S. [Pp. 74, with 16 charts and 5 figures.] (London: McGraw-Hill Publishing Co., 1928. Price 12*s*. 6*d*.)

THIS work contains a set of excellent charts of the constellations visible in the United States—the charts are constructed for the latitude 40° N., that of Philadelphia—together with supplementary charts of the southern circumpolar constellations. The authors have limited the range of stars to those as faint as visual magnitude 4·5; thus the charts are not overburdened with details. In the various charts, too, they have taken account of atmospheric absorption, so that by adopting this plan, they show the stars as they are seen at the particular date and time indicated on the chart. Opposite each chart is a list of the brightest stars (brighter than magnitude 1·5); as this list never contains more than ten stars, it seems a pity that the names of these stars are not given, in the charts themselves, in a type different from that in which the names of the constellations are printed, as they would serve as admirable reference points for those beginning the study of the heavens. The explanatory matter, dealing with the simpler ideas of elementary astronomy, is well written, and should prove interesting and useful to the student. A bibliography and a comprehensive index add to the value of the book.

This work has many merits, and it is warmly recommended.

W. M. S.

Müller-Pouillets Lehrbuch der Physik. 11 Auflage. Bd. V–Zweite Hälfte. Physik des Kosmos. By P. TEN BRUGGENCATE, R. EMDEN, K. GRAFF, T. HELLERICH, J. HOPMAN, H. KIENLE, E. VON DER PAHLEN, C. WIRTZ, and edited by A. KOPFF. [Pp. xii + 595, with 139 figures and 14 plates.] (Braunschweig: Friedr. Vieweg & Sohn Akt.-Ges., 1928. Price R.M. 36 (stitched), 39.50 (bound).)

THIS is a comprehensive work embracing almost every department of astronomy and astrophysics; celestial mechanics, with its vast literature and extensive ramifications, is outside the scope of the volume, but with this exception there is very little in modern astronomy that does not find a place in these pages. It is inevitable that the treatment of many of the subjects with which the volume is concerned cannot be pursued with the same amount of detail as in the treatise of a specialist (this applies more particularly to those departments of astronomy that require a precise and often long mathematical exposition); but the authors have achieved considerable success in laying bare the essential features of the particular problems of this nature and indicating the lines of development pursued by the various specialist workers. The chapters dealing with general practical methods, with the details of the many instruments now in the service of astronomy, and with the tabulation and interpretation of observational results, are fully and clearly written.

The volume is divided into three sections: (*a*) the general foundations and methods of astrophysics; (*b*) the detailed results of astrophysical research; and (*c*) cosmogony. In the first section are three chapters on the fundamental ideas of astronomy, on astronomical instruments and observational methods, and on the internal constitution of the stars. The second section occupies about three-fifths of the volume and deals exhaustively

with the sun, with the bodies of the solar system, with the spectra, colours, motions, distances, diameters, masses, and physical structure of the stars, with double and variable stars, with stellar clusters and nebulæ, and with the structure of the galactic system. The third section (on cosmogony) is descriptive in character, and although rather brief, gives a good account of the historical development of cosmogonic and evolutionary ideas since the time of Kant. The volume closes with an account of relativity theory.

The value of the book is enhanced by fourteen plates containing photographs of celestial objects, and by a large number of figures in the text, many of which are reproductions of photographs. Of the latter may be mentioned Fig. 105, showing the positions of 249 small non-galactic nebulæ in a region of the sky a little more than a quarter of a square degree in extent. Of historical interest is Fig. 123, a reproduction of Kapteyn's earliest pictorial representation of the phenomenon of star-streaming, described at the meeting of the British Association in South Africa in 1905.

The editor and his collaborators must be congratulated on producing a volume, almost encyclopædic in its range, that embodies so admirably the combined qualities of a textbook and work of reference. W. M. S.

PHYSICS

Physics in Medical Radiology. By S. Russ, D.Sc., L. H. Clark, Ph.D., and D. H. Walters, M.Sc. [Pp. xii + 234, with 72 illustrations.] (London : Chapman & Hall, 1928. Price 12*s.* 6*d.* net.)

About eight years ago a Diploma in Medical Radiology and Electrotherapy was instituted at Cambridge University and very shortly afterwards the Universities of Liverpool and Edinburgh took the same step. The institution of the diploma emphasised the important part radiology plays in medicine and at the same time it recognised the need for a definite course of training in the subject by the radiologist. The syllabus, proposed at the time and still adhered to, covers a wide field ; the first part of it deals with the physics of the subject, and the second part comprises the medical aspect of radiology and electrology. The present volume is written with a view to covering the first part of the syllabus. The authors are well equipped for the work, having had some years' experience in teaching physics for the diploma course.

The science of medical radiology is based on physics ; we may almost say that it is as much a part of physics as of medicine. It is a science which falls into the avenue between these two branches of science, each of which is extensive in itself and an intimate knowledge of both is within the reach of but a few. The problem confronting the authors was to supply medical men with a sufficient amount of information about the physics of radiology to enable them to turn with confidence to their apparatus and to understand the physical phenomena underlying their work. At the same time they were to present the subject without assuming much knowledge of mathematics on the part of the reader. The authors have undoubtedly been successful in their task, for here we have a volume in which the reader may find presented in simplest form a concise, clear, and accurate account of the fundamental physical principles underlying medical radiology.

The first seven chapters in the book deal with electrostatics and the electrical conductivity of gases, leading on to the production and the properties of X-rays together with the measurement of X-ray intensity. In the chapter on Electromagnetic Waves due place is given to the properties of ultraviolet radiation, treatment by which has been emphasised and investigated in recent years. The most modern advance in our knowledge of X-ray wave-lengths from crystal analysis is lightly touched upon in Chapter VII. This is essential for the proper understanding of the measurement of the quality of X-ray beams with such instruments as the Seemann

spectrograph, to which reference is made in the book. The next three chapters deal with radioactivity and contain valuable information concerning dosage in radium-therapy. A useful chapter follows on photography. It deals with light and X-rays and is of importance to the radiologist who wishes to perfect his technique. The remaining chapters (XII to XVII) deal with electric motors, alternating current circuits, and high-tension generators, together with high-frequency and diathermy currents. A chapter is devoted to the different types of electrical measuring instruments employed in radiology.

Two appendixes appear at the end of the book. The first contains the full text of the recommendations of the X-ray and Radium Protection Committee, and the second, an excellent description of a "Hospital Radium Service," being a reprint of a recent paper by Russ and Somerset in the *British Journal of Radiology*. These two appendixes are of value to the radiologist, and it was a wise choice on the part of the authors to include them in the book where they are at hand for ready reference.

The book is well printed on good paper. The diagrams throughout are simple and well drawn. There is a good index.

We can heartily recommend the book to radiologists. The volume contains a fund of valuable information necessary for their work, and is presented in a very lucid manner. Not only will the book be welcomed by those who are directly concerned with the examination for the diploma, but we feel it will also be acceptable to all who serve in the wide field of medical practice.

E. A. O.

Cours d'Électricité Théorique. By J. B. POMEY. Tome II. [Pp. 373, with 51 figures.] (Paris: Gauthier-Villars et Cie. Price 100 fcs.)

THE first volume of this work was published in 1914, and contains a general theoretical account of electricity and magnetism suitable for advanced students. The greater part of the present volume is devoted to the propagation of signals along wires, a subject on which the author is well qualified to write, since he is chief engineer des Postes et Télégraphes. Unlike the great number of books appearing at present on telephony, power transmission, etc., this one is not merely a collection of practical instructions with examples. The author aims at a theoretical account which will bring out the desired results with as little trouble as possible, but which will be at the same time consistent and logically deduced from first principles. Such a treatment has always proved difficult from the mathematical standpoint, and Pomey is to be congratulated on the elegance of the methods he uses, many of which are original.

The ordinary equations connected with the flow of electricity along wires were deduced in the first volume. The second begins with the equations appropriate to the submarine cable, *i.e.* where leakance and inductance are not appreciable. The method of solution is virtually that of Heaviside's operational calculus, but the somewhat empirical nature of the latter is obviated by recourse to the now well-known process of contour integration. After considering various states in the cable itself, different expressions for the current at the receiving end are discussed with the help of certain relations which the author has shown to exist between these and elliptic functions. In order to solve the telegraph equations, in which leakance and inductance can no longer be neglected, the ordinary method of forming a propagation equation from the current and potential equations is given up in favour of the method developed by Poincaré. This consists in applying the equations of the electromagnetic field to the space occupied by the wire and that surrounding it. In this way it is possible to obtain a Bessel equation for the vector potential in these two spaces. Telephone conditions will not,

however, submit to this novel method of treatment quite so easily, and the author reverts to the consideration of the equations of propagation and derives the propagation constants in the ordinary way. In solving the equations the author uses a very ingenious method due to Aguillon which, with the help of Lagrange's formula for the reduction of a multinomial surd, gives the values of α and β (the wave-length and attenuation constants) in the form of infinite series in R, C, L, S and j. Thereupon follows a treatment of the important question of reflection at junctions or discontinuities in the cable constants. There is also a chapter on the calculation of such constants as the various end impedances in which the author replaces the usual hyperbolic table methods by making measurements on a logarithmic spiral.

The theory of artificial lines is next dealt with at considerable length. In studying these, different authors have used different characteristics as the means of identification and comparison with actual lines. The subject is, of course, an important one, since the success of laboratory tests in predicting how a real cable will behave depends upon it. To facilitate and unify matters the author attempts to express all these various characteristics in terms of a quadratic function of the currents or potentials at the sending and receiving ends; obviously a very satisfactory method. He also deals with the equally important subject of electric filters as developed by K. S. Johnson and M. Lange. A summary account of loaded lines and the theory of the telephone completes this part of the work.

The rest of the book rather lacks design, and except that the subjects chosen are of more or less interest to those engaged in telegraphy and to some extent in the mechanism of the æther, there is no reason why a hundred and one similar topics might not have come under the title of theoretical electricity. These chapters are, however, extremely well written and form excellent complete summaries in themselves, as, for example, the one on the propagation of electromagnetic waves.

As a whole, the work is more mathematical than descriptive, and the author makes free use of the extremely helpful process of applying dynamical forms, such as Lagrange's equations, to the solution of electrical problems. At the same time, things likely to give results of practical value are cared for first. The writing has, too, that logical directness and lucidity of so many French authors which make easy and interesting reading.

A. Morris Cassie.

Physics for College Students. By A. A. Knowlton, Ph.D., Professor of Physics, Reed College. [Pp. xix + 641, with 437 illustrations.] (London: McGraw-Hill Publishing Co., 1928. Price 18*s.* 9*d.* net.)

The object of this book is to present a course of elementary physics in such a form that it will be attractive to a student, whether he is forced by professional requirements to acquire a knowledge of physics or not. The author, when teaching physics in an engineering college, felt no particular necessity to emphasise the objects of a course of study in physics to his students, but on appointment to an arts college, where the students apparently had free choice of subjects for their curricula, he was forced to consider the teaching of physics in an entirely different light. He found it necessary to teach physics from a humanistic rather than from a technical standpoint, and this book shows the general lines along which he proceeded. The familiar division of the subject-matter into distinct branches such as heat and light is not found here, and a rather unusual amount of modern physics, such as radioactivity, X-ray spectra, and relativity, is included in the course. The essential principles of physics are described in short chapters, each intended to be read at one sitting without undue fatigue to the student, and room is found for the large amount of modern physics by some omission of the

description of classical experiments, lecture apparatus, and laboratory measurements. Each chapter is completed with a summary, a set of problems, and, usually, a list of references for further study. Incidentally, a certain amount of description of classical experiments creeps into the problems; for example, we find a diagram of Rowland's apparatus for the determination of the mechanical equivalent of heat in a problem at the end of a chapter "On the Nature of Things."

The book is exceedingly well printed and illustrated, and will be of interest to all teachers of physics who have tried to make physics appeal to students who have no particular aptitude or desire to study the science, although they will probably not deem it necessary to cease to follow the traditional division of the subject-matter into distinct sections in order to make it more interesting. L. F. B.

CHEMISTRY.

Chemical Affinity. By L. J. HUDLESTON, M.C., B.Sc., A.I.C. [Pp. vi + 132, with 5 figures and tables in appendices.] (London: Longmans, Green & Co. Price 7s. 6d. net.)

A VERY satisfactory feature of the progress of physical chemistry is the emergence of thermodynamics from a phase of subtile abstraction into a state in which it is proving of constant service to physicists and chemists. The development of the ideas now embodied in the "third law" has helped to bring this about, but we should not be witnessing this revival were it not for the work of collecting and standardising numerical data of which G. N. Lewis has been the focus. No book on this subject surpasses Lewis and Randall's *Thermodynamics* where a comprehensive treatment is desired, but this work is long, and requires considerable time and mental exertion for its proper assimilation. There must be many who are unable to devote much time to the subject, but would nevertheless be glad to learn sufficient to enable them to take advantage of the aid that modern thermodynamics can give in surveying research problems. To such Mr. Hudleston's little book can be warmly recommended.

The first two chapters serve to introduce the reader to the concepts of energy and entropy. The treatment is lucid, and calculated to hold the attention of the reader. The only point in this section which is open to criticism is the promulgation of the idea that heat entering a system resides there as "thermal energy." The introduction of yet another term into thermodynamic nomenclature is in any case to be deplored, and this one is likely to lead an unwary reader into the pitfall of unwittingly assuming that in a complete cycle of operations the net heat exchange between a system and its surroundings is zero.

In the next three chapters the author considers free energy, solutions and the third law. We have here a skilful condensation into 70 pages of 300 pages of Lewis and Randall. The shortening is mainly achieved by an omission of all finer points, with the result that the main theme stands out clearly.

In the last chapter, as the author tells us in his preface, lies the purpose of the book. Here, by means of some score of well-chosen examples, the way in which thermodynamics can assist research workers in surveying their problems, estimating the most favourable conditions for experiment and studying the possibility of disturbance by side reactions is ably illustrated. In one particularly interesting example, the hydrolysis of silicon tetrafluoride is discussed. It is found to be a case in which the affinity ($= RT\ln K$) of the reaction is increasing with temperature, but so slowly that $\ln K$ is actually decreasing, and approaching asymptotically a finite limiting value. Thus we are to expect hydrolysis to be encouraged by raising the temperature, but at no temperature will it approach completion.

R. K. SCHOFIELD.

Cohesion and Related Problems. A General Discussion held by the Faraday Society, November 1927. [Pp. 130, numerous figures.] (London: Gurney & Jackson. Price 10*s.* 6*d.*)

In Nature around us there is a perpetual state of internal conflict. Thermal agitation which is for ever urging the ultimate particles of matter to separate from one another has always to contend with their innate tendency to cluster together. In the three states of matter the protagonists have different fortunes. In gases and vapours, thermal agitation is victorious, though cohesion has sufficient influence to prevent this state from being "perfect." In liquids the battle is indecisive, and it is for this reason that this state has proved so difficult to investigate quantitatively. In solids, however, cohesion has the upper hand. Thus, as is to be expected, the kinetic theory has mainly been advanced by studying gases; while the study of solids seems likely to lead to a more exact knowledge of the nature of cohesion. The study of solids has been revolutionised by modern views on atomic structure and by X-ray examination of crystals. This has necessitated an entirely new start in the study of cohesion, which promises to bear fruit in the near future.

Of the papers contributed to the Faraday Society's discussion under review one by A. F. Joffé of Leningrad is of special interest. An account is given of experiments designed to trace the cause of the discrepancy known to exist between the cohesive strength of a rock salt crystal, as determined by the usual experimental methods, and that which, according to Born's calculations, is to be expected of an ionic latice held together by electrostatic forces. Evidence is submitted supporting the view that, unless special precautions are taken, premature failure occurs due to the growth of cracks originally present at the surface. The discrepancy between the electric field found to cause a breakdown of a rock salt crystal and that calculated by Born is also considered. This leads to experiments on very thin layers of dielectric which, besides throwing light on the point at issue, are likely to have practical application. No one reading the paper can fail to be impressed by the experimental ingenuity of the author.

Prof. Masson makes a timely contribution to the discussion in which attention is drawn to facts which show that cohesion of the Van der Waals type is different in kind from that made manifest by chemical action. Prof. Lennard-Jones and Miss Dent, in the course of a mathematical paper, surmise that Van der Waals attraction between electrically neutral atoms arises because each is polarised in the presence of others, and the temporary distortion of the electronic shells gives rise to an attraction. The complication presented by the cohesion in a crystal latice is evident when it is realised that Van der Waals attraction, electrostatic attraction and co-valent linkage (electron sharing) may all operate simultaneously. A number of authors, including Mr. N. K. Adam, consider the phenomenon of adsorption. It is generally concluded that Van der Waals attraction is not in the main responsible for adsorption.

While cohesion is the principal topic of discussion it is natural that some attention should be devoted to the force which prevents atoms from interpenetrating one another. Prof. Masson points out that, since a mixture of ethylene and oxygen is stable in the absence of activating agents, the repulsive force which causes an oxygen molecule and an ethylene molecule to separate again after encounter must prevent an approach close enough to cause the internal upset that would lead to chemical action. It follows that activation must cause the mutual repulsion to be overcome. The importance of considering intermolecular repulsion, now that we no longer think of molecules as resembling billiard-balls, is also brought out in a paper by Prof. Richards.

A number of papers are devoted to a consideration of cohesion in metals. The study of large single crystals which has been so fruitful naturally figures

prominently. The question of the part played by the interior of the crystals and the crystal boundaries respectively in the strength and different types of failure in metals is discussed, and a number of views are expressed. The difficulty of this subject is illustrated by the fact that Prof. G. I. Taylor is unable to make up his mind whether a small or large shearing force is required to cause all the atoms in one plane of a perfect crystal to slide over the atoms in a neighbouring parallel plane. R. K. SCHOFIELD.

Radio-elements as Indicators, and other Selected Topics in Inorganic Chemistry. By FRITZ PANETH, University of Berlin. [Pp. 164, with 17 figures and 28 tables.] (London: McGraw-Hill Publishing Co., 1928. Price 12*s.* 6*d.*)

ISOTOPES, though they can be separated by certain physical means, as in the mass spectrograph, do not become separated in any chemical reaction. When, therefore, a trace of thorium B is thoroughly mixed with a lead salt in solution, and potassium chromate is added, the thorium B and lead atoms being isotopic will be mixed in a uniform ratio throughout the precipitated chromate. If, after washing, the precipitate be shaken with water in a thermostat so as to form a saturated solution, the ratio of the activity of the solution and residue as found by an electroscope will give the ratio of the amounts of thorium B present. But the ratio of lead and thorium B is unalterable by chemical means, and must therefore be the same in solution and residue. Thus the electroscope reading gives the ratio of the amount of lead in the solution to that in the residue. That in the residue can be found by weighing, and hence the solubility of lead chromate, immeasurable by ordinary analytical methods, has been determined. A similar method can be used to demonstrate the fact that the particular atoms or radicals assumed to be liberated by electrolytic dissociation do not retain their places in given molecules, but are free to move back and forth from one place to another. Thus on mixing equimolar amounts of lead chloride and radio-chemically sensitised lead nitrate in solution, and allowing lead chloride again to crystallise, the latter is found to be half sensitised by comparison with the original nitrate. No such effect is found with lead chloride and lead tetraphenyl in pyridine, showing that the lead tetraphenyl does not dissociate. By a somewhat less direct, but very ingenious, method based on the use of radioactive indicators, independent evidence is obtained in support of the view that true adsorption takes place in a monomolecular layer.

The above are only three examples of the uses to which radio-active indicators have been put by Hevesy, Paneth, and co-workers. These and many others are set out in the work under review, which is the substance of a course of lectures delivered at Cornell University under the George Fisher Baker Non-Resident Lectureship in Chemistry. Radio-chemistry in general and other selected are also touched upon. The introductory lecture is on Ancient and Modern Alchemy. In it the author makes a plea for a more sympathetic attitude towards Aristotelean philosophy than that usually accorded it in modern scientific writings. The praise given to Robert Boyle cannot fail to please the English reader.

Discussion of "The Group of Volatile Hydrides" and of "The Natural System of the Chemical Elements" is also included in the lecture course. The author's discovery and isolation of the volatile hydride of tin doubtless prompted him in his choice of the former topic. The classification is very thorough and painstaking, and is likely to prove of value in the development of the theory of molecular structure. The natural system of the chemical elements is treated on a broad basis. The relative advantages of using the short and long periods as the basis of the periodic table are gone into. Finally, the author considers the question as to whether more than ninety-two elements exist on the earth. R. K. SCHOFIELD.

Mathematical Preparation for Physical Chemistry. By FARRINGTON DANIELS, Associate Professor of Chemistry, University of Wisconsin. [Pp. x + 308, with 65 figures.] (London: McGraw-Hill Publishing Co., 1928. Price 15s.)

MANY a student selects chemistry rather than physics as his principal subject because he knows himself to be weak in mathematics or because he has no inclination to study the subject. Embarking on chemistry thus, he is certain to find his lack of mathematics a difficulty when dealing with physical chemistry. This is a greater pity than he knows; because it in all probability leads him to neglect what is not so much a branch of chemistry as an aspect of chemistry. His bias towards the synthetic and analytical side inevitably prevents him from obtaining a true perspective which is the most valued result of all scientific study. And all this is due to lack of mathematics. There are doubtless some who, in spite of earnest effort and good teaching, seem unable to acquire any mathematical facility. There are others who equally lack the facility, but do so from a lack of opportunity rather than a lack of aptitude. To one in this class who has decided to make a serious study of chemistry, and who is unable to attend a regular course in mathematics, Dr. Daniels's book should prove helpful. In particular if he should have to undertake his study of mathematics without other guidance, he may feel sure that in studying this book he is not spending valuable time in mastering parts of the subject which will not be of direct use to him.

The thought of anyone studying mathematics merely as an aid to chemistry will no doubt be painful to those who have grown to appreciate mathematics for its own sake. But the state of a student who requires such a book as this is desperate, and even a mathematician must agree that it were better that he should master a few branches selected for a utilitarian purpose than that he should remain as he is.

The book has many attractive features. The constant use of graphical methods is well suited to its purpose. The presentation of the properties of the exponential is well carried out. A bold attempt is made to lay bare the mysteries of the partial differential coefficient which is so important in thermodynamic chemistry.

Besides being a book that some students would do well to buy, it is one that will be a valuable addition to our libraries. Probably the majority of our students will be acquainted with much that is in it, but will be glad to have it at hand for reference.

R. K. SCHOFIELD.

Elementary Practical Physical Chemistry. By FRANK SHERWOOD TAYLOR, M.A., B.Sc. [Pp. xi + 130, with 51 figures and tables.] (Oxford: at the Clarendon Press, 1928. Price 3s. 6d. net.)

THE author gives us in this admirable little book the benefit of his experience as chemistry master at Gresham's School, Holt. It is intended for the use of the advanced classes in schools and the first year classes of the universities. The fact that, while writing the book, the author has been in close touch with a university teacher in the person of Mr. D. L. Hammick of Oriel College, Oxford, is a guarantee that it really is suitable for university students and is not merely what a schoolmaster thinks university students ought to do, which is sometimes very different. The lack of agreement between school and university teachers as to how a boy studying science can most profitably spend his time is one of the least satisfactory features of education in this country. It is therefore very satisfactory to have a textbook which is the outcome of co-operation between the two.

A detailed description is given of some sixty experiments. The branches of the subject which they illustrate include the ionic hypothesis, reaction velocity, the phase rule and colloids, in addition to the more usual stock-in-

trade. Considerable condensation in many of the theoretical explanations has been necessary in order to keep the book small. The student is certain to be at sea unless a preliminary account has been given by the teacher. The text should, however, be perfectly comprehensible if the ground has been prepared.

The apparatus described is all of a simple nature, and can readily be assembled from the stock generally available in a school laboratory, provided the teacher can manage the simpler operations of glass-blowing. The low cost of the book will recommend it as one to be put in the hands of the boys as well as on the desk of the master. R. K. SCHOFIELD.

Old Chemistries. By EDGAR F. SMITH. [Pp. xi + 89, with 32 plates.] (London: McGraw-Hill Publishing Co.)

THE author describes some twenty-seven early textbooks of chemistry, published chiefly during the latter part of the eighteenth and early part of the nineteenth centuries, with the object of bringing to the attention of American chemists the story of chemical books used for instructional purposes in the early days of the United States. Within the limits of the size of the book and its special object the ground is fairly adequately covered. The author attempts in a short space to indicate the views of the writer of the textbook and the influence the work had upon the advance of chemistry. In so doing he has made a valuable contribution to the history of the science, since at the period under review such books were the general method of disseminating knowledge of chemistry. The principal criticism one can offer is that a much more exhaustive treatise would have been welcome; unfortunately, chemists can no longer look to Prof. Smith for such a labour of love.

The book is well printed and contains many excellent whole-page portraits of famous chemists. O. L. B.

Abegg-Auerbach, Handbuch der anorganischen Chemie. Band IV, 3, erster Teil. Edelgase, by Dr. EUGEN RABINOWITSCH. [Pp. xii + 522, with 50 figures and 232 tables.] (Leipzig: S. Hirzel, 1928. Price 45 M.)

ABEGG'S Handbuch requires no introduction to the chemist and its high standard is fully maintained in the present volume on the Inert Gases. The physical-chemical approach to inorganic chemistry was the outstanding feature of Abegg's original plan, and since the Inert Gases can be dealt with only from this aspect one naturally expects an excellent account of them in this handbook or, perhaps more correctly, series of monographs. The author has not disappointed us. The usual arrangement is adopted, information is collected in many tables, and the subject-matter is fully referenced. Treatment is exhaustive, and Rayleigh and Ramsay in 1895 could have little thought that in some thirty years a book of this size would be necessary to describe the results of their discovery. O. L. B.

Chemical Encyclopaedia. An Epitomised Digest of Chemistry and its Industrial Applications. By C. T. KINGZETT, F.I.C. Fourth Edition. [Pp. viii + 807.] (London: Baillière, Tindall & Cox, 1928. Price 35*s*. net.)

THE preparation of a digest of chemistry and its industrial applications in a volume of some 800 pages is a formidable task, and the claim of the author that he has done so can hardly be substantiated. He has, however, collected in a concise form a large amount of information on chemistry, particularly of the kind often required at short notice by those who deal with chemical

products and processes without themselves being professional chemists. To such, the book will be of great value, and were it but used by those responsible for many of the articles about chemistry in the newspapers, chemists would be deprived of some mirth but the public might acquire more understanding. To the expert, the most useful feature of the book is the description of articles disguised under trade or proprietary names.

Although the author claims to make the work a "bibliographical clearing-house," in this he has not been very successful, many of the comparatively few references being to unimportant articles; if he had succeeded in his object the book would have been of much greater use to chemists. The information is generally accurate, but the author appears to adopt a somewhat sceptical attitude towards much of the modern work, witness the following quotations. "Isotopes, a term introduced to explain the alleged existence of very nearly allied but apparently non-separable forms of some chemical elements." "According to Ramsay, the fractional distillation of argon yields traces of other gases having distinct spectra, and to these he gave the names Neon, Xenon, and Krypton." The necessity for conciseness sometimes results in extraordinary statements; speaking of Neon, it is said that "vacuum-tubes charged with the gas and electricity produce a brilliant red light. The gas has a considerable penetrative power, and is used to some extent in landing lights . . . and picturesque advertisements."

O. L. B.

A Guide to the Literature of Chemistry. By E. J. Crane and Austin M. Patterson. [Pp. v + 438.] (London: Chapman & Hall, 1927. Price 25*s*. net.)

A common error among students of chemistry is to think that when they have obtained their first degree they are fully equipped for their profession. Few realise that a knowledge of where to find information is of more importance than the comparatively small number of facts they have learnt. Many even when engaged on research are still content to apply to their teachers on every occasion without realising that a time will come when they will not have a professor handy to answer their questions. In the past several years' experience was required to gain a knowledge of the literature of chemistry, but with the publication of this book a short cut to competence is provided. Every chemical student should read it towards the end of his training and with its aid learn to use a good chemical library. The scope of the work is indicated by the chapter headings: The Problem and Objectives, Books, Periodicals, Patents, Other Sources, Indexes, Libraries, Procedure. In a series of appendices are given: Bibliography of Articles on Chemical Literature, Symbols and Abbreviations used in Chemical Literature, A List of American Libraries of Interest to Chemists, A Bibliography of Lists of Periodicals, Scientific and Technical Organisations, Periodicals of Chemical Interest, A List of Chemical Book Dealers and Publishers, and A Select List of Chemical Books.

Although the book is written in the first instance for the use of American chemists, except in the matter of Libraries, it is as useful to the chemists of Europe. As the book is in English it is perhaps a pity that the Libraries of the English-speaking countries were not dealt with as a whole, not merely those in Canada and the United States.

Everyone with much experience of chemical literature will have criticisms to make, particularly of what has been included and what left out in the various lists; but such matters are of minor importance compared with the value of the work as a long-wanted guide to the novice when first he has to use the enormous literature of chemistry.

O. L. B.

Biological Chemistry and Physics of Sea Water. By H. W. Harvey, M.A., Hydrographer at the Laboratory of the Marine Biological Association, Plymouth. [Pp. x + 194, with 65 figures.] (Cambridge: at the University Press, 1928. Price 10*s*. 6*d*. net.)

Nobody need be alarmed at the title of this book, for it is singularly free from what is ordinarily known as biological chemistry. The chemistry throughout is of the most elementary character and the physics is served up with a minimum of necessary mathematics. The book is eminently readable and there are few botanists and agricultural research workers who would not benefit by it. It might well find a place in a modern curriculum of geography.

Quantitative results are tabulated or shown in graphic form to illustrate each theme and there are copious references to relevant textbooks and original papers. The arrangement of the bibliography is the only blot on the book; each chapter has a separate reference list printed with it nor are authors' names in alphabetical order. There is, however, a good subject index, also authors' index. The first chapter deals with conditions affecting the growth of marine plants and animals. The relation of the former to nutrient salts, sunshine, and temperature is discussed, and that of the latter to temperature and the composition of the external medium. In chapter two we find useful data as to the chemical composition of sea water.

Special attention is given to those constituents which, present in very minute amount, limit the production of plant—and indirectly of animal—life in the ocean. Of greatest importance are phosphates and nitrates which are completely used up at the surface by the phytoplankton in summer. At great depths their abundance is limited by the amount of light energy available for photosynthesis.

Chapter three contains a simplified account of the tides. Ocean currents are considered in relation to the latter and to differences in temperature and in salinity. The clear way in which Bjerknes' circulation theory is presented is especially commendable. Quantitative data are tabulated and are then used in a calculation in which an oceanic current moving with a velocity of 1·3 nautical miles per hour is shown to exist as a necessary result of the density differences between the water at the two stations considered.

Chapter four deals with the temperature changes of the oceans. These lead to the formation of warm surface layers, the reduced density of which forms a barrier to the vertical circulation of the water. The latter is of the utmost importance, since upon it depends the replenishment of the upper water with nutrient salts regenerated from animal and vegetable decomposition down below.

A brief chapter five discusses submarine illumination, the character of the light as determined by photographic work with screens and its intensity as measured by photo-electric cells.

The whole book leads up to chapter six, which considers all the factors as related to the productivity of the ocean. Interesting calculations are given as to the annual crop, data determined by direct chemical analysis being used.

W. R. G. Atkins.

The Industrial Uses of Bauxite. By N. V. S. Knibbs, D.Sc. [Pp. 141.] (London: E. Benn, 1928. Price 21*s*. net.)

Since the discovery of Bauxite over a century ago by Berthier at Baux in Provence, practically the only important use of the material has been as a source of aluminium, prepared from it by electrolysis. As such it possesses great importance though only from this one aspect.

Within recent years, however, it has been found in addition that bauxite is capable of yielding a very resistant and valuable aluminous cement and at the present day developments in this direction appear to be limited only by the price of the raw material. Dr. Knibbs has collected together a great deal

of valuable information from various sources and a perusal of this book will give one a good idea of the trend of recent developments.

The 14 chapters of the book deal shortly and concisely with the occurrence and preparation, composition and properties of bauxite, and also a short account of its use for the preparation of aluminium, aluminous cement, refractories, abrasives, and its recent applications for oil refining. The book is clearly printed and provided with adequate references and will no doubt prove acceptable to those interested in the subject despite its relatively high price.

F. A. M.

Impurities in Metals, their Influence on Structure and Properties. By Colin J. Smithells, M.C., D.Sc. [Pp. xi + 157, with 166 figures.) (London: Chapman & Hall, 1928. Price 18*s*. net.)

In the introduction to his book on *Impurities in Metals* Dr. Smithells defines his use of the term "impurities" as those constituents, whether metallic, non-metallic, or gaseous which occur naturally in metals or are added intentionally, up to approximately 1 per cent. in amount. They are "minor constituents," and if he had substituted this term in the title, which he admits he would have preferred, the scope of the book would have been better indicated. The field covered is necessarily wide and varied, and the subject matter does not lend itself to classification and generalisation. The effect of the three types of impurity on the structure, mechanical and electrical properties, and on the resistance to corrosion, etc., is dealt with in separate chapters, and this leads to a great deal of repetition. A good index is an aid to obtaining information on the effect of one particular constituent on one particular metal, which otherwise would be extremely difficult to find. Considering the amount of available data, it is disappointing to find so large a proportion of the book devoted to elementary metallography and X-ray crystallography. Most readers of a book of this kind might be expected to have a knowledge of the former and the latter is extraneous matter and can be found elsewhere. There is rather a tendency at the present time to think that no book is complete without a chapter dealing with the application of X-rays. There is even less excuse than usual in this instance, as X-ray crystal analysis has not proved of great value in detecting very minute changes in the lattice and has failed entirely in providing any explanation of the change of properties accompanying the addition of traces of a second constituent.

Dr. Smithells is to be commended for collecting a considerable quantity of scattered information and for presenting it in book form, and the printing and reproduction of the micro-photographs are excellent.

The book should prove useful to metallurgists and manufacturers until a more complete survey of the whole problem is available in a systematic form.

C. F. Elam.

BOTANY.

The Evolution and Classification of Soils. By Dr. E. Ramann. Translated by Dr. C. L. Whittles. (Cambridge: W. Heffer & Sons, 1928. Price 7*s*. 6*d*.)

One of the most striking developments in agricultural science in recent years is the general adoption of the Russian ideas as to the manner in which soils have evolved, and of the corollary to these ideas—a comprehensive system of soil classification. In Great Britain those agricultural scientists whose main interest lay in the soil were confronted by a most complicated geology, and an appreciable variation of climatic conditions from east to west and, to a lesser degree, from north to south. In these conditions, and with the self-evident variations in crops and farming methods over the country, it is not surprising that the few soil surveys made were conducted on the assumption that

geology, climate, and cropping systems were the predominant members of the framework within which the detailed soil classification would fall, and to which it would have to conform. In the large Continental areas, and especially in those regions where modern intensive agriculture was not fully developed, it was found that geology had relatively little influence on soil types. The Russian workers showed that the boundaries of different easily distinguishable soil types and the boundaries of certain definite climatic zones were in general coincident, overriding very largely the geological differences in the underlying rocks, even in those cases where the soil itself was directly derived from them by degradation *in situ*.

The difficulties of language, and the outbreak of war were responsible for the extensive Russian work remaining almost unknown, until recently, by investigators in the rest of Europe. The late Prof. Ramann of Munich was one of the very few outside Russia who grasped the predominant effect of climate in the evolution of soil. He set out his views in summarised form in 1917, and the book under review is an English translation by Dr. C. L. Whittles, formerly Director of the British Guiana Sugar Planters' Experiment Station.

The translation has been well done; it reads smoothly, and the claim that care has been taken to find suitable English equivalents for the names of the various soil types will be generally admitted.

The interest of the book is twofold. It provides, in an accessible form for English and American readers, the mature views of Ramann on soil classification and thus enables those interested in the historical development of the subject to assess correctly his share in its growth. Secondly, it deals with Western and Central European soils that are, geologically speaking, mostly young enough still to retain easily recognisable characteristics of the parent material. They stand, therefore, in some contrast with the Eastern European soils on which the climatic classification was originally developed. Ramann's discussion of these " immature " soils is of direct interest to British workers, who are confronted with the fact that in their scheme of classification they cannot ascribe only a relatively minor influence to the geological character of the various soils in this country.

B. A. K.

The Divine Origin of the Craft of the Herbalist. By Sir E. A. Wallis Budge, Kt., M.A., Litt.D., etc. [Pp. xii + 96.] (London: The Society of Herbalists, 1928. Price 5s. net.)

The Society of Herbalists is a limited company under the direction of Mrs. C. F. Leyel, and Sir Ernest Wallis Budge is the Adviser on Oriental Materia Medica. If the Society produces other books as good as the present one, it will deserve the thanks of all who are interested in the history of science. Sir Ernest has, of course, an unrivalled knowledge of ancient oriental civilisation, and he has exercised great judgment in his selection of material for the present subject. He shows that the art of healing was closely associated with magic, and that the gods themselves were responsible for teaching men the curative power of herbs.

Particularly interesting is the account of the Sumerian and Assyrian Herbals, based largely upon the researches of Dr. Campbell Thompson. The persistence of technical names is astonishing; we find, for example, that the words saffron, styrax, sesame, turmeric, and many others have been transmitted to us from the Sumerian via Greek and Arabic.

The book contains thirteen illustrations, which greatly add to its attractiveness, though some of them might have been more usefully replaced by others. It is surely unnecessary to devote three pages to transcriptions of Assyrian lists of plants in a book which is intended for the general reader.

E. J. H.

ZOOLOGY.

The House-fly, its Life-history, Importance as a Disease Carrier, and Practical Directions for its Suppression. By Major E. E. Austen, D.S.O., Keeper, Department of Entomology. British Museum (Natural History), Economic Series No. 1a. Third Edition. [Pp. 71, with 13 figures.] (London: Printed by order of the Trustees of the British Museum, 1928. Price 1s.)

This pamphlet, of which the second edition appeared two years ago, has been revised throughout and slightly expanded.

Although, as the author says in his preface, no outstanding discoveries have been made since the last edition, there are several important additions. For instance, alternative methods for the suppression of breeding grounds in municipal refuse-dumps are described and the mode of employment in America of hydrocyanic acid gas for killing the flies themselves is mentioned. This latter method is unsuitable for ordinary household use.

Among minor additions the following may be noted: chicken-dung in fowl-runs has been found in the United States of America to be a breeding place of the fly; the house-fly has been proved to be responsible for a certain number of cases of intestinal myiasis; and the close-packing of stable manure, which had been previously proved to be successful in England at least, in killing the eggs, maggots, and pupæ by the utilisation of the natural heat of fermenting manure, has now been shown to be effective in the warmer climate of Bermuda.

A very suggestive footnote is added on p. 63, drawing the attention of medical officers of health and local sanitary authorities to the outrageous fact that even now pastrycooks' wares in shops are freely exposed to contamination by house-flies.

We recommend this pamphlet to all, believing that a true knowledge of the disgusting habits of the house-fly and the subsequent danger to public health, will bring about the necessary concerted action for the prevention of its breeding and thereby destruction the sooner.

H. F. B.

The Social Insects, their Origin and Evolution. By William Morton Wheeler, Ph.D., Sc.D., LL.D. [Pp. xviii + 378, with 79 figures.] (London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace and Company, 1928. Price 21s. net.)

This book, as the author states in his preface, consists basically of twelve lectures delivered by Dr. Wheeler while occupying a Harvard exchange professorship in the spring of 1925 at the University of Paris. These lectures were published by Gaston Doin & Co. in 1926 under the title "Les Sociétés d'Insectes, leur Origine, leur Évolution," in the *Encyclopédie Scientifique*, edited by Prof. M. Caullery. The original lecture form has been retained, but additional passages omitted in the French publication have been restored, a small amount of new material has been added, and various errors have been corrected. A few illustrations appear for the first time and a few are now omitted. Finally, the work has been brought up to date as far as possible. It may be pointed out that this book is not to be confused with a former book by Dr. Wheeler, entitled *Social Life Among The Insects*, published a few years ago by Constable & Co., Ltd.

Dr. Wheeler, the leading authority on social insects, starts the series of lectures with an introduction concerning the scope and meaning of the social among insects. Four general questions relating to what are social insects, whether they have had an evolution, the peculiarities of this evolution, and the cause or conditions that may be assigned to this evolution, are dealt with and partially answered. The origin of the Terebrantia and Aculeata forms the

next lecture. Then follow successive chapters or lectures on the evolution of wasps, bees, ants, and termites; these deal in detail with those insects which have become definitely social and have passed the subsocial state reached by such insects as, for example, the Silvanidæ among the beetles, and earwigs, Embiids, and *Gryllotalpa* among other orders.

Two chapters are devoted to an excellent account of Polymorphism, and these are followed by a very lucid account and explanation of the social medium and trophallaxis or reciprocal exchange of food and other stimuli. Dr. Wheeler re-explains in some detail his conception and use of the term "trophallaxis," and answers his critics, especially Father Wasmann. He goes on to review the relevant facts and emphasises the fact that the usefulness of the principle may be extended to include other phenomena besides the exchange of food, for example, olfaction. In this connection he introduces the term "chemorecept" instead of the anthropomorphic terms "taste" and "smell." The evolution of the guests and parasites of the social insects occupies the two next chapters or lectures. These chapters, perhaps even more than the rest of the story of social insects, illustrate strange parallels between insect societies and human society.

Chapter XII is a very able conclusion in which Dr. Wheeler briefly outlines the probable evolution of the truly social insects. He also indulges, not without good cause, in more detailed comparisons of the social insects and man. There is a good bibliography of 37 pages containing references to writings of over 450 scientific workers. It seems strange, however, that no reference is made to Rösch's work. A full index completes the volume. Very few typographical errors still remain, for example, ψ instead of ϕ on p. 230. The illustrations are good.

On reading the book one is struck by the ease with which the master-hand of Dr. Wheeler deals with the whole ground suggested by the title: a thorough introduction, detailed accounts of the four groups of insects and various phenomena concerned, all following each other with great clarity. Besides appealing to the entomologist, this book must attract the attention of followers of the modern trend of thought in the fields of philosophy, sociology, and animal behaviour. H. F. B.

Leaf-mining Insects. By JAMES G. NEEDHAM, STUART W. FROST, and BEATRICE H. TOTHILL. [Pp. viii + 351, with 3 plates and 91 figures.] (London: Baillière, Tindall & Cox, 1928. Price 27*s*. net.)

LEAF-MINING insects belong to four orders of insects—Lepidoptera, Coleoptera, Diptera, and Hymenoptera—and are quite numerous, about 770 species occurring in America being dealt with in the list of leaf-mining insects in this book, but there are not many of economic importance. Leaf-miners are all larvæ, and in many cases show convergent evolution to a marked degree.

Leaf-mining Insects, as stated in Dr. Needham's prefatory note, is an endeavour by the authors to provide (1) an untechnical introduction to the study of these insects, intelligible to the general reader; (2) an account of their natural history sufficiently detailed to be useful to the working ecologist; and (3) lists of the leaf-miners, of the food plants, and of technical papers concerning them, adequate for the needs of the specialist.

Chapters I and II give one a very good idea as to what leaf-miners are in a general way and fulfil admirably the authors' aim to provide an untechnical introduction to the study of leaf-mining insects.

Chapters III to XI inclusive deal with the Lepidoptera, while Chapters XII, XIII and XIV deal respectively with the Coleoptera, Hymenoptera, and Diptera. Here the separate families and genera are dealt with in order and examples are described. Useful tables of the leaf-mining genera of Coleoptera, Hymenoptera, and Diptera, taken from Dr. Frost's previous publications, are

included. Keys to the more common North American leaf-mining Diptera based on the type of mine produced, the leaf-mining larvæ of *Agromyza* and the larvæ of *Phytomyza* are also to be found in these chapters. These chapters, presumably intended to be useful to the working ecologist, give the impression that the authors have attempted the impossible in their aim to provide a book readable alike by the general reader, ecologist, and specialist.

Chapter XV consists of a list of Leaf-mining Insects and Chapter XVI of a list of Host Plants of Leaf-mining Insects. The Bibliography, consisting of 17 pages, is arranged as follows: general references, and then under each order there is a list of general references, and finally there are references under families. One finds with regret no reference in the bibliography to Leathers' (1922) work on *Chironomus braseniæ*, which is mentioned on p. 28. There is an index of 11 pages. The last two chapters and the bibliography, compiled by Dr. Frost, will be very useful to the American collector of leaf-mining insects and will also serve as check-lists to the specialist.

Throughout the book there are numerous citations, by means of which, together with the references in the bibliography, one can follow any particular branch of the subject.

In spite of one or two minor typographical errors the book is well printed and got up generally. There are, however, several mistakes and omissions which give the book the appearance of having been got together in a hurry. For example, in the Contents it is stated that a table of orders of leaf-mining larvæ is to be found on p. 39, whereas it is on p. 35; Chapter II starts on p. 36, not as stated on p. 35; the lettering of figure 41 is incomplete, and one cannot find *Coleophora limospinnella* in the list of leaf-mining insects in Chapter XV, although it is American and is mentioned on p. 52.

To summarise, *Leaf-mining Insects* gives an excellent account of leaf-mining insects for the general reader and contains valuable check-lists for the ecologist, and specialist; but we consider that the authors have attempted too much in trying to write one book for the general reader, ecologist, and specialist, and that they would have attained their object better if they had written three books.

H. F. B.

The Larval Stages of the Plymouth Brachyura. By M. V. Lebour, D.Sc., F.Z.S. (*Proc. Zool. Soc.*, 1928, part 2, pp. 473–560, pls. 1–16.)

The study of Decapod larvæ is a peculiarly fascinating one, but the extreme difficulty of keeping them alive through the successive moults is a serious obstacle to their identification. The methods evolved at Plymouth have enabled Miss Lebour largely to overcome this difficulty, and her discovery that the larvæ of Oysters, *Echinus*, etc., are readily eaten by the zoeas has made it possible to rear some species from the egg to the crab stage. How difficult the task is is shown by the fact that even Miss Lebour was completely successful only with three species, but nine others were reared as far as the last zoea or the megalopa. Twenty-five years ago my own efforts at Plymouth at rearing these larvæ were always failures, with the single exception of *Pandalina brevirostris*, which was reared from egg to prawn.

Out of 37 species of crab known from Plymouth, Miss Lebour is now able to give descriptions from her own observations of no less than 33. Since among these most of the families are represented, her survey is of the greatest value from the systematic point of view. The larvæ are shown to be by no means so uniform in structure as has hitherto been supposed, and she has discovered features which can be used to distinguish all the genera. For example, the number of setæ on the exopods of the maxillipedes, taken in conjunction with the degree of development of the other parts, give an indication of the number of moults passed through and a strong clue to the identity. The carapace spines which are so characteristic of the Brachyuran zoea are

shown to be relatively unimportant systematically. For instance, *Pinnotheres veterum* has a large dorsal spine, while *P. pisum* has none.

Miss Lebour has been able to draw up keys for the identification of all the genera, and it will be possible also to recognise from her description most of the species of British crabs taken in the plankton. This may prove of much importance in the study of their distribution. Such knowledge as we possess, incomplete as it is, of the larvæ of the Macrura has revealed interesting facts of distribution. For instance, the Trachelifer larva of *Jaxea nocturna* is quite often taken in plankton, though the adult has very rarely been seen. Again, the larva of *Leander longirostris* was not long since identified in plankton from the Eddystone region. At the time the only British locality known for the adult was the Norfolk rivers; but quite recently it has been found to be common in the Tamar.

Miss Lebour finds that these larvæ admit of a systematic arrangement which corresponds very closely with the system generally accepted for the adults, but that there are some discrepancies upon which she lays stress. For instance, the larva of *Thia* differs so much from that of *Atelecyclus* that she finds it necessary to establish for it a new family, Thiidæ. The most important question raised is the relation between the Leucosiidæ and the Pinnotheridæ, the former being hitherto included in the Oxystomata and the latter in the Brachyrhycha. As she points out, some of the resemblances of the Pinnotherid and Leucosiid larvæ may be due in part to their both having the habit of living near the bottom, but the resemblance extends to the megalopa and probably does indicate some degree of relationship. Bourne has already shown that the Raninidæ should be separated from the Oxystomata as a distinct sub-tribe the Gymnopleura, and the differences between the larvæ ascribed by Cano to Dorippe and those of the Leucosiids are so astonishing that a real relationship seems impossible. Unfortunately here, as in many other cases, we are handicapped by serious doubts as to the reliability of Cano's identifications, and it is impossible to base conclusions on his work. There can be little doubt that we shall not reach a satisfactory classification of the Decapoda until the larval stages are taken into account; but very much more information is needed, especially concerning the Dromiacea and Oxystomata among the Brachyura.

Among the many points of interest in Miss Lebour's paper, attention may be drawn to her observation that some crabs such as *Cancer*, *Xantho*, and *Atelecyclus* attempt to mask themselves with foreign matter as soon as they moult from the megalopa stage. The habit is, of course, well known in adult Spider-crabs, but it is rather surprising to find it in the young stages of the genera mentioned.

The paper concludes with a review of work published on foreign crab larvæ which is valuable though not altogether complete. For instance, it would have been helpful if we could have had Miss Lebour's observations on the rather remarkable zoea of *Heteropanope* described by Tesch.

ROBERT GURNEY.

The British Sea Anemones. By T. A. STEPHENSON. Vol. I. [Pp. xii + 148, with 14 plates.] (London: Ray Society, 1928. Price 37*s*. 6*d*. net.)

DR. STEPHENSON'S Monograph on the British Sea Anemones has been eagerly awaited and the present volume more than justifies expectations. It is one of the most beautiful of the many beautiful works given to us by the Ray Society. Vol. I is issued to subscribers for 1927, and contains 14 plates, 11 of which are coloured, and many text figures. It embraces a general account of the true anemones, only the Actinaria being dealt with, as the Ceriantharia and the Zoanthinaria classed by Gosse with the ordinary sea anemones are excluded. The structure, coloration, development, bionomics,

and classification are fully discussed, but the detailed systematic part is still to be published. Thirty-nine species are listed, and it is very satisfactory to find fewer rather than more species than are usually recognised in the older works. For instance, *Peachia hastata* Gosse now embraces both *P. undata* and *P. triphylla* of that author, and there are many similar cases.

The treatment is original and based upon a thorough knowledge both of the technique required for the anatomical study of preserved specimens and of the living animals which have been ceaselessly watched in their marine haunts as well as in the laboratory aquaria. Thus we have offered to us a work both for the systematist and the naturalist by one who has sympathies with both sides. Important and extremely interesting is the author's belief in physiological characters for the determination of species. He finds the distribution of the various methods of reproduction to be characteristic. Thus apparently one species may be viviparous, others may divide by total fission, fragmentation, by tearing or by constriction, others again are characterised by the absence of any of these methods, but they are apparently usually alternative, and a given method of reproduction, in cases so far investigated, is a specific character.

In determining a species of anemone the necessity for studying collectively the various characters including the physiological differences is emphasised. As is truly stated anemones cannot be classified by taking isolated points into consideration. All points must be compared, and a species cannot be determined by merely looking at a coloured figure which only represents one aspect of a creature of extreme variability in all its attitudes. Dr. Stephenson's coloured figures have a peculiar charm and his title-page, head-pieces, and tail-pieces are delightful. We look forward to the publication of the second volume.

M. V. LEBOUR.

MISCELLANEOUS

Natural Man. A Record from Borneo. By CHARLES HOSE, Hon.Sc.D. (Cantab.), with a Preface by Prof. G. ELLIOT SMITH, F.R.S. [Pp. xvi + 284, 126 illustrations and a map.] (London: Macmillan & Co., Ltd., 1926. Price 30*s*. net.)

THE cause of science has been greatly aided in the past by Western Europeans who have held administrative appointments in remote parts of the earth. Dr. Charles Hose has been a notable contributor of that kind by investigating the fauna of Sarawak and supplying numerous museums with specimens from that region. His contributions to cultural anthropology are equally important. *The Pagan Tribes of Borneo*, written in collaboration with Dr. William McDougall and published in 1912, is recognised to be the most authoritative account of the ethnology of the island, and *Natural Man* is a record which will be highly valued by anthropologists. It is essentially a descriptive account of all aspects of the life of the native peoples of Borneo, as they exist to-day, or as they can be safely inferred to have existed before they were modified by Europeans. Many of the customs and beliefs described are doubtless of great antiquity and it is fortunate that they should have been so carefully studied before it is too late. The writer avoids theorising on the significance of the facts in their relation to wider problems, and his book is in that way more coherent than many anthropological surveys of its kind. The reader is never given the impression that a description is in any way coloured to favour a particular hypothesis. The known history and presumed origins of the tribes are briefly outlines, and there is an interesting account of the proceedings which resulted in James Brooke's investiture as Rajah and Governor of Sarawak. The tribal and village life, arts, crafts, customs, and beliefs of the natives are then described in turn. The diverse origins of the population are said to be evidenced by the physical hetero-

geneity found to-day, and the differences of customs and beliefs bear witness to the fact that several different higher civilisations have affected the inhabitants of Borneo at different times. The pagan—i.e. the non-Mohammedan—peoples are classified in six principal groups. Of these the Punans are the most primitive tribe, and they are, perhaps, the most interesting anthropologically. They are solely hunters and gatherers, possessing no houses or domestic animals and practising no agriculture. Though users of metal implements, they are unable to manufacture them. The Punans are said to be incredibly shy, peace-loving and intelligent. In the preface Prof. Elliot Smith suggests that the existence of these kindly savages establishes "the fact of fundamental importance that man is by nature peaceful and good-natured." But one is not compelled to accept that view. It seems quite as reasonable to suppose that the Punans have failed to advance beyond their present low cultural status because they are reserved and unaggressive. Quite a different temperament may be needed to enable a race to achieve a dominant position. In many ways this book will serve as an apology for the natives of Borneo. It will entirely dispel the popular idea that they are no more than head-hunting savages. The origins of their culture are still largely obscure, but it had in some ways reached a high level before there had been any contact with Europeans. The natives still excel in metal working and in numerous arts and crafts, but it is feared that their skill will be lost as more and more articles are imported from Europe. β_2.

Bolles Lee's Microtomist's Vade-Mecum. A handbook of the Methods of Microscopic Anatomy. Ninth Edition. Edited by J. Brontë Gatenby, D.Sc., and E. V. Cowdry, Ph.D., with collaboration of W. R. G. Atkins, O.B.E., Sc.D., F.R.S., the late Prof. Sir William Bayliss, J. Thornton Carter, F.Z.S., Robert Chambers, Ph.D., W. Cramer, D.Sc., the late Dr. C. Da Fano, Helen Pixell-Goodrich, D.Sc., J. G. Greenfield, M.D., R. Ludford, D.Sc., G. Payling Wright, B.M., B.Ch., F. W. Rogers Brambell, D.Sc. [Pp. x + 710, with 9 plates.] (London: J. and A. Churchill, 1928. Price 30s. net.)

The *Microtomist's Vade-Mecum* is well known to biologists as the standard reference-book in English on cytological, histological, and embryological technique. It attained this pre-eminence when it first made its appearance, and has retained it up to the present time. Other books on microscopical technique have appeared, but none have attempted to rival the *Vade-Mecum* as the standard compendium of all the more important microscopical methods.

The scientific world has recently borne the loss of Arthur Bolles Lee, the original author of this book. Although the book has changed much since he ceased to edit it, we feel very strongly that it owes a great deal to his genius. We think that future editions would be enhanced by the inclusion of a frontispiece photograph and a short biography of the author.

The present edition is extensively revised to bring it up to date with the most recent advances in technique. The cytological section includes the new modifications of the osmic acid method for impregnating the Golgi apparatus introduced by Ludford, Murray, and Nassonov. New sections are introduced dealing with histological applications of measurements of *p*H by Dr. W. R. G. Atkins and with micro-dissection by Dr. R. Chambers. The protozoological section has been completely rewritten by Dr. H. Pixell-Goodrich, the neurological section by Dr. J. G. Greenfield, and the tissue culture section by Drs. G. P. Wright and F. W. R. Brambell. These alterations and additions have greatly increased the usefulness of the book to the practical worker and perform a very important function in supplying a concise account of the modern methods in these rapidly growing fields.

Recent editions have tended to contain an increasing number of little-

used modifications of the more important methods. The inclusion of these was of little practical value and was inclined to confuse the inexperienced. We are glad to note that many of these have been removed from the present volume or have been incorporated in a simplified form. This is advantageous and might, we think, be carried still further.

We congratulate Prof. J. Bronté Gatenby and Dr. E. V. Cowdry on this edition and confidently recommend it to those interested in microscopical technique.

A. S. PARKES.

The Extra Pharmacopœia. By MARTINDALE and WESTCOTT. Vol. I. Nineteenth Edition. Revised by W. H. MARTINDALE. [Pp. xxxvi + 1207.] (London: H. K. Lewis & Co., Ltd., 1928. Price 27*s.* 6*d.* net.)

A BOOK such as this, which has survived nineteen editions, has obviously more than justified its existence and has indeed made itself an essential part of the equipment of medical men, pharmacists, and analysts; but this by no means completes the list of those who may be recommended to refer to the pages of this book for enlightenment. The information contained in this handy little volume is encyclopædic, and besides giving the most up-to-date accounts of the properties and therapeutics of all the well-established and also more recent drugs it provides an elucidation with regard to all the more important orders, regulations, and international conventions relating to matters of medicine, hygiene, etc. From the large amount of matter dealt with it is difficult to select for special comment, as each reader would have his own preference, but suffice it to say that, in addition to containing informative articles on official and non-official substances included under the heading of Materia Medica, digressions are occasionally made from therapeutics, as in the case of Gossypium, under which heading is included a short account of the manufacture of artificial silk; in addition, the book contains sections of considerable length devoted to vaccines and anti-toxins, organotherapy, as well as a very instructive therapeutic index of diseases and symptoms and a supplementary list of drugs which includes a number of rarely used plant materials together with their supposed medicinal action. Altogether a most useful book, which may often supply information where other books fail.

P. H.

The Alchemy of Light and Colour. By OLIVER L. REISER. [Pp. 86.] (London: Kegan Paul, Trench, Trübner & Co., 1928. Price 2*s.* 6*d.*)

PROF. REISER'S essay—it can hardly be called a book—deals with the phenomena of colour, and incidentally of light, from an alchemical or rather psychological standpoint. Colour is regarded as "filtered light," and is considered to be due to a "kind of resonance effect." We observe the colour complementary to the one absorbed. This "synchronisation" was at one time regarded as consisting of an attunement between molecular groupings and radiant energy. Now the tendency is to explain it in terms of electronic behaviour. The absorption of moving light-waves is considered to be due to intra-atomic vibration. "Reduction" of a dye to a colourless leuco-compound is urged in support of this, and elsewhere mention is made of "living" and "dead" hydrogen-atoms.

The author's main theme is the biochemical basis of colour, and he concludes by suggesting that his views "might be tested by applying a delicate electroscope to the region around the brain when part of the skull has been removed (*i.e.* in trepanning)—the only trouble is that under such conditions the patient is usually unconscious from the effects of an anæsthetic: that phosphorescence of the brain which we call consciousness is therefore absent, so that the emanations would also be absent. However, the ingenuity of the scientist in overcoming limitations is marvellous."

J. G. F. D.

Horology. The Science of Time Measurement, and the Construction of Clocks, Watches, and Chronometers. By J. Eric Haswell, F.B.H.I. [Pp. xvi + 267, with 19 plates and 106 figures.] (London: Chapman & Hall. Price 25s. net.)

Both the author and publisher are to be congratulated on the production of such a useful book; the former for such a clear expression of a variety of subjects, combined with many accurate drawings and plates; and the latter for their artistic presentation and pleasing type.

The book naturally opens with a chapter on the astronomical measurement of time, the complete revolution of the earth on its axis, with reference to the stars, being the only true Standard Clock. Brief details are given for determining the period of a complete revolution of the earth by means of the transit instrument, two illustrations of which are shown.

Thence follow early Egyptian methods of subdividing and recording this period, by Clepsydra, or Water Clocks (the earliest continuous motion clock) to the Stewart Electrical Clock, the latest.

The pendulum as an early time measurer is then described, and its subsequent application to ordinary clocks, and the development of its compensation for changes of temperature, for its different forms, as new metals for their construction, such as invar, became available; then as applied to different types of escapements, through ascending stages of accuracy to the highly detached escapement of Strasser, Reifler, and Shortt.

These, when enclosed in air-tight cases to maintain the air at a constant density (and pressure, too, if the temperature be constant), are measuring the length of the day with remarkable accuracy, the Shortt especially so, as it is relieved of the duty of unlocking the escapement and is run in a much higher vacuum; but it needs another clock to show its time.

The author gives clear details for the calculations of the length of pendulums and mentions the importance of their compensation, but omits examples for finding the lengths of the compensators for the pendulums of ordinary regulators, and for those in air-tight cases, which, of course, are not the same.

Train calculations, gearing, both epicycloidal and involute, all clock escapements, with the drawings, are all well done.

Electrical clocks, and their impulse dials, are brought up to date in the book.

The drawings of the various watch and chronometer escapements, and the chapter on the balance spring, are excellent.

A very good case is made out for the fixed escapement for watches, owing to a fuller knowledge of springing, as against the Tourbillon or revolving escapement, and proved very clearly by the observatory trials.

Chronographs, both single and split seconds, quarter and minute repeaters, are fully explained and illustrated.

Mainsprings, barrels, keyless, and stem-setting mechanism, and many details as to the new alloys used in horology, must all appeal to the student and enthusiastic collector.

Facing the closing chapter on the Chronometer is a fine plate of John Harrison's No. 4, which won the Government award, but whose name we find is omitted from the index of the book.

Late examples of chronometers by Nardin, Kullberg, and Mercer are shown.

A fine surveyor's recording chronograph, with combined chronometer having electrical contacts by Mercer, fitted with their latest ferro-nickel balance, and special balance spring of their own alloy, and manufacture, from which they have obtained such fine rates, completes a most informative and instructive book.

E. T. Cottingham.

Aerial Photography: A Comprehensive Survey of its Practice and Development. By CLARENCE WINCHESTER and F. L. WILLS, F.R.P.S. [Pp. xv + 236, with 176 illustrations.] (London: Chapman & Hall, 1928. Price 25*s*. net.)

THIS book contains a "Foreword" by Sir Alan J. Cobham, and introductory notes by Sir Peter Clutterbuck, Timber Adviser to the High Commission for India, and by Sir Felix J. C. Pole, General Manager of the Great Western Railway.

It is divided into four parts headed respectively: (1) Summary (one chapter); (2) Photographic Material (four chapters); (3) Apparatus (eight chapters); and (4) Applications of Aerial Photography. This last section contains eight chapters dealing respectively with stereoscopic photography; the application of aerial photography in industrial and municipal affairs; in education; in archæology; and in geography; aerial surveying and map-making; aerial photography abroad; and the future of aerial photography.

The names associated with the book are a sufficient guarantee that it contains much valuable information.

The authors state that the book is due to experience and research, and Sir Alan J. Cobham describes it as a gift to the coming generation of knowledge which has been acquired in this way.

The illustrations are excellent, and the production generally very much to be praised.

Aerial photography has, however, made enormous strides since the Great War, not only in respect of the range of its application, and the results achieved by its aid, but also in its technique. To attempt to cover the whole ground in one book is no small effort, and the result is that some of the technical portions (particularly perhaps with regard to aerial surveying) are rather too condensed to be easily understood.

Still they indicate sufficiently the problems to be solved, and the lines along which solutions have been advancing.

It seems a pity that, with so many interesting plates, there is no list of illustrations enabling them to be turned up quickly; and there are several passages which might have been improved by more careful revision. Thus on p. 37 we have a table of suitable exposures, of which the *maximum* is one-fiftieth second. On p. 71 we read that the *minimum* exposure should be one-fiftieth second, "the *least* which should be given." Other examples might easily be adduced of passages in which the wording could have been readily improved; but all these are, after all, small defects, and do not detract seriously from the value of the book.

The writers state that their aim has been "to present fairly and accurately the case for aerial photography."

Their book should not only lead to a more widespread interest in the subject, but also be of service to those who wish to enter this line of work.

M. T. M. ORMSBY.

"The Thirsty Earth": The Theory and Practice of Irrigation in all Countries. By E. H. CARRIER, M.Sc. [Pp. vii + 222.] (London: Christophers, 1928. Price 10*s*. 6*d*.)

THIS book aims at presenting a coherent account of irrigation, which should include the causes of such works, the history and scope of man's efforts and their prospects. Previously the subject was to be found treated in isolated aspects and for particular areas in a variety of publications, official and technical.

The author realised the importance of the climatic background, and so the opening section deals with this and indicates the methods and value of irrigation. The second part, which concerns the processes adopted by the

ancient empires, is of historical significance. The main portion of the book is devoted to the scientific distribution of water in the modern world. The clearness with which all the necessary facts are brought into focus calls for commendation, whilst the dispassionate and balanced criticism of the possible results of irrigation (p. 156) will be appreciated. The correlation of relevant climatic and geological features is another noteworthy feature.

May the reviewer suggest that the statements of the value of lucerne and the use of 1913 figures for ostrich farming (pp. 198 and 201) are liable to be interpreted more generously than the recent available details of that industry merit? The return for 1922 shows £393,919 actual value recorded for feathers exported, against £2,953,587 (£3 million quoted).

In subsequent editions the author will perhaps avail himself of such recent matters as the variations in the level of Lake Nyasa and the proposed flooding of the areas in the Sahara.

The illustrations are well chosen, whilst the triple indices and bibliography play their parts in making a book of excellent character for the general reader. A careful study of this work, with the aid of a good atlas, will be of high value to the geographer. J. ELING COLECLOUGH.

The Development of the Microscope. Edited by ALFRED N. DISNEY, M.A., B.Sc., [Pp. xi + 297, with 30 plates and 36 figures.] (London: The Royal Microscopical Society, 1928. Price 17*s*. 6*d*. net.)

THE influence of the microscope on the science of Medicine and of our knowledge of the causes of disease can hardly be overstated. The modern outlook upon the infective diseases is largely based upon discoveries made with the microscope, and within even the last few years the causes of malaria, of sleeping sickness, plague, and other similar diseases, have all been ascertained by its aid. It is therefore surely true to say that no other single instrument of any kind has contributed so greatly to the health and happiness of mankind.

In the improvement and development of this king of instruments from shortly after its first invention up to the time of Abbe and the achromatic and immersion objectives, practically all the important improvements both in the mechanical construction of the stand and in the optical construction of the lenses were made by English opticians. It is therefore a strange fact that scarcely any account of the development of the microscope, beyond that in Mayall's lectures to the Royal Society of Arts, is to be found in the English language. Yet there must be many users of the microscope who would wish to know something of its history. This volume, published by the Royal Microscopical Society, and edited by three of the Fellows of the Society, partly supplied this deficiency. It consists of two portions; the first part is a history of optics and the microscope down to the publication of Hevelius's *Machina Cœlestis* in 1673, together with an annotated bibliography; and the second part is a descriptive catalogue of 161 microscopes in the collection of the Society, and of a number of objectives and other accessories.

In the historical account, not only is an outline given of the growth of optical knowledge, but sufficiently full extracts from the books referred to are included (in English), together with the illustrations, to enable the reader to judge for himself the extent of the contribution of each. In many cases the original extracts are copied *in extenso* in the bibliography at the end of this part. These extracts add the more to the value of the book, as the books from which they are taken are in most cases extremely rare. The books are written frequently in Latin, as this was the language universally used by scientists in the Middle Ages, and it is therefore very acceptable to the ordinary reader to have the English translation given in the text.

After dealing with the early Greek writers the authors have given an

excellent account of the state of the knowledge of Optics at the time of the Arabian philosopher Alhazen, with many extracts from his *Opticæ Thesaurus*. The suggestion that magnifying glasses were known to the Ancients is very fully examined, and rejected, "The silence of Galen and of Pliny show that in the first and second centuries of our era dioptric lenses were unknown" (p. 62). The conflicting accounts of the invention of the microscope and the telescope are collected and examined in a very interesting chapter, extracts being given from Borellus, Descartes, Huygens, Galileo, Scheiner, and others, together with illustrations of early telescopes and microscopes that are to be found in museums. The authors quote the conclusions of Van Swinden and Dr. Moll that Lippershey made a telescope in 1608, and that Jansen may possibly have made a microscope in 1590.

In the Society's collection, catalogued in the later portion of this volume, there are several microscopes of historical importance. The Marshall microscope (A2) is unfortunately not in its original state; the body and its supports are Marshall's, but the base, the bottom of the pillar, the stage, and mirror are later additions. We are glad to see that the unique "Description and Use of Culpeper's new-invented set of double Microscopes . . ." has been reproduced (Plate 4), as this contains Culpeper's account of this important microscope which continued to be made both here and on the Continent for over a hundred years. Among other interesting instruments in the collection are the large Universal Martin (Plate 16), the silver Watkins (Plate 10), the Adams Universal (Plate 9), the Joblot (Plate 3), and the Amici and Cuthbert reflecting microscopes (A 100, A 102). The experimental lenses made by Joseph Jackson Lister, *c.* 1830–50, are also, of course, of great interest, as these lenses reflect his endeavours to improve the correction of the spherical aberrations of the achromatic lens.

The thirty plates are printed on art paper and give excellent reproductions of the instruments.

Altogether the book is well printed and is one which should be of great interest to all lovers of the microscope.

R. CLAY.

Poems. By RONALD ROSS. [Pp. 96.] (London: Elkin Mathews & Marrot. Price 3*s*. 6*d*. net.)

THE present writer has always found Sir Ronald Ross one of the most enjoyable of modern poets. Sir Ronald has in rather a rare degree the combination of sensuous and intellectual qualities; that is to say, in his case, of verbal music strongly rhythmical and of a moralising reflection which sometimes becomes sharp satire and at other times approaches dream fantasy.

The beautiful "In Exile," in a previous volume, showed his mastery of the brief stanza, but he is just as happily at home in the slower moving music of "Hesperus," which begins:

"Ah! whither dost thou float, sweet silent Star,
In yonder floods of evening's dying light?
Before the fanning wings of rising night,
Methinks thy silvery bark is driven far
To some lone isle or calmly haven'd shore,
Where the lorn eye of man can follow thee no more."

The feeling for rhythm which informs the sonorous diction of these poems is unfailing in the unrhymed verse, some of which in this volume should interest the technical specialist, but it is well seen in the blank verse, a notoriously difficult medium. One notices, too, in the few lines from the "Proem," which follow, a delicately just use of natural imagery for metaphor:

"Now while the golden orb still tops the trees
Glean every scattered ear, that when at eve
Our hard taskmaster come to claim his fee
We with no shame may render up our due,
Though all our upland acres grant but small
Return, and thistles many mingle there
With yellow fruit."

In the same poem is an example of this poet's gift for the memorable single line:

"Who now beholding yon far-westering Sun
And phantom Moon upon the filmy east. . . ."

The reviewer wishes he had more space to quote others.

Among the pieces in this remarkably diverse collection which are more interesting for the matter than the poetry, that is to say, the occasional verses, is the sonnet "Petition" of 1916, dedicated to Sir William Ramsay before his death. Many readers will be glad to find it, even though it is not at the poet's highest artistic level:

"Spirit who dwell'st on the high hills of Thought
Far in thine eyrie of an ice-cold north;
Sister of Eagles, whose wings bear them forth
To find—whose eyes can see what they have sought:
Dwell not for ever in reverie overwrought,
With stars and ice and visions of high worth,
Silent and still, for ever above the Earth,
On the frost-firèd Pinnacles of Thought:

"Descend, for the world sickens! Come, be swift!
Fall like a falchion through this dreadful gloom,
Here where we lie, and die, and throw God's gift
To murderous idols; save us from our doom:
Great Science, such for thou dar'st doubt the truth;
Sister of Stars and Mountains; for that makes thee Truth."

The unscientific reader cannot help regretting that the author of such prose romances and such books of poetry as Sir Ronald has given us should ever have been claimed by science; but that is, no doubt, a narrowly sectarian view!

R. L. MÉGROZ.

Geschichte der Alchemie. By KARL CHRISTOPH SCHMIEDER. Herausgegeben und eingeleitet von FRANZ STRUNZ. [Pp. x + 613.] (Munich: Otto Wilhelm Barth-Verlag G.m.b.H., 1927. Price, br., RM. 10, gb., RM. 12.)

KARL CHRISTOPH SCHMIEDER, who was Professor at Kassel, published his well-known *History of Alchemy* at Halle in 1832. While it is by no means a thoroughly reliable work, and contains many errors which may reasonably be described as ludicrous, it is, nevertheless, a book which the serious student of alchemy and the history of chemistry cannot afford to neglect. Schmieder accumulated a mass of information from many sources, some of which are not now available, and while the critical faculty was not one of his main characteristics, he certainly had a *flair* for the romantic and fascinating details in which the story of chemistry is so rich. Prof. Strunz has therefore performed a useful service in reprinting the work, which had become extremely rare, and which was, in fact, almost impossible to procure in this country. In his short Introduction, Strunz gives an interesting estimate of Schmieder's service to the history of science.

The book appears to be reproduced in facsimile from the original edition.

E. J. H.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Analytic Geometry. By R. L. Borger, Ph.D., Professor of Mathematics, Ohio University. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xii + 334.) Price 12*s.* 6*d.* net.

The Theory of Determinants, Matrices, and Invariants. By H. W. Turnbull, M.A., Regius Professor of Mathematics in the United College, University of St. Andrews. London and Glasgow: Blackie & Son, 1928. (Pp. xvi + 338.) Price 25*s.* net.

Theory and Application of Infinite Series. By Dr. Konrad Knopp, Professor of Mathematics at the University of Tübingen. Translated from the Second German Edition by Miss R. C. Young, L. ès Sc. London and Glasgow: Blackie & Son, 1928. (Pp. xi + 571.) Price 30*s.* net.

Leçons sur l'Intégration et la Recherche des Fonctions Primitives Professées au Collège de France. Par Henri Lebesgue. Deuxième Édition. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1928. (Pp. 342.) Price 60 fcs.

Les Espaces Abstraits et leur Théorie Considérée, comme Introduction à l'Analyse Générale. Par Maurice Fréchet. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1928. (Pp. 296.) Price 50 fcs.

Exercices d'Analyse. Par Gaston Julia. Rédigés par René Harmegnies et Roger Julia. Tome I. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1928. (Pp. 454.) Price 80 fcs.

The Collected Works of J. Willard Gibbs, Ph.D., LL.D., formerly Professor of Mathematical Physics in Yale University. In two volumes. London: Longmans, Green & Co., 1928. (Pp., Vol. I, x + 434; Vol. II, xviii + 284.) Price 15*s.* each, or 25*s.* the two.

The Elements of Astronomy. A Non-mathematical Textbook for Use as an Introduction to the Subject in Colleges, Universities, etc., and for the General Reader. By Edward Arthur Fath, Professor of Astronomy in Carleton College. Second Edition. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. x + 323.) Price 15*s.* net.

Eos, or the Wider Aspects of Cosmogony. By J. H. Jeans, D.Sc., LL.D., F.R.S., Secretary of the Royal Society and Research Associate of Mount Wilson Observatory. London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1928. (Pp. 88.) Price 2*s.* 6*d.* net.

Life and Work of Sir Norman Lockyer. By T. Mary Lockyer and Winifred L. Lockyer, with the assistance of Prof. H. Dingle and contributions by Dr. Charles E. St. John and others. London: Macmillan & Co., St. Martin's Street, 1928. (Pp. xii + 474.) Price 18*s.* net.

The Sun, the Stars, and the Universe. By W. M. Smart, M.A., D.Sc., F.R.A.S., John Couch Adams Astronomer and Chief Assistant in the University Observatory, Cambridge. London: Longmans, Green & Co., 1928. (Pp. xii + 291, with 20 plates.) Price 12*s.* 6*d.* net.

The Nature of the Physical World. By A. S. Eddington, M.A., LL.D., D.Sc., F.R.S., Plumian Professor of Astronomy in the University of Cambridge. Gifford Lectures, 1927. Cambridge: at the University Press, 1928. (Pp. xix + 361.) Price 12*s*. 6*d*. net.

An Outline of Physics. By Albert Edward Caswell, Ph.D., Professor of Physics, University of Oregon. New York: The Macmillan Company, 1928. (Pp. xiv + 773.) Price 18*s*. net.

The Physics of Crystals. By Abram F. Joffé, Ph.D., D.Sc., D.Eng., LL.D., Director Roentgen Institute; Director, Physical Technical Institute, Leningrad, Russia. Edited by Leonard B. Loeb, Associate Professor of Physics in the University of California. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xi + 198.) Price 15*s*. net.

Handbuch der Experimentalphysik. Band 13, 2 Teil. Herausgegeben von W. Wien und F. Harms. Physik der Glühelektroden, von Prof. Dr. W. Schottky und Dr. Ing. H. Rothe, mit 55 Abbildungen. Herstellung der Glühelektroden, von Dr. Rev. Techn. H. Simon, mit 36 Abbildungen. Technische Elektronenröhren und ihre Verwendung, von Dr. Ing. H. Rothe, mit 88 Abbildungen. Leipzig: Akademische Verlagsgesellschaft m.b.H., 1928. (Pp. 492.) Price, brosch. M. 44; geb. M. 46.

An Introduction to Crystal analysis. By Sir William Bragg, K.B.E., D.Sc., F.R.S. London: G. Bell & Sons, 1928. (Pp. vii + 168.) Price 12*s*. net.

Müller-Pouillets Lehrbuch der Physik. II Auflage. Fünfter Band, erste Hälfte. Physik der Erde. Herausgegeben von Alfred Wegener. Braunschweig: Friedr. Vieweg und Sohn, Akt.-Ges., 1928. (Pp. xviii + 840, with 341 figures and 7 plates.) Price, geheftet, R.M. 49; gebunden, R.M. 53.

Laboratory Physics. A Short Course. By H. W. Heckstall-Smith, M.A., Physics Master, Stowe School, Buckingham, and B. A. Fletcher, B.Sc., Physics Master, Gresham's School, Holt, Norfolk. Oxford: at the Clarendon Press, 1928. (Pp. vii + 224.) Price 4*s*. 6*d*. net.

The Discovery of the Rare Gases. By Morris W. Travers, D.Sc., F.R.S., Fellow and Honorary Professor in the University of Bristol, Fellow of University College, London. London: Edward Arnold & Co., 1928. (Pp. vii + 128.) Price 5*s*. net.

Matter, Electricity, Energy. The Principles of Modern Atomistics and Experimental Results of Atomic Investigation. By Walter Gerlach. Translated from the Second German Edition by Francis J. Fuchs, Ph.D. London: Chapman & Hall, 11 Henrietta Street, W.C.2. (Pp. xii + 427.) Price 30*s*. net.

Four Lectures on Wave Mechanics. Delivered at the Royal Institution, London, on March 5, 7, 12, and 14, 1928. By Dr. Erwin Schrödinger, Professor of Theoretical Physics in the University of Berlin. London and Glasgow: Blackie & Son, 1928. (Pp. viii + 53.) Price, 5*s*. net.

Modern Physics. By H. A. Wilson, M.Sc., M.A., D.Sc., F.R.S., Professor of Physics, Rice Institute, Houston, Texas, U.S.A., formerly Fellow of Trinity College, Cambridge. London and Glasgow: Blackie & Son, 1928. (Pp. xiv + 381.) Price 30*s*. net.

Selected Papers on Wave Mechanics. By Louis de Broglie, Académie des Sciences, Paris, and Léon Brillouin, Docteur ès Sciences. Authorised translation by Winifred M. Deans, B.Sc., B.A. London and Glasgow: Blackie & Son, 1928. (Pp. 151.) Price 15*s*. net.

Collected Papers on Wave Mechanics. By E. Schrödinger, Professor of Theoretical Physics at the University of Berlin. Translated from the Second German Edition. London and Glasgow: Blackie & Son, 1928. (Pp. xiii + 146.) Price 25*s*. net.

The Determination of Hydrogen Ions. An Elementary Treatise on Electrode, Indicator, and Supplementary Methods, with an Indexed Bibliography on Applications. By W. Mansfield Clark, M.A., Ph.D. Third Edition. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1928. (Pp. xvi + 717, with 100 figures.) Price 30*s*. net.

An Introduction to Advanced Heat. By Ivor B. Hart, O.B.E., Ph.D., B.Sc. London: G. Bell & Sons, 1928. (Pp. vii + 335.) Price 7*s*. net.

Anthropomorphism and Physics. By T. Percy Nunn, M.A., D.Sc., Litt.D. Annual Philosophical Lecture, Henriette Hertz Trust British Academy, 1926. From the Proceedings of the British Academy, Vol. XIII. London: Humphrey Milford, Amen House, E.C. (Pp. 35.) Price 2*s*. net.

Introduction to Modern Physics. By F. K. Richtmeyer, Professor of Physics at Cornell University. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xv + 596.) Price 25*s*. net.

An Introduction to Biophysics. By David Burns, M.A., D.Sc., Professor of Physiology in the University of Durham. With a Foreword by Prof. D. Noël Paton, M.D., LL.D., F.R.S. Second Edition. London: J. & A. Churchill, 40 Gloucester Place, Portman Square, 1929. (Pp. xix + 580, with 116 illustrations.) Price 25*s*. net.

Fixation of Atmospheric Nitrogen. By Frank A. Ernst, Fixed Nitrogen Research Laboratory, U.S. Dept. Agric. London: Chapman & Hall, 11 Henrietta Street, W.C.2. (Pp. ix + 154, with illustrations.) Price 12*s*. 6*d*.

Heat and Thermodynamics. By J. K. Roberts, Ph.D. London and Glasgow: Blackie & Son, 1928. (Pp. xvi + 454.) Price 30*s*. net.

Physics for Medical Students. By Sidney Russ, D.Sc., F.Inst.P., Physicist to the Middlesex Hospital. Edinburgh: E. & S. Livingstone, 17 Teviot Place, 1928. (Pp. vii + 230.) Price 10*s*. 6*d*. net.

The Technology of Low-temperature Carbonization. By Frank M. Gentry. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1928. (Pp. xvii + 399, with 80 figures.) Price 34*s*. net.

Principles and Applications of Electrochemistry. In two volumes. Vol. I, Principles. By H. Jermain Creighton, Associate Professor of Chemistry, Swarthmore College. Second Edition, Revised and Enlarged. New York: John Wiley & Sons; London: Chapman & Hall, 1928. (Pp. xvi + 488.) Price 20*s*. net.

Volumetric Analysis. A Practical Course based on Modern Theoretical Principles. By A. W. Wellings, B.Sc., Chemistry Master at Leamington College. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xi + 196.) Price 5*s*. net.

An Introduction to the Chemistry of Plant Products. Vol. I: On the Nature and Significance of the Commoner Organic Compounds of Plants. By Paul Haas, D.Sc., Ph.D., Reader in Plant Chemistry in the University of London, University College, and T. G. Hill, D.Sc., A.R.C.S., Reader in Vegetable Physiology in the University of London, University College. Fourth Edition. London: Longmans, Green & Co., 1928. (Pp. xvi + 530.) Price 18*s*. net.

Colloid Chemistry. By The Svedberg, Professor of Physical Chemistry, University of Upsala. Second Edition, Revised and Enlarged in Collaboration with Arne Tiselius, Research Assistant in Physical Chemistry, University of Upsala. American Chemical Society Monograph Series. New York: The Chemical Catalog Company, 419 Fourth Avenue, at 29th Street, 1928. (Pp. 302.) Price $5.50.

Symbols and Formulæ in Chemistry. An Historical Study. By R. M. Caven, D.Sc., F.I.C., Professor of Inorganic Chemistry in the Royal Technical College, Glasgow, and J. A. Cranston, D.Sc., A.I.C., Lecturer in Physical Chemistry in the Royal Technical College, Glasgow. London and Glasgow: Blackie & Son, 1928. (Pp. ix + 220.) Price 15*s*. net.

Calculations in Physical Chemistry. By J. R. Partington, D.Sc., Professor of Chemistry, East London College, University of London, and S. K. Tweedy, B.Sc. London and Glasgow: Blackie & Son, 1928. (Pp. viii + 152.) Price 7*s*. 6*d*. net.

Volumetric Analysis. By Dr. I. M. Kolthoff, Professor of Analytical Chemistry of the University of Minnesota. With the Collaboration of Dr. Ing. H. Menzel, Dresden, Germany. An Authorised Translation based upon the German Edition. By N. Howell Furman, Ph.D. Vol. I: The Theoretical Principles of Volumetric Analysis. New York: John Wiley & Sons; London: Chapman & Hall, 1928. (Pp. xvii + 289.) Price 15*s*. net.

The Viscosity of Liquids. By Emil Hatschek. Lecturer on Colloids at the Sir John Cass Technical Institute. London: G. Bell & Sons, 1928. (Pp. xii + 237.) Price 15*s*. net.

Practical Physiological Chemistry. By Sydney W. Cole, M.A., Trinity College, Cambridge. Eighth Edition. Cambridge: W. Heffer & Sons, 1928. (Pp. xii + 479, with 66 figures.) Price 16*s*. net.

The Fundamentals of Chemical Thermodynamics. Part I: Elementary Theory and Applications. By J. A. V. Butler, D.Sc., Lecturer in Chemistry in the University of Edinburgh. London: Macmillan & Co., St. Martin's Street, 1928. (Pp. xi + 207.) Price 6*s*. net.

Inorganic Quantitative Analysis. By Harold A. Fales, Ph.D., Associate Professor of Chemistry at Columbia University. London: G. Bell & Sons, 1928. (Pp. xii + 493.) Price 12*s*. 6*d*. net.

The Geology of Venezuela and Trinidad. By Ralph Alexander Liddle. Texas, Fort Worth: J. P. MacGowan. (Pp. xxxvi + 552, with 88 plates and 22 figures.) Price 33*s*. 6*d*. net.

The Vertebrate Fossils from the Glacial and Associated Post-Glacial Beds of Scotland in the Hunterian Museum, University of Glasgow, and their Evidence on the Classification of the Scottish Glacial Deposits. By J. W. Gregory, D.Sc., F.R.S., President of the Geological Society, and Ethel D. Currie, B.Sc., Ph.D., F.G.S. Monographs of the Geological Department of the Hunterian Museum, Glasgow University, No. II. Glasgow: Jackson, Wylie & Co., 1928. (Pp. 25, with 3 plates.) Price 7*s*. 6*d*. net.

Elements of Optical Mineralogy. An Introduction to Microscopic Petrography. By Alexander N. Winchell, Professor of Mineralogy and Petrology, University of Wisconsin. Third Edition, Revised and Enlarged. Part I: Principles and Methods. New York: John Wiley & Sons; London: Chapman & Hall, 1928. (Pp. viii + 238, with 260 figures.) Price 17*s*. 6*d*. net.

Stratigraphical Palæontology. A Manual for Students and Field Geologists. By E. Neaverson, D.Sc., F.G.S., Demonstrator and Lecturer in Palæontology, University of Liverpool. London: Macmillan & Co., St. Martin's Street, 1928. (Pp. xiii + 525, with 70 plates.) Price 18*s.* net.

Geological Maps. Their History and Development, with Special Reference to Wales. By F. J. North, D.Sc., F.G.S. Cardiff: Published by the National Museum of Wales and by the Press Board of the University of Wales, 1928. (Pp. vi + 133, with 11 plates.) Price 1*s.* net.

The Geology of Petroleum and Natural Gas. By Ernest Raymond Lilley, Sc.D., Associate Professor of Geology at New York University. London: Chapman & Hall, 11 Henrietta Street, W.C.2. (Pp. x + 524.) Price 30*s.* net.

Agricultural Geology. By Frederick V. Emerson, Ph.D. Revised by John E. Smith. New York: John Wiley & Sons; London: Chapman & Hall, 1928. (Pp. xvi + 377, with 271 figures.) Price 16*s.* net.

Principles of Plant Pathology. By Charles Elmer Owens, Associate Professor of Plant Pathology, Oregon State Agricultural College, Corvallis, Oregon. New York: John Wiley & Sons; London: Chapman & Hall, 1928. (Pp. xii + 625, with 222 figures.) Price 23*s.* 6*d.* net.

An Introduction to the Study of Plants. Being an Elementary Account of their Morphology, Physiology, and Ecology. By F. E. Fritsch, D.Sc., Ph.D., F.L.S., Professor of Botany in the University of London, East London College, and E. J. Salisbury, D.Sc., F.L.S., Reader in Plant Ecology, University of London, University College, and Fellow of University College. London: G. Bell & Sons, 1928. (Pp. x + 399, with 8 plates and 223 figures.) Price 7*s.* 6*d.* net.

Air, Water, and the Chemistry of Plant Life. Based on the Lectures given at the Wye Agricultural College by Sir John Russell. By Martin Alford Fayers, F.R.H.S., Head Master of the Mitcham Gorringe Park Central School. London: George Philip & Son, 32 Fleet Street, E.C.4; Liverpool: Philip, Son & Nephew, 20 Church Street, 1928. (Pp. vii + 86.) Price 1*s.* 9*d.* net.

Vertebrate Zoology. An Introduction to the Comparative Anatomy, Embryology, and Evolution of Chordate Animals. By G. R. de Beer, M.A., B.Sc., F.L.S., Fellow of Merton College, John Wilfred Jenkinson Memorial Lecturer in Comparative and Experimental Embryology in the University of Oxford. With an Introduction by Julian S. Huxley, M.A., Ullerian Professor of Physiology, Royal Institution. London: Sidgwick & Jackson, 1928. (Pp. xx + 505, with 185 illustrations.) Price 15*s.* net.

The Elasmobranch Fishes. By J. Frank Daniel, Professor of Zoology in the University of California. Second Edition. Berkeley, California: University of California Press, 1928. (Pp. xi + 332, with 270 figures.) Price 27*s.* 6*d.* net.

The Larval Stages of the Plymouth Brachyura. By Marie V. Lebour, D.Sc., F.Z.S., Naturalist at the Plymouth Laboratory. From the Proceedings of the Zoological Society of London, 1928. Part II. Published July 18, 1928. (Pp. 473–560, with 16 plates.)

Barnacles in Nature and in Myth. By Edward Henn-Allen, F.R.S. London: Oxford University Press, 1928. (Pp. xv + 180, with 53 figures.) Price 15*s.* net.

Observations on Human Heredity. By J. S. Manson, M.D. (Edin.), D.P.H. (Manch.). London: H. K. Lewis & Co., 1928. (Pp. ix + 84, with 19 figures and 8 plates.) Price 6*s.* net.

Life in Inland Waters. With Especial Reference to Animals. By Kathleen E. Carpenter. With an Introduction by Julian S. Huxley, M.A., Fullerian Professor of Physiology, Royal Institution. London: Sidgwick & Jackson, 1928. (Pp. xviii + 267, with 12 plates and 94 figures.) Price 12*s.* net.

A First Biology. By S. Mangham, M.A., Professor of Botany, University College, Southampton, and W. Rae Sherriffs, M.A., D.Sc., F.L.S., Professor of Zoology, University College, Southampton. London: Sidgwick & Jackson, 44 Museum Street, W.C.1. (Pp. ix + 184.) Price 2*s.* 6*d.* net.

The Biology of Spiders. By Theodore H. Savory, M.A. London: Sidgwick & Jackson, 1928. (Pp. xx + 376, with 121 figures.) Price 16*s.* net.

The Frog. An Introduction to Anatomy, Histology, and Embryology. By the late A. Milnes Marshall, M.D., D.Sc., M.A., F.R.S. Edited by H. G. Newth, M.Sc. Twelfth Edition. London: Macmillan & Co., St. Martin's Street, 1928. (Pp. x + 182, with 40 figures.) Price 6*s.* net.

The Seas. Our Knowledge of Life in the Sea and How it is Gained. By F. S. Russell, D.S.C., B.A., Assistant Naturalist to the Marine Biological Association, Plymouth, and C. M. Yonge, D.Sc., Ph.D., Leader of the Great Barrier Reef Expedition, 1928–9. London: Frederick Warne & Co., and New York. (Pp. xiii + 379, with 384 illustrations.) Price 12*s.* 6*d.* net.

Physiology and Biochemistry of Bacteria. Volume I. Growth Phases, Composition, and Biophysical Chemistry of Bacteria and their Environment; and Energetics. By R. E. Buchanan, Ph.D., Dean of the Bacteriologist of the Iowa Agricultural Experiment Station, Iowa State College, and Ellis I. Fulmer, Ph.D., Professor of Biophysical Chemistry, Department of Chemistry, Iowa State College. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1928. (Pp. xi + 516, with 74 figures.)

The Protomines and Histones. By the late Albrecht Kossel, Professor of Physiology in the University of Heidelberg. Translated from the original German Manuscript by William Veale Thorpe, M.A., Ph.D., Lecturer in Biochemistry in the University of Birmingham. London: Longmans, Green & Co., 1928. (Pp. xi + 107.) Price 9*s.* net.

Ultra-violet Radiation and Actinotherapy. By Eleanor H. Russell, M.D., B.S., and W. Kerr Russell, M.D., B.S. With Forewords by Sir Oliver Lodge, F.R.S., D.Sc., LL.D., and Sydney Walton, C.B.E., M.A., D.Litt. Third Edition. Edinburgh: E. & S. Livingstone, 16 Teviot Place, 1928. (Pp. 648, with 259 figures.) Price 21*s.* net.

The Harveian Oration. Delivered before the Royal College of Physicians of London on October 18, 1928. By Sir Humphrey Davy Rolleston, Bart., K.C.B., M.A., M.D., Hon.D.Sc., D.C.L., LL.D. Cambridge: at the University Press, 1928. (Pp. 149.) Price 3*s.* 6*d.* net.

Catalogue of Manuscripts in the Library of the Royal College of Surgeons of England. By Victor G. Plarr, M.A., Librarian to the College, 1928. (Pp. 76.)

The Pressure Pulses in the Cardiovascular System. By Carl J. Wiggers, M.A., Professor of Physiology in the School of Medicine of Western Reserve University, Cleveland, Ohio. London: Longmans, Green & Co., 1928. (Pp. xi + 200, with 48 figures.) Price 14*s.* net.

The ABC of Nerves. By D. F. Fraser-Harris, M.D., D.Sc., F.R.S.E. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1928. (Pp. xi + 223.) Price 4*s.* 6*d.* net.

Handbook of Physiology. By W. D. Halliburton, M.D., LL.D., F.R.C.P., F.R.S., and R. J. S. McDowall, M.B., D.Sc., F.R.C.P. Eighteenth Edition. London: John Murray, Albemarle Street, W.1. (Pp. xxii + 902, with 500 figures.) Price 18*s.* net.

Indian Therapeutics. For Medical Students and Practitioners. By Dr. D. V. Sandu Chembur. Bombay: D. K. Sandu Bros. (Pp. 146.) Price 1*s.* 6*d.* net.

The Genesis of Epidemics and the Natural History of Disease. An Introduction to the Science of Epidemiology, based upon the Study of Epidemics of Malaria, Influenza, and Plague. By Clifford Allchin Gill, M.R.C.S. (Eng.), L.R.C.P. (Lond.), D.P.H. (Eng.). London: Ballière, Tindall & Cox, 7 Henrietta Street, Covent Garden, W.C.2, 1928. (Pp. xxvi + 550, with 19 charts.) Price 21*s.* net.

The Great Engineers. By Ivor B. Hart, O.B.E., Ph.D., B.Sc. London: Methuen & Co., 36 Essex Street, W.C. (Pp. viii + 136, with 33 diagrams.) Price 3*s.* 6*d.* net.

Bells Thro' the Ages. The Founders' Craft and Ringers' Art. By J. R. Nicholls. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1928. (Pp. xi + 320, with 129 illustrations.) Price 21*s.* net.

An Introduction to the Theory of Eddy-current Heating. By C. R. Burch and N. Ryland Davis. With an Introduction by A. P. M. Fleming. London: Ernest Benn, Bouverie House, E.C.4, 1928. (Pp. 72.) Price 12*s.* 6*d.* net.

The Theory of Film Lubrication. By R. O. Boswall, B.Sc., M.Sc., Lecturer in Mechanical Engineering in the College of Technology and in the University, Manchester. London: Longmans, Green & Co., 1928. (Pp. xi + 279.) Price 12*s.* 6*d.* net.

Nitroglycerine and Nitroglycerine Explosives. By Phokion Naoúm, Ph.D., Director of Scientific Laboratories, Alfred Nobel Dynamite Company, Hamburg, Germany. Authorised English Translation, with Notes and Additions, by E. M. Symmes. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1928. (Pp. xi + 469, with 3 plates and 36 figures.) Price 31*s.* 6*d.* net.

Geography of North America. By George J. Miller and Almon E. Parkins. New York: John Wiley & Sons; London: Chapman & Hall, 1928. (Pp. xv + 605.) Price 22*s.* 6*d.* net.

Démocrite Doctrines Philosophiques et Réflexions Morales. Traduites et précédées d'une Introduction par Maurice Solovine. Paris: Libraire Félix Alcan, 108 Boulevard Saint-Germain, vi 1928. (Pp. xlvii + 167.) Price 15 fcs.

An Outline of Comparative Psychology. By C. J. Warden, Ph.D. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1928. (Pp. 147.) Price 2*s.* 6*d.* net.

Auguste Comte. Thinker and Lover. By Jane M. Style. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1928. (Pp. 208.) Price 5*s.* net.

The Great Law. Told Simply in Seven Visits. By Hamisch MacHuisdean. Vol. II. Third Visit. A few Applications of the Law and Some Theology. Glasgow: Fraser, Edward & Co., 141 Bath Street, 1928. (Pp. xi + 86, with 70 illustrations.) Price 5*s.* net.

Diogenes, or the Future of Leisure. By G. E. M. Joad. London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co. (Pp. 102.) Price 2*s.* 6*d.* net.

Cawthron Lecture. Volume III. The Cawthron Institute, Nelson, New Zealand. Four Lectures by Dr. J. P. Lotsy, Velp, Holland; Sir Ernest Rutherford, University of Cambridge; J. T. Ward, Hon. Director Wangarui Observatory; and Prof. A. J. Grant, University of Leeds. Nelson, New Zealand: R. W. Stiles & Co., Waimea Street, 1928. (Pp. 22.)

Passing through Germany, 1928–1929. Fifth Annual Publication. Berlin: Terramare Office, Wilhelmstrasse 23. (Pp. 255, with illustrations.)

Pioneers of Science. Seven Pictures of Struggle and Victory. By Amelia Defries. London: George Routledge & Sons, 68 Carter Lane, E.C., 1928. (Pp. 289.) Price 5*s.* net.

The Battle of Behaviorism. An Exposition and an Exposure. By John B. Watson and William McDougall. Psyche Miniatures General Series. London: Kegan Paul, Trench, Trübner & Co., Broadway House, Carter Lane, E.C., 1928. (Pp. 103.) Price 2*s.* 6*d.* net.

The Notation of Movement. Text, Drawings, and Diagrams. By Margaret Morris. With an Introduction by H. Levy, M.A., D.Sc., F.R.S.E., Professor of Mathematics, Imperial College of Science and Technology. Psyche Miniatures General Series. London: Kegan Paul, Trench, Trübner & Co., Broadway House, Carter Lane, E.C., 1928. (Pp. 103.) Price 2*s.* 6*d.* net.

Fortuna, or Chance and Design. By Norwood Young. London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1928. (Pp. 93.) Price 2*s.* 6*d.* net.

Empire Forestry Journal. Vol. VII, No. 1, 1928. London: The Empire Forestry Association, 22 Grosvenor Gardens. (Pp. 132, with 17 plates.) Price 7*s.* 6*d.* net.

An Economic and Financial Analysis of Fifteen East Anglian Farms, 1926–27. By R. McG. Carslaw, M.A., and W. H. Kirkpatrick, C.D.A. Cambridge: W. Jeffer & Sons, 1928. (Pp. 17.) Price 1*s.* net.

Natural History of Canterbury. Issued by the Philosophical Institute of Canterbury. A Series of Articles on the Early History of the Province and on the History of Scientific Investigation up till 1926, as well as on some results of this Investigation. R. Speight, Arnold Wall, and R. M. Laing, Honorary Editors. Christchurch: Simpson & Williams, 1927. (Pp. ix + 299, with 28 illustrations.) Price 17*s.* 6*d.* net.

Emotion and Delinquency. A Clinical Study of Five Hundred Criminals in the Making. By L. Grimberg, M.D. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1928. (Pp. ix + 147). Price 7*s.* 6*d.* net.

The Child in Primitive Society. By Nathan Miller, Ph.D., Assistant Professor in the Department of Sociology at the Carnegie Institute of Technology. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1928. (Pp. v + 307.) Price 12*s.* 6*d.* net.

Contributions to Analytical Psychology. By C. G. Jung. Translated by H. G. and Cary F. Baynes. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1928. (Pp. xi + 410.) Price 18*s.* net.

Enduring Passion. Further New Contributions to the Solution of Sex Difficulties, being the continuation of "Married Love." By Marie Carmichael Stopes. London: G. P. Putnam's Sons, 24 Bedford Street, Strand, W.C.2, 1928. (Pp. 214.) Price 6s. net.

The Works of Geber. Englished by Richard Russell, 1678. A New Edition, with Introduction by E. J. Holmyard, M.A., D.Litt. London and Toronto: J. M. Dent & Sons. (Pp. xl + 264.) Price 6s. net.

Autolycus, or the Future for Miscreant Youth. By R. G. Gordon, M.D., D.Sc., F.R.C.P. London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1928. (Pp. 94.) Price 2s. 6d. net.

The Bases of Modern Science. By J. W. N. Sullivan. London: Ernest Benn, Bouverie House, E.C.4, 1928. (Pp. x + 246.) Price 12s. 6d. net.

The Ordinall of Alchimy. By Thomas Norton of Bristoll. Being a Facsimile reproduction from Theatrum Chemicum Britannicum with annotations by Elias Ashmole. With Introduction by E. J. Holmyard, M.A., D.Litt., F.I.C. London: Edward Arnold & Co., 1928. (Pp. viii + 125.) Price 10s. 6d. net.

Problems of Instinct and Intelligence. By Major R. W. G. Hingston, M.C. London: Edward Arnold & Co., 1928. (Pp. vii + 296.) Price 10s. 6d. net.

The Corridors of Time. V. The Steppe and the Sown. By Harold Peake and Herbert John Fleure. Oxford: at the Clarendon Press, 1928. (Pp. 160, with 84 figures.) Price 5s. net.

Artificial Silk. By Ing. Dr. Franz Reinthaler. Enlarged and Revised Edition. Translated from the German by F. M. Rowe, D.Sc., F.I.C. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1928. (Pp. xii + 276.) Price 21s. net.

Les Routes Aériennes de l'Atlantique, Aperçu Météorologique. Par A. Baldit. Paris: Gauthier-Villars et Cie, 5 Quai des Grands-Augustins, 1928. (Pp. 122.) Price 28 fcs.

The Spectroscopy of the Extreme Ultra-violet. By Theodore Lyman, Ph.D., Hollis Professor of Natural Philosophy, Emeritus, in Harvard University. Second Edition. London: Longmans, Green & Co., 1928. (Pp. vii + 160.) Price 10s. 6d. net.

Printed in Great Britain by
Hazell, Watson & Viney, Ld., London and Aylesbury.

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By E. C. Titchmarsh, M.A., University College, London.

Dirichlet's Divisor Problem.—The problem which has become known as Dirichlet's divisor problem is one of the most mysterious of modern mathematics. Unlike some difficult problems, it is easy enough to state. We denote by $d(n)$ the number of divisors of n, so that $d(1) = 1$, $d(2) = 2$, $d(3) = 2$, $d(4) = 3$, etc. Let

$$D(x) = \sum_{n \leqq x} d(n),$$

so that $D(x)$ is a non-decreasing function of x. The problem is to determine the asymptotic behaviour of $D(x)$ as $x \to \infty$.

It is quite easy to find a first approximation to $D(x)$; in fact it is easily seen from the definition that $D(x)$ is the number of lattice-points (points with integer co-ordinates) in the first quadrant underneath the hyperbola $XY = x$. This number is approximately equal to the area between the axes and the hyperbola cut off by the lines $X = 1$, $Y = 1$; and this easily leads to the asymptotic formula

$$D(x) \backsim x \log x.$$

Actually it is not very difficult to carry the approximation a good deal further, and to prove that

$$D(x) = x \log x + (2\gamma - 1)x + O(x^{\frac{1}{2}}),$$

where γ is Euler's constant.

The real difficulty appears when we seek to discover the real order of magnitude of the difference

$$\Delta(x) = D(x) - x \log x - (2\gamma - 1)x,$$

and it is this problem which has so far baffled all attempts to solve it.

We have seen that $\Delta(x) = O(x^{\frac{1}{2}})$. What may be called

the first real discovery in the problem was made by Voronoï in the early years of the present century; he proved that

$$(1) \qquad \Delta(x) = O(x^{\frac{1}{3}} \log x).$$

Later it was shown by Hardy that the equation

$$(2) \qquad \Delta(x) = O(x^{a})$$

is certainly untrue if $a < \frac{1}{4}$, so that there is a definite lower limit to the accuracy with which we can approximate to D(x) by simple increasing functions. It has also been conjectured that this lower limit represents the real truth, *i.e.* that (2) *is* true if $a > \frac{1}{4}$. But this has never been proved, and until comparatively recently the gap between the exponents $\frac{1}{4}$ and $\frac{1}{3}$ remained.

It was in 1923 that a Dutch mathematician, van der Corput, showed that (2) is true for a value of a less than $\frac{1}{3}$, in fact for any value of a greater than $\frac{33}{100}$. Quite recently, in *Mathematische Annalen* 98 (1928), pp. 697–716, van der Corput has produced a new version of his method, by which he proves that

$$(3) \qquad \Delta(x) = O\left\{x^{\frac{27}{82}} (\log x)^{\frac{11}{41}}\right\}$$

This result is the furthest point which research has so far reached.

The improvement of (3) over (1) may seem to be slight, the fraction $\frac{27}{82}$, which is the important feature of (3), being much nearer to $\frac{1}{3}$ than $\frac{1}{4}$. The reader who has not referred to the original papers can have no idea of the difficulty of obtaining even such small improvements. Even (1) is not exactly easy to prove, though several fairly short proofs of it are now known. The method by which van der Corput obtained his first result has been described (by Littlewood and Walfisz, *Proc. Royal Soc.*, A 106 (1924), 478–88) as "probably the most formidable argument in the whole of pure mathematics." Anyone who knows the analytic theory of numbers at all will realise that this does mean something. As to the later paper, it seems to preserve the characteristic difficulties of the method. I do not claim to have mastered this or indeed any of van der Corput's works, and the reader had best be spared any attempt at a popular version of the proof.

There is another problem like Dirichlet's which has a similar history, viz. the problem of the number R(x) of lattice points in the circle of radius x. A first approximation is given by the area of the circle, and we obtain

$$R(x) = \pi x + O(x^{\frac{1}{2}}).$$

The error term here, which we denote by P(x), is just as

mysterious as the $\Delta(x)$ of the previous problem, and its history is substantially the same. Several comparatively straightforward proofs are known of the result that

$$P(x) = O(x^{\frac{1}{3}}),$$

and we know, on the other hand, that this exponent $\frac{1}{3}$ cannot be replaced by any number less than $\frac{1}{4}$. Beyond this progress is extremely difficult. Van der Corput established the existence of a number θ less than $\frac{1}{3}$ such that $P(x) = O(x^{\theta})$. It was in connection with this problem that Littlewood and Walfisz remarked on the difficulty of van der Corput's method, and gave a proof of the equation

$$P(x) = O\left(x^{\frac{37}{112} + \epsilon}\right),$$

where ϵ is arbitrarily small. They gave their method principally because "it will be found that the proof . . . is both reasonably short and not impossible to grasp in its entirety." Van der Corput's new method has now been used by Nieland *Mathematische Annalen*, **98**, 1928, 717–36, to show that

$$P(x) = O\left(x^{\frac{27}{82}}\right).$$

There is one curious thing about these problems. Taking the problem of $\Delta(x)$ for example, all known methods depend on obtaining an inequality for the modulus of a sum of the form

$$\sum_{n=n'}^{n''} e^{4\pi i\sqrt{(un)}}.$$

The behaviour of these sums is extremely complicated, and much of the difficulty of the proof lies in dealing with them. But van der Corput shows that, even if we could prove about these sums everything that can possibly be true, we should still only be able to obtain the result

$$\Delta(x) = O(x^{\frac{1}{4}} \log^{\frac{1}{2}} x),$$

and the exact order of $\Delta(x)$ would still be undetermined. This seems to suggest that some completely new method is required before the problem can be solved.

Problems left by Ramanujan.—In 1927 the Cambridge University Press published the collected papers of Ramanujan, the Indian mathematician who became a fellow of Trinity College, Cambridge and of the Royal Society. This book contains one of the most remarkable collections of curious formulæ that exists. Ramanujan had amazing insight into the formal relations between such things as hypergeometric series, integrals involving the Γ-function, and so forth. He

had, too, a love for the remarkable special case. He could often, apparently, obtain a general formula by some process of his own which he would have found it impossible to explain; then, noticing a particularly elegant special case, he would present the astonished reader with this. Some of his formulæ turn out to be fairly easy to prove, some extremely difficult. All have some point of interest which is well worth bringing to light.

Since the collected papers appeared it has been a favourite occupation for mathematicians to construct proofs of Ramanujan's improved formulæ, and eight papers by various authors have recently appeared in the London Mathematical Society's *Journal* on topics suggested by Ramanujan. I propose to devote the remainder of this article to an account of them.

The first paper, by C. T. Preece, *London Math. Soc. Journal* **3** (1928), 212–16, deals with the values of certain definite integrals. A typical formula is

$$\int_0^\infty \frac{dx}{(1+x^2)(1+r^2x^2)(1+r^4x^2)\dots} = \frac{\pi}{2(1+r+r^3+r^6+r^{10}+\dots)}$$

where 1, 3, 6, 10, . . . are sums of natural numbers. This is proved by obtaining a recurrence relation between the integrals I_n obtained from the above integral by terminating the product at the factor $1 + r^{2n}x^2$; we can then evaluate I_n and proceed to the limit. This formula is connected with the theory of elliptic functions. Another curious result, typical of much of Ramanujan's work on integrals, is that

$$\int_0^\infty \tan^{-1} \frac{2nz}{n^2+x^2-z^2} \frac{dz}{e^{2\pi z}-1}$$

can be found exactly (*i.e.* it is an elementary function of x) if $2n$ is an integer. This can be deduced from Binet's well-known formula for $\log \Gamma(z)$.

The next paper is by Prof. G. N. Watson, *ibid.*, pp. 216–225, and deals with the summation of series. Many of Ramanujan's formulæ can be proved fairly simply by means of complex integration and the theorem of residues, though this is not the way in which Ramanujan himself is supposed to have found them. His favourite method was the use of Fourier integrals and their related formulæ.

The first formula dealt with is

$$\frac{1}{1^3}\cdot\frac{1}{2}+\frac{1}{2^3}\cdot\frac{1}{2^2}+\dots = \frac{1}{6}(\log 2)^3 - \frac{\pi^2}{12}\log 2 + \frac{1}{1^3}+\frac{1}{3^3}+\frac{1}{5^3}+\dots$$

This is a special case of a functional equation satisfied by the function

$$\phi(x) = \sum_{n=1}^{\infty} \frac{x^n}{n^3},$$

viz.

$$\phi\left(\frac{x}{x-1}\right) + \phi(x) + \phi(1-x) - \phi(1) = \frac{\pi^2}{6} \log(1-x)$$
$$+ \frac{1}{6}\{\log(1-x)\}^2 \{\log(1-x) - 3\log x\}.$$

This can be proved by expressing $\phi(x)$ as an integral and submitting it to various transformations.

Another series summed by Ramanujan is

$$\frac{1^{13}}{e^{2\pi}-1} + \frac{2^{13}}{e^{4\pi}-1} + \frac{3^{13}}{e^{6\pi}-1} + \ldots = \frac{1}{24}.$$

This is connected with formulæ in the theory of elliptic functions. It is a special case of the formula

$$\text{(A)} \qquad \sum_{n=1}^{\infty} \frac{n^{4m+1}}{e^{2n\pi}-1} = \frac{B_{2m+1}}{8m+4},$$

where B_1 . . . are Bernoulli's numbers. Another series which no one but Ramanujan would have summed is

$$\frac{\coth \pi}{1^7} + \frac{\coth 2\pi}{2^7} + \frac{\coth 3\pi}{3^7} + \ldots = \frac{19\pi^7}{56700}.$$

It can be proved by integrating $\coth z\pi \cot z\pi \cdot z^{-7}$ round a suitable contour; it is not easy to imagine how Ramanujan could have obtained it, if he did not use contour integration. There are numerous other results of the same kind.

The next paper, by G. Szegö, *ibid.*, 225–32, deal with the following problems, which were set as questions by Ramanujan in the *Journal* of the Indian Mathematical Society.

1. Show that if n is a positive integer

$$\frac{1}{2}e^n = 1 + \frac{n}{1!} + \frac{n^2}{2!} + \ldots + \frac{n^n\theta}{n!},$$

where θ lies between $\frac{1}{2}$ and $\frac{1}{3}$.

2. If n is positive show that

$$\frac{1}{n} > \frac{1}{n+1} + \frac{1}{(n+2)^2} + \frac{3}{(n+3)^3} + \frac{4^2}{(n+4)^4} + \frac{5^3}{(n+5)^5} + \ldots$$

and find approximately the difference when n is great. Hence show that

$$\frac{1}{1001} + \frac{1}{1002^2} + \frac{3}{1003^3} + \frac{4^2}{1004^4} + \ldots$$

is less than $1/1000$ by approximately 10^{-440}.

3. If $\Phi(x) = e^{-x} + \frac{x}{1!}e^{-2x} + \frac{3x^2}{2!}e^{-3x} + \frac{4^2x^3}{3!}e^{-4x} + \ldots$

show that $\Phi(x) = 1$ when x lies between 0 and 1, and that $\Phi(x) \neq 1$ when $x > 1$. Find the limit of $\{\Phi(1+\epsilon) - \Phi(1)\}/\epsilon$ as $\epsilon \to 0$ through positive values.

Ramanujan gives a partial solution of (1), proving that $\theta(0) = \frac{1}{2}$, and that $\theta(n) \to \frac{1}{3}$ as $n \to \infty$. The other two problems have not yet been attacked. Szegö gives fairly simple proofs of all three results.

Ramanujan's intuitions were usually correct, but he was sometimes led astray by false analogies. In a note entitled "Two assertions made by Ramanujan," *ibid.*, pp. 232–37, Miss G. K. Stanley considers two formulæ which are only partially correct. If $B(x)$ is the number of numbers less than x which can be represented as the sum of two squares, Ramanujan says that

$$B(x) = C\int_1^x \frac{dy}{(\log y)^{\frac{1}{2}}} + O\left\{\frac{x}{(\log x)^r}\right\},$$

where C is a constant, for all values of r. This formula was no doubt constructed by analogy with the well-known formula for $\pi(x)$, the number of primes less than x. It gives a correct first approximation to $B(x)$, but the approximation is not nearly so close as Ramanujan supposed. Actually the result is false if $r > \frac{3}{2}$, and the true theorem can be written in the simple form

$$B(x) = C\frac{x}{(\log x)^{\frac{1}{2}}} + O\left\{\frac{x}{(\log x)^{\frac{3}{2}}}\right\}.$$

The second assertion of Ramanujan is about the coefficient $\tau(n)$ of x^n is the expansion of

$$x\{(1-x)(1-x^2)(1-x^3)\ldots\}^{24}.$$

Ramanujan dealt with various congruence properties of $\tau(n)$, and considered in particular the number $T(x)$ of numbers n not exceeding x for which $\tau(n)$ is not divisible by 5. The real asymptotic formula for $T(x)$ is

$$T(x) = C\frac{x}{(\log x)^{\frac{1}{4}}} + O\left\{\frac{x}{(\log x)^{\frac{5}{4}}}\right\}.$$

Ramanujan supposed that a better approximation could be obtained by replacing the first term by an integral, as in the formula for $B(x)$. Miss Stanley shows, however, that this is not so.

In the next note, "A Formula of Ramanujan," *ibid.*,

238–40, Prof. G. H. Hardy states that, since the formula (A) above was proved by Prof. Watson, he has discovered among Ramanujan's manuscripts a more general formula, viz.

$$\alpha^k\left\{\tfrac{1}{2}\zeta(1-2k)+\Sigma\frac{n^{2k-1}}{e^{2n\alpha}-1}\right\}=(-\beta)^k\left\{\tfrac{1}{2}\zeta(1-2k)+\Sigma\frac{n^{2k-1}}{e^{2n\beta}-1}\right\}.$$

Here k is an integer greater than 1, α and β are positive, and $\alpha\beta=\pi^2$. If k is odd and $\alpha=\beta$, both sides vanish and we obtain a result equivalent to (A). Two proofs are given, one depending on the inversion formula of Mellin, and the other on the formula of Poisson in the theory of Fourier integrals.

The next paper, by T. Estermann "On the divisor problem in a class of residues," *ibid.*, 247–50, is connected with the problems discussed in the first part of this article. Suppose that instead of considering the divisors of *all* numbers less than x, we only consider the divisors of numbers in a certain arithmetic progression, say the progression whose general term is $mv+c$, where v and c are given integers, and $m=1, 2, \ldots$ It was stated by Ramanujan, without any proof, that

$$\sum_{m\leq n} d(mv+c)=\alpha_c(v)n\,(\log n+2\gamma-1)+\beta_c(v)n+O\,(n^{\frac{1}{4}}\log n)$$

where the coefficients $\alpha_c(v)$, $\beta_c(v)$, depend on v and c only, and are defined as the coefficients of v^{-s} in certain Dirichlet series. The particular case $v=1$, $c=0$ is Dirichlet's divisor problem already considered. The object of Estermann's paper is to prove this formula as far as the first two terms are concerned, without attempting to obtain the best possible result for the error terms—in fact he only proves that it is $O(\sqrt{n})$. The proof is quite elementary. There is, of course, little doubt that, once the first two terms have been disposed of, we could prove about the error term in the general case precisely the same results as in the particular case $v=1$, $c=0$.

Another paper by C. T. Preece, *ibid.*, pp. 274–82, deals with some more transformations of series and integrals. The first formula proved is

$$\pi\left\{\frac{1}{2}-\frac{1}{\sqrt{1}+\sqrt{3}}+\frac{1}{\sqrt{3}+\sqrt{5}}-\ldots\right\}=\frac{1}{1\sqrt{1}}-\frac{1}{3\sqrt{3}}+\frac{1}{5\sqrt{5}}-\ldots$$

If we transform the left-hand side by writing $1/(\sqrt{1}+\sqrt{3})=\frac{1}{2}(\sqrt{3}-\sqrt{1})$, etc., the formula takes the form

$$\pi(1-\sqrt{3}+\sqrt{5}-\ldots)=1^{-3/2}-3^{-3/2}+5^{-3/2}-\ldots$$

and is now easily recognisable as a particular case of the func-

tional equation of the function $L(s) = 1^{-s} - 3^{-s} + 5^{-s} - \ldots$, viz. that if

$$\xi(s) = \left(\frac{\pi}{4}\right)^{-\frac{1}{2}(s+1)} \Gamma\{\tfrac{1}{2}(s+1)\} L(s)$$

then $\xi(s) = \xi(1-s)$.

A curious formula on the multiplication of power series is that

$$1 - \frac{x^2\,3!}{(1!\,2!)^3} + \frac{x^4\,6!}{(2!\,4!)^3} - \frac{x^6\,9!}{(3!\,6!)^3} + \ldots$$
$$= \left\{1 + \frac{x}{(1!)^3} + \frac{x^2}{(2!)^3} + -\right\}\left\{1 - \frac{x}{(1!)^3} + \frac{x^2}{(2!)^3} - \ldots\right\}.$$

This is proved by constructing differential equations satisfied by the various functions.

Proofs are also given of a number of curious relations between definite integrals, *e.g.*

$$\sqrt{\alpha}\int_0^\infty \frac{e^{-x^2}}{\cosh \alpha x}\,dx = \sqrt{\beta}\int_0^\infty \frac{e^{-x^2}}{\cosh \beta x}\,dx$$

if $\alpha\beta = \pi$, and

$$\frac{1}{\sqrt[4]{\alpha}}\left\{1 + 4\alpha\int_0^\infty \frac{xe^{-\alpha x^2}}{e^{2\pi x} - 1}\,dx\right\}$$

in equal to a similar expression in β, if $\alpha\beta = \pi^2$. These can all be proved by means of Fourier integrals.

The last paper by Prof. G. N. Watson, *ibid.*, pp. 282–89, is on "Approximate Integration and Summation of Series." Two typical results are the asymptotic formulæ

$$1^2 \log 1 + 2^2 \log 2 + \ldots + x^2 \log x$$
$$= \frac{1}{6}x(x+1)(2x+1)\log x - \frac{1}{9}x^3 + \frac{1}{4\pi^2}\left(\frac{1}{1^3} + \frac{1}{2^3} + \ldots\right) + \frac{x}{12} + \ldots$$

and

$$1 + \left(\frac{x}{1!}\right)^5 + \left(\frac{x^2}{2!}\right)^5 + \left(\frac{x^3}{3!}\right)^5 + - = \frac{\sqrt{5}}{4\pi^2}\,\frac{e^{5x}}{5x^2 - x + \theta}$$

where θ vanishes when $x \to \infty$.

The former is obtained by a straightforward application of Plana's theorem. The latter can be deduced from Barnes's asymptotic expansion of generalised hypergeometric functions. It is a particular case of the formula

$$\sum_{n=0}^{\infty}\left(\frac{x^n}{n!}\right)^\mu = \frac{e^{\mu x}}{(2\pi x)^{\frac{1}{2}(\mu-1)}\sqrt{\mu}}$$
$$\left\{1 - \frac{\mu^2 - 1}{24\mu x} + \frac{(\mu^2 - 1)(\mu^2 - 25)}{1152\,\mu^2 x^2} + O\left(\frac{1}{x^3}\right)\right\}^{-1}$$

There are two particular cases, $\mu = 1$ and $\mu = 5$, in which the third term in the bracket vanishes, and the first two terms give a better approximation than one would normally expect. If $\mu = 1$ the left-hand side is simply e^x, and the whole bracket is identically equal to 1. The really interesting case is therefore $\mu = 5$, and it is this that Ramanujan selected.

ASTRONOMY. By W. M. Smart, M.A., D.Sc., Observatory, Cambridge.

An important publication from the Royal Observatory, Greenwich, is entitled, "Observations made with the Cookson Floating Zenith Telescope in the years 1919–1927 for the determination of the Variation of Latitude and the Constant of Aberration." The instrument to which reference is made in the title is the Zenith photographic telescope designed by the late Mr. Bryan Cookson; it was presented to Cambridge University Observatory and in 1911 loaned to the Royal Observatory for a period of 7 years, which has been subsequently extended. The volume deals with the second period of 7 years. The use of levels as in the ordinary Zenith instrument is avoided by carrying the rotation axis on an iron ring which floats in a shallow annular trough of mercury. The instrument can thus be quickly and easily rotated through an angle of 180°. The range in the variation of latitude at Greenwich during the period under consideration is just a little over half a second of arc, corresponding to a change in the position of the earth's pole of about 50 feet; this result alone is sufficient to demonstrate the precision with which observations can be made with the instrument. The principal results for the period 1919–27 are, first, the observed minute changes in the latitude of Greenwich which are compared, in the volume, with the corresponding Japanese results obtained by Prof. Kimura and, second, the determination of the constant of aberration to be $20''{\cdot}447$ which differs only by 5 in the last place from the result obtained in the first period of 7 years. From the aberration constant and the values of the velocity of light, the equatorial radius of the earth and the sidereal year, the solar parallax is found to be $9''{\cdot}815$, which is about 0·1 per cent. higher than the best observed values for this important constant.

In *Bulletin of the Astronomical Institutes of the Netherlands* Dr. J. H. Oort contributes an interesting paper *Dynamics of the galactic system in the neighbourhood of the sun*. The work of Oort, Lindblad, Charlier and J. S. Plaskett has indicated—it would seem, conclusively—that the more striking peculiarities in the motions of the stars can be explained in terms of the simple hypothesis that the galactic system is in rapid rotation about an axis perpendicular to the plane of the Milky Way and situated at a considerable distance from the sun in the

direction of the centre of the system of globular clusters. Utilising radial velocities, Oort had found—and his results were later corroborated by Plaskett—that the position of the sun in the galactic system is markedly eccentric and that the direction of the centre is in the galactic plane and in galactic longitude 325°, which is almost identical with Shapley's result for the centre of the globular cluster system. The hypothesis of galactic rotation being granted, the stellar group in the vicinity of the sun will consequently be moving, with reference to the centre of the galaxy, at right angles to the direction just mentioned, towards galactic longitude 55°. In the present paper, Oort first considers the velocity of escape from the stellar system. If it is presumed that the galactic system as a whole contains few stars whose velocity exceeds the velocity of escape, then it is clear that the motions of stars exceeding a particular limit should not be directed towards a sector of the Milky Way around galactic longitude 55°. Previous considerations have suggested that the limiting velocity is about 65 kilometres per second, and Oort, in a diagram, shows that when the directions of the high-velocity stars are plotted in galactic longitudes there is a definite quadrant around 55° which is avoided completely. Further corroboration of the centre of the avoided region is obtained from a study of stars with large radial velocities for which no reliable space-motions can be calculated. The evidence of the connection between the motions of the high-velocity stars and the presumed rotation of the galaxy is striking. Oort goes on to discuss mathematically the steady state of a rotating galaxy and his theory gives a qualitative explanation of the general phenomenon of star-streaming in the galactic plane, the vertex of star-streaming being situated not far from the direction of the centre of the galactic system. The theory gives the ratio of the galactic axes of Schwarzschild's velocity-ellipsoid, which is somewhat larger than that derived from observations, but the results can be reconciled by increasing the distance of the galactic centre (previously derived from a study of the rotation) to a value which is close to that assigned by Shapley from his studies of the globular clusters.

In *Publications of the Dominion Astrophysical Observatory* (Victoria, B.C.), Vol 4, No. 4, J. A. Pierce and S. N. Hill give useful graphs for obtaining rapidly for any star the position angle and distance from the solar apex and also the galactic co-ordinates. The position of the apex adopted is R.A. 271°, Declination + 28°; this position is that derived by Campbell and Moore from the Lick radial velocities of 2119 stars and by R. E. Wilson from the corrected proper motions of the Boss stars.

F. E. Ross contributes an interesting paper on " Photographs of Venus," in *Contributions from Mount Wilson Observatory*, No. 363 (*Astrophysical Journal*, Vol. 68). The layman is generally greatly surprised that the modern astronomer who revels in thousands and millions of light-years has hitherto made comparatively little progress in his exploration of some of the heavenly bodies, figuratively speaking, on his doorstep. The study of Venus is a case in point. The rotational period is not known with anything approaching accuracy ; various estimates ranging from a few days to 225 days (the period of revolution around the sun) have been given at different times. The explanation of the failure, by visual observers, to determine a definite period for the rotation of the planet lies in the almost complete absence of surface markings. The axis of rotation is equally a matter of uncertainty. In this paper, Dr. Ross gives an account of a photographic programme carried out at Mount Wilson in the summer of 1927. The planet was photographed, by means of a special enlarging camera attached to the 60-inch or to the 100-inch telescope, through different colour screens. This was the procedure adopted by Mr. W. H. Wright in his well-known and striking work on Mars during the favourable oppositions of 1924 and 1926. It may be recalled that the ultra-violet photographs of Mars showed not a vestige of detail with the exception of the polar cap, while the red and infra-red photographs revealed the markings familiar to visual observers. Wright's interpretation is that the ultra-violet photographs refer to the upper strata of the Martian atmosphere while the infra-red photographs depict the surface markings. The photographs of Venus secured by Ross (a selection of these are beautifully reproduced) are exactly the reverse in character ; the long-wave exposures (the red and infra-red) show no details, while such markings as there are appear on the ultra-violet photographs ; photographs in blue light give rather weak details. Ross interprets the phenomena as follows ; the outer layer of the planet's atmosphere is composed of a thin stratum of cirrus clouds, while the inner atmosphere is supposed to be exceedingly dense and yellowish. The markings shown in the short-wavelength photographs represent atmospherical disturbances high up and visible either as dark areas or as regions of enhanced brilliance, depending on the character of the disturbance. The photographs show pretty consistently that the bright areas occur near the cusps, from which it is inferred that the poles are in the neighbourhood of the cusps. The dark areas are generally in the form of bands presumably parallel to the planet's equator. The author points out that the atmospherical changes are rapid, from which it may be reasonably deduced

that the rotation period must be very much shorter than the orbital period of 225 days. On the other hand, the spectroscopic evidence is against a rotation period of a day or so with some indication, however, that it is probably of the order of 20 days. Ross considers a compromise of 30 days not unreasonable in view of all the uncertainties in which the problem has been hitherto shrouded. The photographs enable the diameter of the planet to be measured; the linear diameter comes out to be 7,650 miles; the mean density, the planet's mass being derived by gravitational methods, is 0·92 times that of the Earth, a value rather higher than that determined from previous work. The paper includes a large number of references to the work of other observers, visual, spectroscopic and radiometric; this constitutes a valuable addition to the paper. It may be added that to the best of Ross's belief the first photographs of markings on Venus were taken in 1921 by Mr. A. Rordame *in full daylight* on ordinary commercial film through a yellow screen.

In *Harvard College Observatory Bulletin*, No. 862, Dr. H. Shapley withdraws a suggestion, made with evident conviction, to which reference was made in the previous number of *Science Progress*. Shapley had announced that microphotometer tracings of early type spectra showed unmistakably a shallow absorption band around λ 4180 which he attributed to cyanogen. As cyanogen could not possibly be a constituent of the atmospheres of these very hot stars, he attributed the origin of the band to in-falling meteoric and cometary bodies which are known to be rich in cyanogen. Shapley now says: "The attribution I now believe to be wrong and withdraw it. Although the band may receive in some stars a small contribution from meteoric cyanogen, as I suggested, the major part must be otherwise explained; but whatever the cause the observed irregularities in curves of background intensity are serious for accurate temperature work on early class stars and possibly disturbing also in studies of the contours of wide lines." The origin of the band has now been traced mainly to absorption in the 11-inch object glass of the Draper refractor, with possibly less important contributions from the glass objective prism.

Astronomische Nachrichten Bd. 234 (17), No. 5609, contains an interesting article by A. Markov, of Poulkovo Observatory, on "The nature of spiral and gaseous nebulæ." The experimental groundwork is the determination of the surface brightness of the nebulæ. The in-focus image of a nebula and the out-of-focus images of neighbouring stars are obtained simultaneously by a simple device. From the latter the relative brightness of the nebula is first obtained. Then by way of

the known absolute brightness of the solar surface, the sun's stellar magnitude, and the magnitudes of the comparison stars, the absolute brightness of the nebula's surface is deduced. This has been done by the author for 19 nebulæ, spiral and gaseous. A calculation based on the most up-to-date information concerning stellar density is then made to obtain an estimate of the surface brightness of our galaxy as seen from a point outside. The author's results are now indicated; the values of the absolute surface brightness of the spiral nebulæ are so great that they cannot be considered as clusters of cosmic dust, the visibility of which might be explained by the reflection of light from the galactic stars, a theory put forward some years ago by F. A. Lindemann, which was intended to explain the recessional velocities of many of these objects as the result of light pressure from the galaxy. The great nebula in Andromeda seems to stand out apart from its fellows and, according to Markov, cannot be regarded as a typical spiral; its surface brightness is over 12 times the average brightness of the spirals studied and is 76 times the surface brightness of the galaxy. This latter result, depending as it does on a calculation in which the data cannot be regarded as unassailable, is quoted for what it is worth. If it is somewhere near the truth, we shall have to admit that our galaxy is but a very average one after all; but as we are getting used to yet more numerous indications of our insignificance, this last blow need not evoke more than a passing regret. Dealing with the gaseous nebulæ, Markov finds that the photographic surface brightness is lower than the visual brightness and, photometrically, is of the same order as the luminescence of radio-active substances and of phosphorescence. Markov, however, concludes that the observational material so far obtained is still insufficient for the solution of the problem concerning the nature of the radiation of gaseous nebulæ.

PHYSICS. By L. F. Bates, B.Sc., Ph.D., F.Inst.P., University College, London.

Cosmic Rays.—We have often discussed in this section the important experiments of Millikan and his collaborators on the penetrating radiations known as cosmic rays. The origin of these rays forms the subject of another paper by Millikan and Cameron (*Phys. Rev.*, vol. 32, p. 533, 1928). So far, the experimental results indicate that the cosmic ray spectrum consists of two main sets of bands nearly three octaves apart. The set of longer wavelength is responsible for most of the ionisation in the atmosphere and has an absorption coefficient of 0·35 per metre of water. The set of shorter wavelength can be resolved into two bands with absorption coefficients

of 0·08 and 0·04 respectively, the ionisation due to the second band being about twice that of the first. Millikan and Cameron start with the idea that these bands of cosmic rays are due to some nuclear acts or transformations which result in the liberation of sharply defined quantities of energy. Applying the Einstein equation, $E = m \cdot c^2$, where E is the energy of the cosmic radiation quantum in ergs, m is the loss of mass in grams which occurs during a transformation and c is the velocity of light, they come to the conclusion that the transformations responsible for the emission of rays of the enormous penetrating power observed, correspond to the creation of atoms of helium and oxygen from hydrogen nuclei, and of atoms of silicon and iron from helium nuclei.

Let us see how these deductions are made. From Aston's mass spectrograph or packing fraction curve (*Proc. Roy. Soc.*, vol. 115, p. 487, 1927) the authors show that only very heavy and relatively rare atoms can be considered to disintegrate with the ejection of α-rays and evolution of energy. They calculate, with the aid of the Einstein equation, the energy set free by the α-ray disintegration of an atom of thorium, and find that within reasonable limits it is equal to the maximum energy of the emitted α-ray, a result which inspires confidence in the application of this equation to such cases. The chief point of interest, however, which emerges from the examination of radioactivity data is that no radiation of energy greater than that corresponding to the fall of an electron through a potential difference of about eight million volts can possibly be emitted by radioactive sources known to us. Now, the most penetrating cosmic rays possess energy equal to that acquired by an electron falling through a potential difference of two hundred and sixteen million volts. Again, further examination of Aston's curve shows that the cosmic rays cannot arise from the formation of heavier atoms by the addition one by one of protons to the nuclei of lighter atoms. Even in the most favourable case, namely, the formation of the nucleus of carbon by the addition of one proton to the nucleus of boron, the energy released would give rise to radiation outside the region of the cosmic rays. A similar conclusion is formed when the formation of a heavier nucleus by the addition of one helium nucleus to another lighter nucleus is considered. But does there exist radiation corresponding to the formation of atomic nuclei in the manner we have just considered? The answer is that the experiments of Millikan and Bowen, made with sounding balloons which were flown very high, showed that no strongly intense radiation of penetrating power intermediate between that of the hardest γ-rays and that of the observed cosmic rays entered the earth's

atmosphere. Such atom building acts must therefore be very infrequent.

Now, the outstanding fact of cosmic radiation is that most of the cosmic radiation in the upper atmosphere has an absorption coefficient equal to 0·3. From a formula given by Dirac (*Proc. Roy. Soc.*, vol. 111, p. 423, 1926) it is possible to calculate the absorption coefficient of radiation of frequency corresponding to the formation of a helium nucleus from four protons, and it is found to be 0·3. From a theoretical point of view the formation of helium from hydrogen is the most likely act of atom-building, and the intense long-wave cosmic radiation appears thus to be accounted for. The cosmic radiation of shorter wavelength has now to be explained. To do this, Millikan and Cameron consider that Bowen's discovery (*Astrophys. Journ.*, vol. 67, p. 1, 1928), that nebulium consists of oxygen and nitrogen, proves that these two gases are extremely plentiful in the heavens, and consequently radiation corresponding to the creation of oxygen and nitrogen (and also carbon) should exist. According to Dirac's formula the radiation arising from the energy released in the formation of oxygen or nitrogen by the sudden combination of the requisite number of protons, possesses an absorption coefficient of 0·08, and thus the occurrence of another band of cosmic rays is explained. To explain the remaining band of radiation the authors, from consideration of the relative abundance of the elements known to us, show that there are no elements between oxygen and iron which by sudden formation from protons would produce radiation of appropriate intensity except silicon, and its neighbours. From Dirac's formula the radiation accompanying the formation of a nucleus of silicon from protons possesses an absorption coefficient of about 0·41. The quantum of this radiation should possess energy corresponding to the fall of an electron through a potential difference of two hundred and sixteen million volts, and thus the band of radiation of highest penetrating power is explained. If the silicon nucleus were formed by the simultaneous union of seven helium nuclei, a radiation of penetrating power about equal to that of the radiation liberated by the formation of helium from hydrogen should be produced. The authors also calculate the energy liberated by the formation of iron, which is relatively abundant in the universe, and show that it would correspond to cosmic radiation of absorption coefficient 0·019, which cannot be detected as the experimental mass-absorption curve does not possess sufficient resolving power in this region. Assuming that cosmic radiation is due to the creation of helium, oxygen, silicon and iron at rates directly proportional to the relative masses in which they occur in nature, the authors construct a

theoretical curve which shows the ionisation due to cosmic radiation at any depth expressed in equivalent metres of water. Although this curve may not be of any great importance, it is to be noted that it fits the experimental points remarkably well, and it may therefore be taken as evidence that the four elements helium, oxygen, silicon and iron are all that are required to account for the cosmic ray curve, and also as evidence of the direct formation of iron from hydrogen. Of course, it is very difficult to understand how such a large number of protons and the necessary number of electrons can come together suddenly to form a complex nucleus. Firstly, there is the problem of chance, which is in a way quite familiar to chemists interested in the formation of complex molecules. Secondly, there is the question of the enormous amount of work required to bring protons to within the minute distances of each other favourable to atom-building. This work is so great that high temperatures, even of the hottest stars, cannot be expected to help the process much. The authors appear to find an escape from these difficulties by assuming that these building processes are rendered possible under the unknown conditions of low temperature and density of interstellar space, which may permit a kind of loose clustering of a number of protons which may precede the formation of a complex nucleus. It is interesting to remember that Eddington and Jeans, in order to account for the huge energy output of the stars, have suggested that the source of stellar heat lies in the annihilation of matter, *i.e.* an occasional proton suddenly transforms its entire mass into radiation. This type of radiation would be about four times as hard as the most penetrating cosmic radiation definitely recorded. The fact that it is not detected experimentally, however, does not trouble us, for it is clear that if the annihilation process occurs inside a star, then the energy of the radiation is dissipated as heat long before it reaches the exterior portions of the star. So we now have two processes to consider, atom-building in interstellar space, and atom-annihilation inside the stars. According to Millikan and Cameron we may imagine a kind of cycle as follows. The protons and electrons which exist in great abundance in interstellar regions condense into atoms under the peculiar conditions reigning in these regions. These atoms move under the influence of gravitational forces to the stars. Here under the peculiar conditions of huge temperatures and pressures, an occasional proton, perhaps forming a part of the nucleus of a heavy atom, transforms its entire mass into radiation to supply the heat and light emitted by the star. This cycle is clearly incomplete. If the atom-building processes have been going on for ages in the interstellar regions, why are there any

protons and electrons left there? This suggests that to complete the above cycle we must assume that somehow or other the protons and electrons are being replenished throughout space by some unknown process, possibly the condensation of light into matter (as Sir Oliver Lodge once suggested at a British Association Meeting). The evidence of the cosmic rays leads us to assume that this process only occurs under the conditions peculiar to interstellar space (where the protons formed would certainly not possess the kinetic energy corresponding to the temperature of the radiation), and hence, that under these large-scale processes the Second Law of Thermodynamics is violated. But, be it noted, this new hypothesis means that if the universe is to be treated as a closed system, then it will not take up at some future time a state of uniform temperature, but it is already in a steady state, and the " Warme-tod " or state of disappearance of all available energy is avoided.

So far we have only given an uncritical description of Millikan and Cameron's ideas, but it is well to pay attention to the solemn note of warning sounded by Sir Ernest Rutherford in his recent presidential address to the Royal Society (*Proc. Roy. Soc.*, vol. 122, p. 1, 1929). He points out that when we are dealing with the passage of ordinary X-rays through matter, they lose energy chiefly by the " photo-electric " process, *i.e.* by the transference of energy to electrons; but with highly penetrating radiations the energy may be almost completely lost by a scattering process, which we know as the Compton effect. The absorption coefficient for such radiation may be calculated from formulæ due to Compton and to Dirac, or from the more recent formula of Klein and Nishina (*Nature*, Sept. 15, 1928), which is derived from the relativistic form of wave mechanics developed by Dirac. The three formulæ give different results, and for radiations corresponding to 100 million volts the absorption coefficient calculated from Klein and Nishina's formula is about five times greater than that given by the Dirac formula. Experimental evidence from gamma-ray sources indicate that Klein and Nishina's formula is the most correct. Now, Millikan's results indicate that on the basis of this formula the most penetrating cosmic radiation should have an energy quantum of 940 million volts, which would correspond to the annihilation of a proton. This is very interesting, but Rutherford points out that it means the extrapolation over a range of energies of 1,000 million volts of a formula which at present we can only hope to check over a range of 3 million volts. Again, in the scattering of highly penetrating radiation may not the nuclear electrons take part, and may they not be liberated with sufficient energy to dis-

integrate atomic nuclei in their paths? Rutherford concludes by emphasising the need for much more experimental evidence, possibly from the extensive use of the Wilson chamber or the Geiger counter, before we are in a position thoroughly to test the correctness of the interesting speculations on the origin of cosmic rays.

The Raman Effect.—The Raman effect was discussed in this section for the first time in the last issue of *Science Progress*, and we naturally expect a large number of new contributions to our knowledge of the effect. Three interesting papers are to be found in the *Indian Journal of Physics* (vol. 3, p. 105, 1928). In the first of these papers S. Venkateswaran studies the effect in glycerine and glycerine-water mixtures. Glycerine is particularly important, because in addition to the modified lines and the unmodified lines of radiation scattered in its passage through the liquid, it exhibits a pronounced continuous spectrum. Venkateswaran shows that the differences in frequency between the modified lines and the exciting line to which they are due may be correlated with Pfund's measurements (*Astrophys. Journ.*, vol. 21, p. 19, 1906), on the surface reflection of glycerine, in which definite maximum values are found. The scattered light is strongly polarised, and it is found that the degree of polarisation is very nearly the same for the modified and unmodified portions of the scattered radiation. The continuous spectrum is also found to be pronouncedly polarised, the degree of polarisation being very nearly the same as that of the unmodified or classical scattered radiation. In all cases the polarisation is independent of the wavelength of the exciting line. Now, the continuous spectrum is observed in many liquids, even after the most careful purification, and in the case of glycerine it is definitely shown to be a characteristic of the pure liquid. In some unknown way, it appears to be connected with the viscosity of the glycerine, for it diminishes when the glycerine is diluted with water and when the temperature of the glycerine is raised. The second paper, by I. Ramakrishna Rao, deals with the Raman effect in crystalline ice and quartz, whilst the third paper, by L. A. Ramadas, deals with the effect in gases and vapours. In the case of ether the frequency differences between the modified and unmodified lines are found to be the same in ether liquid and ether vapour, within the limits of experimental error. This is an important observation because it indicates that the individual molecules alone are responsible for the effect in ether, and that the state of aggregation of the molecules has no bearing upon it. This is supported by the observation that the intensity of the modified radiation is roughly proportional to the density of the fluid, liquid or vapour; on the

other hand, the ratio of the intensity of the unmodified radiation scattered in the liquid to that scattered in the vapour is much less than the ratio of the densities of the liquid and vapour. We must notice that there is no clash here with Venkateswaran's experiments on glycerine, because in the latter experiments the continuous spectrum of scattered radiation is the spectrum whose intensity appears bound up with the viscosity of the glycerine.

Raman and Krishnan (*Proc. Roy. Soc.*, vol. 122, p. 23, 1929) also describe experiments on the polarisation of the scattered radiation in benzene, carbon tetrachloride and amyl alcohol. They find that all the unmodified lines are polarised practically to the same extent, but, whilst the modified lines of the same frequency shift which accompany different incident lines are polarised to the same extent, they are polarised to a different extent than the unmodified line. Modified lines corresponding to different frequency shifts are polarised to different extents, the intensity of the weaker component varying from almost zero to 40 or 50 per cent. of that of the stronger line. Lines of greater frequency than the incident lines are polarised to the same extent as those of corresponding lower frequency. Little is said about the polarisation of the continuous spectrum of scattered radiation. It is, however, suggested that the continuous spectrum may be due to the combination of the incident frequency with the rotational frequencies of the molecules, the dense medium opposing such motions and thus assisting the emission of a continuous spectrum instead of a line spectrum. To explain the incomplete polarisation which is observed, the authors suggest tentatively that there are three principal directions in the molecule along which a given energy transition may occur, the probability of a transition being different in the three directions. If these directions and probabilities are also different for different energy transitions, then the differences in intensity and degree of polarisation of the modified lines may be explained. On the basis of this explanation, the ratio of the intensity of the two principal polarised components of the scattered light can never exceed 50 per cent., which is in agreement with the experimental facts.

Do scattered X-rays exhibit the Raman effect? In this connection an interesting letter in *Nature* (Dec. 22, 1928), from Krishnan calls attention to the work of Bergen Davis and Mitchell (*Phys. Rev.*, vol. 32, p. 331, 1928). These experimenters have found that the molybdenum $K\alpha_1$ rays scattered from graphite contain three new lines with frequency differences or shifts corresponding to changes in energy level of an electron by 279, 57 and 34 volts respectively. The first and last may be due to the removal of a K and L_1 electron

respectively to an outer level in the carbon atom, and Krishnan suggests that 57 volts may be due to the similar removal of both the L_1 electrons at the same time.

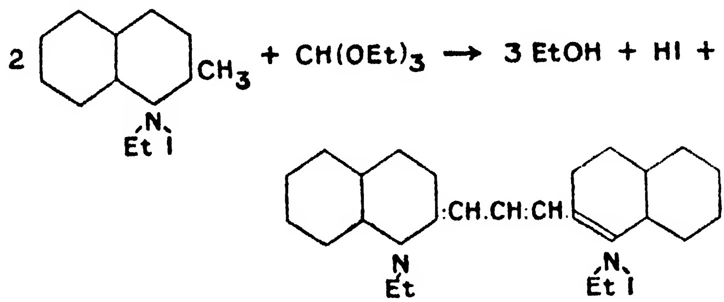
The Raman effect in aqueous solutions forms the subject of a paper by Carrelli, Pringsheim and Rosen (*Zeit. f. Phys.*, vol. 51, p. 511, 1928). They find in the case of solutions of HNO_3, $NaNO_3$, $NaNO_2$, NH_4NO_3 and NH_4OH, illuminated by light from a mercury arc, that typical Raman spectra are exhibited. All three solutions containing the NO_3 ion give lines with displacement frequencies of about 1040 cm.$^{-1}$, *i.e.* corresponding to an infra-red wavelength of about 9·6 μ. This agrees well with an absorption band found in solid $NaNO_3$ by Schaefer and Schubert (*Ann. der Phys.*, vol. 55, p. 577, 1918). Ammonia solution gives two frequency displacements, both a little over 3300 cm.$^{-1}$, both of which may be associated with the NH_4 ion and one with the OH ion. It is noteworthy that solutions of NaCl and HCl show no Raman lines. Hence it is suggested that the Raman lines found in solutions correspond to vibrations of atomic nuclei within molecular groups in a crystal, and not to vibrations of the crystal lattice as a whole.

The Charge on an Electron.—For many years the determination of the charge on an electron or the fundamental unit of electrical charge, by Millikan, has been regarded as one of the most brilliant pieces of accurate experimental work, and the value $4{\cdot}774 \times 10^{-10}$ *e.s.u.* has been universally accepted as correct to a very high degree of accuracy. It is therefore with something of a shock that we learn that this value is now challenged. In the first place, Eddington (*Proc. Roy. Soc.*, vol. 122, p. 358, 1929) draws attention to the occurrence of the number $hc/2\pi e^2$ in the equations of wave mechanics. The experimental value of this number from Millikan's data is 137·1. Eddington's paper is very difficult to summarise, and we can only indicate that he takes as the basis of his arguments the Exclusion Principle of Fermi and Dirac. This principle describes a mutual interaction of two electrons. Eddington combines it with the theory of relativity and finds that the numerical coefficient in some of the terms, in the expression, which he suggests represents the whole of the interaction, is equal to $2\pi e^2/hc$ and must be replaced by the numerical value $^1/_{136}$. Hence the theoretical value of $hc/2\pi e^2$ is 136, which differs appreciably from the experimental value. In his paper, Eddington also states that he has learnt that a recent determination of e by Siegbahn gives a value $4{\cdot}792 \times 10^{-10}$ *e.s.u.*, but he has not verified this information. The writer of this article has also heard this news, but he is not aware that the work in question has yet been published. Its publication will be awaited with considerable interest on both sides of the Atlantic.

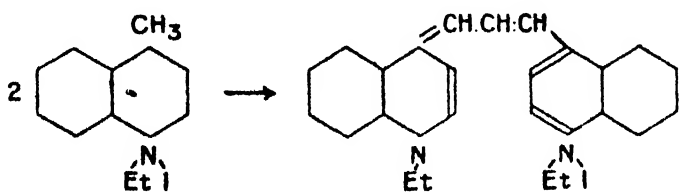
ORGANIC CHEMISTRY. By J. N. E. DAY, M.Sc., A.I.C., University College, London.

THE demand for dyes of the cyanine group, for use as sensitisers, to increase the range of the photographic plate, has led to a considerable amount of work being carried out in this field of research. A summary of the methods of preparation of these dyes is given by Hamer (*J.C.S.*, 1927, 2796).

Hamer has found that the yields obtained were greatly improved by the use of a basic solvent. As an example the preparation of 1 : 1′-diethyl-2 : 2′-carbocyanine iodide, from quinaldine ethiodide and ethyl orthoformate in pyridine solution, may be given :

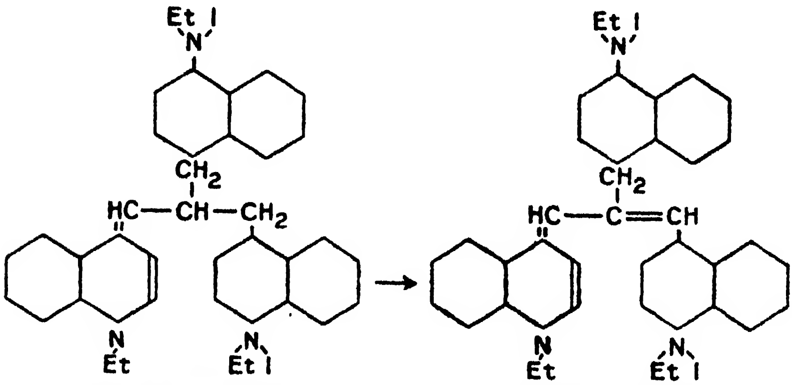


Similar dyes, having a 4 : 4′- and a 2 : 4′-linking, are known. An example of the former linkage is given by kryptocyanine, 1 : 1′-diethyl-4 : 4′-carbocyanine iodide, which may be prepared from lepidine ethiodide and ethyl orthoformate (Adams and Haller, *J.A.C.S.*, 1920, **42**, 2661 ; Mills and Braunholtz, *J.C.S.*, 1923, **123**, 2804 ; Hamer, *ibid.*, 1928, 1472).



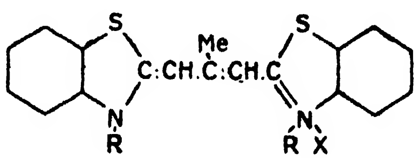
In this preparation a second dye, neocyanine, is also obtained in small quantities. Hamer (*loc. cit.*), by using lepidine metho-*p*-toluenesulphonate, and converting the resulting sulphonate into bromide, greatly increased the proportion of the neocyanine, a yield of 41 per cent. of neocyanine methobromide being obtained. Neocyanine ethiodide was obtained in a 26 per cent. yield. In the case of complex substances such as these, analytical results are not completely conclusive ; but they

indicate that three molecules of lepidine alkyl halide react with one of ethyl orthoformate. The structure of this trilepidylmethane derivative, however, is believed not to represent the constitution of neocyanine, which would be expected to have a similar structure to that of kryptocyanine. It is therefore probable that this derivative loses two hydrogen atoms to give the neocyanine. Instances in which two hydrogen atoms are lost, during such cyanine condensations, are already known.



(trilepidylmethane derivative) (neocyanine ethiodide)

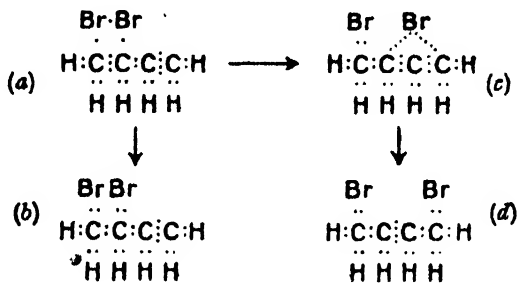
Other dyes with a substituent in the three carbon chain have now been prepared (Hamer, *ibid.*, 3160), by condensing 1-methylbenzthiazole alkyl halides with ethyl orthoacetate, of the general formula :



The addition of bromide to butadiene has been reinvestigated by Farmer, Lawrence and Thorpe (*J.C.S.*, 1928, 729). They found that when bromine was added to the hydrocarbon dissolved in chloroform, hexane, acetic acid, or carbon disulphide, cooled with a freezing mixture, a mixture of two dibromides was formed. The first, an oily liquid, was found to be the 1 : 2-compound, as it gave an ozonide which on decomposition gave $\alpha\beta$-dibromopropaldehyde and formaldehyde :

$$CH_2Br \cdot CHBr \cdot \overset{O_3}{CH\!-\!\!-\!\!-\!CH_2} \rightarrow H \cdot CHO + CH_2Br \cdot CHBr \cdot CHO.$$

The second was a solid, and had the 1 : 4-constitution. Both these dibromides, on heating to 100°, gave an equilibrium mixture of the two, containing 20 per cent. of the 1 : 2- and 80 per cent. of the 1 : 4-compounds. The percentage of the 1 : 4-compound obtained on bromination varied with the different solvents from 38·4 to 70. At room temperature, the 1 : 2-compound isomerised slowly, the change taking several days ; while at 100°, conversion took only a few minutes. Thus the 1 : 4-compound could not apparently be formed from the 1 : 2- during bromination under the conditions employed. It is suggested that an intermediate form may be represented by (*a*) which may either yield the 1 : 2-compound (*b*) or pass through (*c*) to the 1 : 4-compound (*d*).



Sah (*J.A.C.S.*, 1928, **50**, 516) has extended the method for the preparation of esters of ortho-acetic acid, and has prepared the following ortho-acetates : trimethyl, dimethylethyl, methyl-diethyl, ethyldi-*n*-propyl, ethyldi-*n*-butyl.

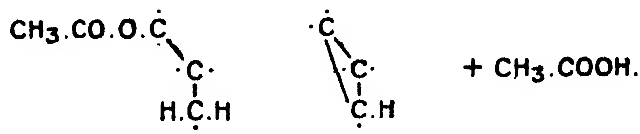
$$CH_3CN + ROH + HCl \rightarrow CH_3\underset{\displaystyle OR}{\underset{|}{C}}:NHHCl$$

$$CH_3\underset{\displaystyle OR}{\underset{|}{C}}:NHHCl + 2R'OH \rightarrow NH_4Cl + CH_3\underset{\displaystyle OR}{\underset{|}{C}}(OR')_2$$

Pseudaconitine.—Further work on this alkaloid has been carried out (Henry and Sharp, *J.C.S.*, 1928, 1105 ; Sharp, *ibid.*, 3094). These authors accept the formula $C_{36}H_{51}O_{12}N$ for this substance, the constitution of which, as far as is known at present, is represented by :

$$C_{19}H_{21}\begin{cases}(O\cdot CH_3)_4\\ CH\cdot OH\\ N\cdot CH_3\\ OH\\ O\cdot CO\cdot CH_3\\ O\cdot CO\cdot C_6H_3(O\cdot CH_3)_2\end{cases}$$

An account of some oxidation products is given. The structure of the nucleus, however, still resists elucidation. The way in which pseudaconitine, when heated above its melting point, loses acetic acid and forms pyropseudaconitine, has been investigated. The results indicate that it is probably formed from an acetyl group and a hydrogen from a (remote) carbon atom forming a bridged linkage :



Selenophen.—In view of the analogy between sulphur and selenium, the preparation of selenophen, CH : CH / CH : CH >Se, is of interest (Briscoe and Peel, *J.C.S.*, 1928, 1741,2514). It was obtained as a liquid b.p. 109·9°–110·1° at 752·1 mm., on treating selenium with acetylene at 400° and fractionating the distillate. Tetrabromoselenophen, C_4Br_4Se, and tetrachloroselenophen, C_4Cl_4Se, which are white solids, have also been prepared.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., Ph.D., University, Glasgow.

Sedimentation and Sedimentary Rocks.—Prof. R. M. Field has begun an extensive study of the marine carbonate sediments now in process of formation on the Great Bahama Bank (*Amer. Journ. Sci.*, xvi, 1928, pp. 239–46). He thereby hopes to throw light on Palæozoic stratigraphical problems, although he believes that modern coral reefs, with the exception of their lagoon facies, have little relation to Palæozoic conditions, and are peculiar to Post-Palæozoic or even Post-Mesozoic times. The " drewite," or fine calcareous sediment, which forms in the lagoons, and which has been attributed to the action of bacteria, is clay-like in texture, and is exceedingly plastic. As none of its material is of land origin drewite consists of pure calcium carbonate.

A very complete study of the marine chalks and marls of the Upper Tertiary (probably Pliocene) of the Moluccas, has been made by T. S. Hok (*Jaarb. Mijn. Nederl. Oost-Indie*, Verh. iii, 1926, 165 pp.). A review of this work appears in the *Geol. Mag.*, lxv, 1928, pp. 328–9.

Prof. S. H. Reynolds has contributed a useful discussion and classification of breccias with special reference to British examples (*Geol. Mag.*, lxv, 1928, pp. 87–107). He recognises

four classes : breccias of sedimentary origin ; breccias of igneous origin ; breccias due to movement ; and breccias of chemical origin.

A very comprehensive book on the origin of laterite has been published by H. Harrassowitz (*Laterit*. Berlin : Geb. Borntræger, 1926, 311 pp.). Not only is laterite dealt with but also such cognate materials as bauxite, kaolinite, and fuller's earth. The geology of these deposits has been kept in the forefront of the work. For a review see *Journ. Geol.*, xxxv, 1927, p. 748.

A similar study of the laterite-bauxite series of deposits with bauxite, in this case, as the principal material, has been made by Dr. C. S. Fox of the Geological Survey of India (*Bauxite*. London : Crosby Lockwood & Son, 1927, 312 pp.). He deals with the geology of bauxite and similar materials, in the origin of which he stresses the rôle of colloids. The residual material known as terra rossa is also discussed. For a review see *Geol. Mag.*, lxiv, 1927, pp. 381–2.

In a detailed experimental study of bauxite (*Min. Mag.*, xxi, 1928, pp. 407–30) Dr. T. V. M. Rao points out that laterite and bauxite are so intimately related that study of the one is impossible without reference to the other. As his work deals as much with laterite as with bauxite we may perhaps suggest that that fact should have been indicated in the title. This work has shown that bauxite is a definite mineral having a composition corresponding to a dihydrate of alumina. The process of lateritisation consists of the action of alkaline carbonates on the aluminium silicates of rocks, when hydrated aluminium carbonate is formed. This unstable compound breaks down into hydrated aluminium oxide, which is deposited in the form of bauxite. Together with the undecomposed residues of the original rock, this material forms laterites.

Prof. G. Hickling has made a study of the chemical relations of the principal varieties of coal (*Trans. Inst. Min. Eng.*, lxxii, pt. 5, 1927, pp. 261–81), with the object of ascertaining whether amongst the wide range of chemical composition shown by coals, there are any compositions which, by special frequency of occurrence, point to the development of products of a more stable type ; or whether, on the contrary, there are no such predominating types. His results show that the long range of normal coal types (excluding cannels, bogheads, etc.) has been developed by the progressive chemical alteration of essentially similar masses of plant materials. At the same time there have been considerable differences in the nature of those changes at different stages in the series. In the course of his work Prof. Hickling has developed very useful graphs illustrating the chemical composition of coal, which he suggests

could also be made to provide the best possible classification of coals.

Dr. L. L. Fermor's work on the relationship between the specific gravity and ash contents of the coals of Korea (Central Provinces) and Bokaro, and on coals as colloid systems (*Rec. Geol. Surv. India*, lx, pt. 2, 1928, pp. 313–57), shows that these quantities are connected in the Korea coals by the relationship

$$a = 100\,(g - k),$$

where a is the ash content, g the density, and k the density of pure ash-free Korea coal, taken as 1·29. Such a rule, if of general application, would obviously be of the first importance in prospecting, as the value of a coal could be obtained from its specific gravity as tested in the field. This rule was shown to be valid in assessing the values of the coals of the Bokaro field.

Three macroscopically homogeneous substances make up the majority of the coals: the fundamental colloidal substance (bright coal, or vitrain), greasy-lustred dull coal (durain), and a micaceous carbonaceous shale. Dr. Fermor believes that coals of the vitrain-durain series can be treated as suspensoid colloid systems, in which the vitrain acts as the dispersion medium, the ash contents as the disperse phase (the suspensoid), and the vegetable detritus of the durain as a second disperse phase (coarse suspension). Vitrain itself is an emulsoid, or gel.

Dr. W. H. Wong discusses the various classifications of coal which have been proposed on the basis of the available analyses of Chinese coals (*Bull. Geol. Surv. China*, No. 8, 1926, pp. 33–55), and has invented a nomenclature, or rather notation, to designate the various kinds and ranks of coal.

Prof. C. N. Kemp reviews the literature of the X-ray examination of coal sections (*Proc. Roy. Soc. Edinburgh*, xlviii, 1928, pp. 167–77), and describes the preparation of large, uniform, and comparatively thin slices of coal for radiographic examination, as this is the first step towards carrying out quantitative measurements and rendering valid comparisons possible. Sections of less than half an inch in thickness failed to show any advantages as regards detail over half-inch sections. Hence this thickness of section is utilised for general purposes. The paper is illustrated by a number of superb radiographs of coal.

Prof. J. E. Marr has studied the deposition of the later Silurian rocks of the English Lake District (*Geol. Mag.*, lxiv, 1927, pp. 494–500) as a continuation of his work on the condition of deposition of the Stockdale Shales (see Science Progress, Jan. 1927, p. 400). The sediments were deposited in a wide bay or gulf under relatively open-water conditions

in Valentian and Wenlock times, and under shallower conditions in Ludlow times. Actual coastal conditions, however, did not appear till towards the end of the Ludlow. Many of the changes in the characters of the sediments were due to variations in depth and distance from the coast-line, but climatic changes probably also played some part.

In his description of the Lower Carboniferous rocks of the Menaian region of Carnarvonshire (*Quart. Journ. Geol. Soc.*, lxxxiv, pt. 3, 1928, pp. 382–439), Dr. E. Greenly deals at length with the sedimentary petrology. The rocks consist of red and green breccias containing fragments of exclusively local derivation; "loams," a term implying pelitic accumulations which do not seem to have originated as muds; conglomerates, sandstones, shales, limestones, and cherts. The petrography of these rocks is described in considerable detail. Dr. Greenly discusses the oolitic structures found in the loams and breccia-matrix; kaolinite ooliths also occur in the latter rock. The oxidation, reduction, and migration of iron in the loam-breccia formation, and the dolomitisation of the limestones, are also discussed. In a later section of the paper he discusses the sources and origin of the loam-breccia formation. The breccias are regarded as local talus, or scree, while the loam, of distant provenance, represents a wind-transported dust derived from lateritised shales.

The lack of well-established seam correlations and recognisable fossil bands in the Coal Measures strata of Durham has led Dr. J. G. Kellett (*Proc. Univ. Durham Phil. Soc.*, vii, pt. 4, 1928, pp. 208–32) to investigate the mineral contents of many of the rocks with a view to the recognition of definite horizons. Unfortunately, however, his work merely serves to establish the fact that the same parent-rocks have contributed the same mineral suite to the Coal Measures rocks throughout the whole period. The existing differences in the heavy mineral crops are due to differences in the grade-size of the constituents, and to varying relative amounts especially of the authigenous minerals.

The Taveyannaz Sandstone, the geology and petrography of which is fully described by F. de Quervain (*Schweiz. Min. Petr. Mitth.*, viii, 1928, pp. 1–86) is a member of the Helvetian Tertiary series occurring from the Rhine Valley to Dauphiny. It consists of clastic sediments, fine-grained and coarse-grained sandstones interbedded with coarse conglomerates. It is of singularly uniform composition, consisting mainly of lava fragments of andesitic characters, with subordinate fragments of granite, aplite, quartzite, limestone, sandstone, and clay-slate. The Taveyannaz Sandstone is regarded as a true shore and shelf-zone marine sediment. The origin of the pre-

dominating lava fragments is still uncertain, although Niggli has pointed out their chemical relationships to the granites and diorite of the Late Alpine stocks.

Diamonds have now been found on the western coast of South Africa along a total distance of 500 miles. Their occurrence in the Namaqualand coastal region of Cape Colony has now been closely investigated by Dr. P. A. Wagner and Dr. H. Merensky (*Trans. Geol. Soc. S. Africa*, xxxi, 1928, pp. 1–41). The diamonds occur mainly in raised marine shingle beaches ranging in age from Miocene to Pliocene, and containing a peculiar extinct oyster (*Ostrea prismatica*) which has served as a guide to the diamonds. Some of the gems may have been derived from known and unknown diamond pipes in the interior of the country, but the constitution of the parcels is, on the whole, so different from those of the known diamond pipes, that Drs. Wagner and Merensky prefer to believe that the diamonds have been derived from sources lying beneath the Atlantic waters off the coast of Namaqualand.

The name *melikaria* has been proposed by F. A. Burt (*Journ. Geol.*, xxxvi, 1928, pp. 539–44) for small, septaria-like, siliceous, vein-complexes of honeycomb form, which are found in the Quaternary deposits of Texas. They are not eroded septarian vein skeletons, as they appear to be at first sight, but have developed directly in the rocks through the medium of ground waters containing silica in solution.

In an elaborate discussion of the styloliths and pressure cones (Druckzapfen) which are found in sedimentary, especially in calcareous, rocks, Dr. A. Kumm (*Geol. Rundsch.*, xix, 1928, pp. 448–64) shows that the supposed plant fossil from the Wealden shales, described by Geinitz under the name of *Guilielmites*, is identical with undoubted pressure cones from the same formation.

Two papers on the origin of pyrite in shales are presented by W. A. Tarr and H. E. Mathias (*Journ. Geol.*, xxxvi, 1928, pp. 434–9, 440–50). Prof. Tarr deals with pyritisation in local reducing areas within the Pennsylvanian shales of Missouri. The replacement is believed to have been syngenetic, and to have taken place under conditions similar to those now found in limans. Mr. Mathias has studied the pyritic concretions of the Pennsylvanian shales of North-Eastern Missouri. They are also syngenetic, and have been precipitated in the same way as pyrites is at present being deposited on the sea floor in many localities.

Dr. S. Tomkeieff discusses the origin of kaolinite-bearing nodules which occur in a fine-grained Coal Measures shale at Cow Gate, Newcastle-on-Tyne (*Proc. Geol. Assoc.*, xxxviii, 1927, pp. 518–47). The kaolinite fills up septarian fissures in the

nodules. As the nodules occur beneath a coal, an exogenetic origin for the kaolinite by the action of descending moor waters seems to be most feasible. Dr. Tomkeieff also discusses the origin of the shales and nodules, and the chemistry of kaolinite.

In discussing desert mud cracks Dr. C. R. Longwell (*Amer. Journ. Sci.*, XF, 1928, pp. 136–45) mentions three common kinds : " mud-curls," playa cracks, and mud cracks developed in argillaceous deposits on well-drained slopes. The last-named do not appear to be dealt with in the literature, and are mostly found in calcareous soils.

Metamorphism and Metamorphic Rocks.—Dr. C. E. Tilley has dealt with vesuvianite and grossular as products of regional metamorphism (*Geol. Mag.*, lxiv, 1927, pp. 372–5), in which environment these minerals are much less common than in that of thermal metamorphism. The Loch Tay Limestone of the Perthshire Highlands provides excellent material in which this association can be studied under conditions of regional metamorphism. It is shown that these limestones occur in the almandine zone of metamorphism.

The Shetland serpentines described by Dr. F. C. Phillips (*Quart. Journ. Geol. Soc.*, lxxxiii, 1927, pp. 622–52) occur mainly in the northernmost islands of Unst and Fetlar. The parent rocks are dunites, peridotites, pyroxenites, and gabbros. Autometamorphism in this series has produced the serpentinisation of the ultrabasic rocks, amphibolisation of the pyroxenes, and saussuritisation of the basic felspars. Contemporaneous dynamic action was a contributing factor in these changes. Subsequent dynamic metamorphism has given rise to talc- and anthophyllite-schists. Some chlorite-magnetite-schists are ascribed to the alteration of adjacent sediments concurrently with the introduction of magnesia from the igneous rocks.

In a paper entitled, " Petrographic Notes on Three Rock-types from the Shetland Islands," Dr. F. C. Phillips (*Geol. Mag.*, lxv, 1928, pp. 500–7) describes an epidote- and allanite-bearing granite, chloritoid-garnet-andalusite-schists, and a secondary serpentine with kæmmererite. The andalusite of the second-named of these rock-types developed at high temperature by granitic intrusion, and by later stress action it was broken into fragments. At this stage the chloritoid was formed.

In a paper on the nature and origin of the amphibole asbestos of South Africa, Dr. M. A. Peacock (*Amer. Mineralogist*, 13, 1928, pp. 241–86) shows that the dominant variety of asbestos is blue crocidolite occurring in thin but extensive cross-fibre seams conforming strictly to the bedding of the ironstones in which the mineral appears. The long-fibred

ash-grey asbestos, amosite, which is typically developed in ironstones within the contact aureole of the Bushveld intrusion, proved to be an orthorhombic amphibole with ferrous oxide as the dominant base. The banded ironstones are regarded as having originated as chemical precipitates of marine deposition. All the constituents of crocidolite, except the soda, are present in adequate proportions in the ironstones. Crocidolitisation is conceived as a mild, static, non-additive metamorphic process taking place within layers in the ironstone that are supposed to have been initially rich in soda.

The grünerite-bearing rocks of the Lake Superior region, described by S. Richarz (*Journ. Geol.*, xxxv, 1927, pp. 690–707), have been developed by the thermal metamorphism of sideritic and greenalitic cherts. In the metamorphosed sideritic cherts magnetite, garnet, and either quartz or calcite occur with the grünerite; but in the greenalitic varieties the mineral association is grünerite, fayalite, and magnetite.

In Dr. C. A. Matley's paper on the Pre-Cambrian Complex and associated rocks of South-Western Lleyn (Carnarvonshire) (*Quart. Journ. Geol. Soc.*, lxxxiv, 1928, pp. 440–504) the chapter on the petrology of the metamorphic rocks has been written by Dr. E. Greenly. The gneissic formation on the mainland includes acid and basic components, both of which are essentially similar to the corresponding gneisses of Anglesey. Numerous other rocks of the metamorphic complex are described, including grits, quartzites, green schists, jaspers, limestones, and a spilitic suite including spilitic lavas and tuffs, albitised dolerites, and basic schists due to the deformation of these rocks. The anamorphism of the complex is usually of a low grade, but some of the rocks of the lower part of the bedded succession are found in the higher anamorphic condition known in Anglesey as the Penmynydd zone of metamorphism.

Dr. F. C. Phillips has shown that the thermal metamorphism of the Upper Devonian rocks of North Cornwall, due to the Bodmin Moor granite and traceable by spotting, is superposed on an earlier dynamic metamorphism which has given rise to phyllites containing white mica, chlorite, and chloritoid (*Geol. Mag.*, lxv, 1928, pp. 541–56). Analogous changes in the spilitic lava horizons have given rise to schists of the general type of prasinite.

A bed of nodular shale 75 feet below the base of the Whin Sill, formerly described as a "spotted" metamorphic rock, is now shown by L. R. Wager to be a metamorphosed nodular shale, in which the nodules are similar to those in the underlying Basal Carboniferous Conglomerate (*Geol. Mag.*, lxv, 1928, pp. 88–91). The nodules were washed into, or formed in, the shale when it was being deposited, and the whole rock was

later altered by the Whin Sill. It is, however, unusual to find igneous rocks of Whin Sill type exerting a metamorphic influence through 75 feet of intervening strata.

G. Andrew has published two short papers on Donegal rocks, the first being a note on a basic intrusion into the Dalradian formation, which is regarded as one of the prototectonic sills connected with the Caledonian igneous and orogenic activity (*Manchester Lit. Phil. Soc., Mem. & Proc.*, **72**, 1928, pp. 205–19). This rock is a basalt or dolerite in which the original augites and ophitic texture have been preserved. The distribution of sills of this type may indicate the limits of the structural unit in N.W. Ireland which is characterised by this very low grade of metamorphism.

The second paper deals with the contact phenomena of the Donegal Granite. Intimate lit-par-lit injection of the adjacent schists and the magmatic incorporation of schists with the formation of oligoclase-gneisses, is recorded. The phenomena accord well with those that have been described around the correlated Scottish and Scandinavian granites.

An interesting note by Dr. H. H. Read on the " ptygmatic folding " of veins in the Sutherland granite complex (*Summ. Prog. Geol. Surv. of Gt. Brit.* for 1927, Part II, 1928, pp. 72–7) suggests that, unlike the ptygmatic veins first described by Sederholm, their tortuous character is original; they were never plane. Furthermore, they are true igneous injections and are not connected with any form of metamorphism. Their tortuous form is believed to result from the resistance to plane fissuring in the country-rock, a resistance arising from the mineral and structural habit of the rock.

In a paper on the geology and petrology of the granitic pegmatites intruding the Pre-Cambrian basement of the southernmost part of Norway, Dr. T. Barth (*Neues Jahrb. f. Min., Beil.-Bd.*, lviii, Abt. A, 1928, pp. 385–432) shows that the pegmatite dykes always occur as intrusives in the more basic rocks, such as the amphibolites of Kristiansand and Iveland, and the anorthosites of Flekkefjord and Farsund.

In a second paper under the same general title, " Zur Genese der Pegmatite im Urgebirge," Dr. Barth describes the syntectic rock-complex of Baneheia near Kristiansand (*Chem. d. Erde*, **4**, 1928, pp. 95–136). Amphibolite has been transformed to augen-gneiss by the injection of a granite-pegmatite magma, which had gaseous and liquid phases. The process of alteration took place under high pressure, but at a temperature not exceeding 500° C. It is described as anatexis, or " palingenesis," involving re-solution but not re-melting of the rocks under a deep-seated contact metamorphism. The invading pegmatite magma is regarded as the gaseous-liquid

fraction of the "regional granitic shell" of the earth, a material for which Sederholm's term *ichor* is suitable.

Dr. A. Laitakari describes the palingenesis or re-melting of the Tarkki Granite (Finland), and its re-injection, at local contacts with diabase dykes (*Fennia*, 50, No. 35, 1928, 25 pp.).

An intensive study of the geology and petrography of the Flüela group in Gräubunden, Swiss Alps, has been carried out by A. Streckeisen (*Schweiz. Min. Petr. Mitth.*, viii, 1928, pp. 87–239). The greater part of the region dealt with is covered by the Silvretta nappe, which is a fragment of a basal complex folded in Hercynian times. This sheet consists of parallel zones of paragneisses and amphibolites, which are concordantly intruded by the two granites of Flüela and the Mönchalp. The paragneisses, besides the usual quartz and felspar, contain garnet, staurolite, kyanite, sillimanite, andalusite, and tourmaline. The six first-named minerals are due to the Hercynian stress phase, the andalusite and tourmaline to the succeeding granitic intrusion phase. The gneissification of the granites represents the final act of the Hercynian folding. The Alpine folding produced only minor dislocation-metamorphism in the complex, including, however, strong mylonitisation along certain zones. The petrography of the metamorphic and igneous rocks is done in great detail with the aid of 10 new analyses.

ZOOLOGY. By F. W. Rogers Brambell, Ph.D., D.Sc., King's College, London.

Yolk-absorption in Mollusca.—The Cephalopoda among the Mollusca are unique in the large amount of yolk which is stored in the egg and which profoundly affects the development of the embryo. This huge mass of yolk presents some interesting problems concerning the manner of its absorption and assimilation by the growing embryo. Portmann and Bidder (*Q.J.M.S.*, vol. 72, 1928) have worked on the problem in Loligo in which a large quantity of yolk is present within the animal until a late stage of development. Two separate methods of yolk-absorption are employed successively. One is by the blood system and the other by the liver, which is, through the greater part of development, in close and direct contact with the yolk-sac. The liver absorbs the yolk from the yolk-sac, which it finally replaces. The so-called "pancreas" is in direct communication with the liver and is surrounded by a blood sinus. The nutritive material from the liver appears to pass through the pancreas to the blood stream. The absorptive function of the liver and pancreas in the adult is controversial, but appears more probable in the light of their absorptive powers in the embryo.

Reactions of Gammarus to Infection.—Pixell Goodrich (*Q.J.M.S.*, vol. 72, 1928) has made some very interesting observations on the reactions of Gammarus to injury and disease, notably microsporidial and fungoid infections. A yeast, Cryptococcus gammari, causes a disease in Gammarus which may be lethal, but is sometimes cured by phagocytosis and autotomy. The phagocytes effect the destruction of the parasites by secreting a chitinoid substance around them. Large chitinoid nodules which become dark brown and are visible through the body-wall may be formed in this way by the action of a number of phagocytes. The spores of Nosema and of Thelohania are also pathological and may destroy a muscle-fibre of Gammarus. Phagocytes which secrete a chitinoid substance around the spores and destroy them may then attack this fibre. The black or pathological chitin which closes wounds in Gammarus is a similar substance and is a product of the leucocytes. This chitinous secretion of the leucocytes in Gammarus is not, however, chemically identical with the true chitin secreted by the epidermis.

Histology of the Mammalian Thymus and Adrenal.—The problem of histogenesis of the thymus has excited considerable attention from time to time. Deanesly (*Q.J.M.S.*, vol. 72, 1928) has studied the problem of the thymus reticulum from an experimental point of view, by means of X-ray treatment and tissue culture.

In the mouse irradiation is followed almost immediately by degeneration of most of the small thymus cells of the cortex, by amitotic proliferation of the connective tissue and by a peripheral invasion from the sheath by fresh connective tissue and blood-vessels. Numerous ciliated cysts, derived from the blood-vessels, develop but soon disappear. Regeneration begins three days after irradiation and results in a reconstruction of the cortex from thymus cells persisting at the cortex edge. Fresh medullary areas are formed by the ingrowth of connective tissue and blood-vessels, most of which degenerate and give rise to corpuscles of Hassall and cysts which soon disappear.

Pieces of embryonic cat thymus cultured in tubes of plasma exhibit similar changes in the implants (to those resulting from irradiation). These consist in pycnosis of the small thymus cells and proliferation of fibrous connective tissue, which forms a border round the implant in which fresh corpuscles of Hassall develop. According to these results there is no epithelial reticulum in the thymus such as is commonly described, and the corpuscles of Hassall and cysts arise from the degenerating blood-vessels and accompanying connective tissue.

During recent years several papers have appeared describing

histological differences in the cortex of the adrenal gland of mammals at different ages and of different sexes. The cortex of the adrenal can be divided into an outer zona glomerulosa, a middle zona fasciculata and an inner zona reticularis according to the arrangement of the cells. The chief differences associated with age and sex concern the last-mentioned zone.

Deanesly (*P.R.S.*, B. vol. 103, 1928) recently has made an extensive reinvestigation of this subject in the mouse (which clears up several points previously in doubt). It is found that at three weeks old the adrenals are alike in the two sexes and have a distinct inner cortical zone. Growth of this zone stops in the male before the fifth week and a small amount of degeneration takes place. This zone in the female continues to grow until puberty, when it occupies more than half the cortex. It degenerates slowly after puberty in the unmated female and normally disappears before the end of the reproductive period. It has been claimed that cyclic changes take place in the histology of the cortex corresponding with the œstrous cycle. Deanesly finds no such correlation, but notes that in all mice pregnancy leads to complete degeneration of the inner cortical zone unless it has previously disappeared. Sometimes a new inner zone arises later in the cortex and degenerates in its turn. Castration of the male results in the formation of an inner cortical zone of the female type, but ovariotomy appears to have no effect. Double adrenalectomy slightly lengthens the œstrous cycle in the unmated female, but does not affect breeding.

Donaldson (*Proc. Soc. Exp. Biol. and Med.*, vol. 25, 1928) has shown that the adrenal glands of the wild grey rat weigh roughly twice as much as those of albino rats of the same sex and body weight. The absolute amount of chromaffin tissue in the wild grey is greater than in the albino, but most of the larger mass consists of cortical tissue. It is therefore clear that both the absolute and relative mass of cortical tissue is greater in the wild grey rats.

Intersexual Pigs.—Baker (*Brit. J. Exp. Biol.*, vol. 6, 1928) describes some interesting results of his expedition to the New Hebrides to investigate the peculiar intersexual pigs which are found there. These intersexes are of a new type and are distinguishable from those found occasionally among European pigs by the complete absence of uterus or vagina. Their anatomy is described and it is suggested that they are genetic males in which the testicular hormone has been produced too late in development.

These intersex pigs occur only in the more northerly islands of the New Hebrides, where they are extraordinarily abundant. They occur in nearly every little native village and it is

estimated that there are between ten and twenty intersexes per hundred normal males. On one occasion 125 intersexes were seen together.

The natives regularly breed these pigs and maintain that a female which has produced some will usually produce one or more in every farrow, and further, that females born in the same farrow as intersexes are themselves intersex producers. It is shown that these native reports fit in with the view that these intersexes are produced by a sex-linked character. At all events it is clear that the natives are able to breed them in remarkable numbers.

The importance of the intersex pigs in the social life of the non-Christian natives is remarkable. The interest taken in them is consequently very great, and they sell for considerably higher prices than the normal animals. The various grades of intersexuality found have each a special native name.

Each time a chief obtains higher rank a definite number of intersexes and males are killed as an essential part of the ritual. The highest rank is only attained after the slaughter of ten to twenty full-grown intersexes with large tusks. The remarkable frequence of these curious freaks is thus due to the social importance set on them by the natives and the care with which they are bred in consequence.

The Breeding Season in the Opossum.—Hartman (*Journ. Morph.*, vol. 46, 1928) has made some interesting observations on the breeding season and rate of development of the Opossum (*Didelphis virginiana*) in Texas. The season commences, after a three months anœstrus, at the beginning of January, and the percentage of females pregnant reaches a maximum in the third and fourth weeks. Copulation precedes ovulation by 12 hours and gestation lasts about 13 days. The pouch young begin to open their eyes and lips when they are about 50 days old and have attained the size of an adult mouse, but they continue to suckle for 30 days more. The mother again becomes pregnant soon after weaning the first brood, but it is improbable that more than two broods are reared in the year, except in rare cases.

The rate of intra-uterine development has always proved difficult to determine with accuracy, owing to the impossibility of determining the exact time of fertilisation. Hartman surmounted this difficulty by surgical removal of one uterus, noting the stage of eggs or embryos which it contained, and allowing the surviving uterus to incubate its ova a given time. It is remarkable that this operative treatment did not result in abortion or resorption of the embryos in the untouched uterine cornu, which continued to develop in an apparently normal manner. The method enabled accurate measure-

ments of growth and development over a precise period to be made.

It was shown that the rate of development of the Opossum during the first ten days resembles most closely among the Eutheria that of the rabbit. The primitive streak stage is attained 7½ days post coitum. This leaves only 5½ days for the subsequent embryonic development prior to birth, which is therefore very rapid. The curve of postnatal development resembles the embryonic growth curve of the higher mammals. The paper also contains some remarkably beautiful figures of Opossum embryos of various stages.

Loris and the Evolution of the Primates.—Any new information regarding the evolution of the Primates is of special interest since it concerns the ancestry of Man. Some years ago Hill (*P.Z.S.*, 1919) put forward the view that the Lemuroids, so far as their development is concerned, are the most primitive of existing Primates and can be regarded as the direct descendants of that stock from which all the higher Primates evolved along one or more lines of descent.

This view is corroborated in a recent important paper by Hill, Ince and Subba Rau (*P.Z.S.*, 1928) on "The Development of the Fœtal Membranes in Loris," embodying important observations on the mode of vascularisation of the chorion. The chorion in Loris differentiates early and forms, relatively early, the entire outer wall of the embryonal sac. Moreover, its vascularisation does not depend on the slow growth and ultimate fusion of the allantois with the chorion as in lower mammals, but is effected precociously. This takes place by the direct ingrowth into the chorion of the umbilical vessels as soon as the primary allantoic vesicle, bearing them, unites with a localised area of its inner surface. The primary function of the allantois is to provide a bridge or pathway along which the umbilical vessels can reach and invade the chorion at an early stage. The allantois, in fact, acts at first simply as a vesicular connecting stalk.

This method of vascularisation in Loris apparently represents "the first and most significant step towards the evolution of that characteristic structure, so long regarded as distinctive of the higher Primates, the connecting or body stalk, which in Tarsius and the Anthropoidea directly connects the embryo with the chorion and serves as the pathway for the umbilical vessels to and from that membrane."

These observations on the Lemur thus enable us to bridge the gap between the primitive allantois of the lower mammals and the highly specialised condition represented by the connecting stalk in Man. Loris provides precise knowledge of the first stages in the evolutionary history of this precociously

developed and greatly reduced representative of the allantois of the lower mammals, and shows that it is another example of developmental acceleration or heterochrony which is such a marked phenomenon in the early embryology of Man.

PEDOLOGY. By Prof. G. W. Robinson, M.A., University College of North Wales, Bangor.

Although the past year has witnessed no signal advances in our subject, much steady work is being done with the object of defining more clearly those aspects of the study of the soil which have occupied attention during the last decade. And this, indeed, is the kind of progress which must be expected, for it is unlikely that any revolutionary discovery will change the whole aspect of pedology. Our advance will probably be slow and laborious rather than spectacular on account of the very diversity of the material. In no branch of science is it more dangerous to generalise on scanty data, and it is thus necessary to prove every advance in our knowledge by investigation over as wide a field as possible.

New Books.—Among the books published during the past year, two call for mention. The first, though a translation, has much of the merit of original work. I refer to *The Evolution and Classification of Soils* (Cambridge, Heffer), translated by Dr. C. L. Whittles from the German of the late Prof. E. Ramann. The translator has improved on the original both by amplifying the bibliography and also by inserting helpful notes on certain aspects of the subject. The book may be strongly recommended to the attention of students of the soil. *The Soils of Cuba*, by H. H. Bennett and R. V. Allison (Washington, Tropical Research Foundation), must be specially noticed because it is the first generally accessible example of the application of the newer methods of the United States Soil Survey to work in a tropical country. Dr. C. F. Marbut, Chief of the Survey, contributes a section on the principles of classification which explains the methods used in the work. I shall have occasion later to allude to this subject. The different series of soils distinguished are fully described both by field observations and by laboratory data. In addition, much valuable agricultural information of correlative importance is given. It is worthy of note that some of the soils of Western Cuba are assigned to series of which there are representatives in the south-east of the United States. The soils of Eastern Cuba are more characteristic of tropical conditions. The differences encountered in the properties of soils of similar colloidal content but of different chemical composition are significant. It is interesting to learn that certain ferruginous clays are markedly stable to erosion. In

view of the future importance of tropical agriculture and the dangers attending unintelligent exploitation of virgin soils, this book is of considerable importance and may well serve as a model for future surveys of tropical regions.

International Society of Soil Science.—The proceedings of the First Congress of Soil Science, held at Washington in 1927, have now been published from Washington and will stand as a record of the present state of studies in pedology. The work is in four volumes, amounting in all to over 2,700 pages. The work of the different sections has already been summarised by B. A. Keen, A. A. J. De Sigmond, S. A. Waksman, D. R. Hoagland, C. F. Marbut, and others (*Soil Science*, 1928, **25**, 1–106). It is only possible on the present occasion to allude briefly to the principal topics dealt with by each section. The First Commission (Soil Physics) was principally occupied with discussing and standardising methods of mechanical analysis, single value determinations, and soil moisture problems. An international method of mechanical analysis was agreed upon. In the Second Commission (Soil Chemistry) the following problems were discussed: (1) determination of plant nutrients in the soil; (2) acid extraction of the soil; (3) soil acidity and base exchange; and (4) soil organic matter. A large number of contributions deal with the influence of exchangeable cations on soil reaction and with methods for determining the saturation deficit of soils. Agreement on the nature of the base exchange reactions is still to be reached. The simple hypothesis underlying the methods of Hissink would not appear to be a sufficient explanation of all the experimental facts. Meanwhile, arrangements have been made for the continuance of co-operative work. Discussion in the Third Commission (Soil Microbiology) centred round methods of investigation, humification problems, and the nitrogen cycle in soils. The Fourth Commission (Soil Fertility) was occupied with the mechanism of plant nutrition, and methods of determining the status of the soil with regard to plant nutrients.

From many points of view, the work of the Fifth Commission (Classification and Mapping of Soils) was of principal interest, particularly as a month's tour of the United States and Canada was made in order to examine soil types in the field. The Commission in Washington definitely addressed itself to the task of obtaining, if possible, a comprehensive system of soil classification to include all types of soil in all parts of the world. It was inevitable that the individual contributors to this discussion should press their own particular aspects of the problem and some sharp differences of opinion emerged. The proposals of C. F. Marbut, embodying the scheme actually used at present by the U.S. Soil Survey, appear to be more

comprehensive in their scope than any yet advanced. The climatic system of the Russian school of pedologists has been evolved mainly as the results of studies in the Russian Empire, where, in spite of its great extent, the variety of pedogenic possibilities is not so great as in the United States and Canada, especially since the studies of the American school have been extended to tropical soils. Marbut stresses the importance of considering the soil as a natural body and regarding the soil profile as the unit of study. The principal divisions of his classification rest, nevertheless, on a climatic basis. Briefly, Marbut's classification consists of a series of divisions based on the mature soil profile. Firstly, there is the division into "*pedocals*" and "*pedalfers*," the former characterised by a zone of calcium carbonate accumulation, and the latter by the absence of such a zone, and by vertical migration of sesquioxides. This division is practically the same as the division into arid and humid proposed by Hilgard, except that it is stated in terms of the soil itself. The next division is made on the basis of temperature and gives, in the case of the pedalfers, the podsolic soils on the one hand and the lateritic soils on the other. The corresponding classes in the pedocals have not been worked out. In the next subdivision, moisture conditions are used. This gives, in the case of the mid-latitude pedocals, the well-known succession ranging from the tshernosems or black earths to the grey desert soils. The corresponding divisions in the remaining pedocals and in the pedalfers remain to be distinguished. Then follows a further subdivision on the basis of temperature giving, in the case of the podsolic soils, the succession, tundra, podsol, brown earth, red earth, yellow earth, with prairie soils as a side class, and in the case of the lateritic soils, the laterites and the ferruginous laterites. Again, in the case of the pedocals, gaps remain to be filled in this stage of subdivision. The final categorisations are made by the successive consideration of profile maturity, nature of parent material, and texture. On the whole the scheme is the most comprehensive hitherto propounded, and it will doubtless be possible, when further experience has been accumulated, to fill in the gaps which admittedly exist.

Constitution of Clay.—The nature of clay, the principal inorganic reactive constituent of soils, has been the subject of much investigation of recent years. Unfortunately, much of the work on clay in other branches of applied chemistry is not generally accessible to pedologists. A paper by C. S. Ross (*Proc. First Congr. Soil Sci.*, 1928, Vol. IV, 555–565) is therefore worthy of special attention. The author gives the results of studies by X-ray and other methods of the minerals of clay. Clay consists of two groups of minerals.

In the first group are the crystalline minerals, kaolinite, $Al_2O_3 \cdot 2SiO_2 \cdot 2H_2O$, anauxite, $Al_2O_3 \cdot 3SiO_2 \cdot 2H_2O$, halloysite, which has the same formula as kaolinite but is submicroscopically crystalline, and leverrierite which approximates to anauxite in composition. The water of the minerals of this group is firmly combined and only lost at high temperatures. The second group of minerals includes montmorillonite, $(Ca,MgO)Al_2O_3 \cdot 5SiO_2 \cdot 5H_2O \pm$, beidellite, $Al_2O_3 . 3SiO_2 + \cdot 4H_2O \pm$, its ferric isomorph, nontronite, and a crystalline halloysite. In the second group of minerals, the water is more loosely held and may be regarded as adsorbed. The dominant clay-forming mineral of ordinary soils is beidellite associated with its ferric isomorph, nontronite. Kaolinite is an uncommon constituent. X-ray studies show that clay minerals are truly crystalline and not, as is often supposed, amorphous gels. Montmorillonite is the characteristic mineral of bentonite.

The constitution of clay has also been investigated by other authors, who have studied the natural substance. H. W. Kerr (*Soil Sci.*, 1928, **26**, 385–398), from the fractionation of the clay of certain soils, concludes that the composition of the constituent responsible for base exchange reactions agrees with the formula $H_2O(Al_2O_3 \cdot 6SiO_2) \cdot 8H_2O$. G. W. Robinson (*Nature*, 1928, **121**, 903), from a study of the clay fractions of a number of soils, concludes that the primary product of silicate weathering is a mixture of hydrated sesquioxides having a silica-sesquioxide ratio of 2·0. Variations from this ratio occur as a result of eluviation, whereby the ratio is increased in the A horizon and decreased in the B horizon of a podsol profile. The higher ratios in alluvial soils and in unconsolidated aqueous deposits generally is attributed to concomitant precipitation of silicic acid present in river water, and originating from silicate hydrolysis. R. S. Holmes (*J. Agric. Res.*, 1928, **36**, 459–470) finds the inorganic colloidal material approximately constant in composition in the soils of one type. The results indicate that the eluviation under such conditions is purely mechanical.

Synthesis and Properties of Clay.—S. Mattson (*Soil Sci.*, 1928, **25**, 289–311), by mixing varying proportions of sodium silicate and aluminium chloride solutions, has obtained a series of electropositive sols, isoelectric precipitates, and electronegative sols. An isoelectric precipitate corresponds with each ratio of silica to alumina. The ratio becomes zero at *p*H 7·0, which is the isoelectric point of alumina, and reaches a maximum, below 3·0, at a *p*H of about 5·0. Precipitates of higher silica-alumina ratios may be prepared by increasing the proportions of cations present. As in the case of natural gels, the base exchange capacity increases with the silica-alumina

ratio. Evidence is adduced in favour of the occurrence of true adsorption rather than neutralisation. The acidoid behaviour of adsorbents is held to be associated with polar orientation of the interfacial layer of water molecules. The same author (*ibid.*, 1928, **25**, 345–350) finds that alternate treatment of soil colloids with neutral sodium chloride solution and 0·05 normal hydrochloric acid liberates greater quantities of aluminium and iron in solution than treatment with acid alone. This is associated with the presence of a high concentration of chloride ions when neutral salts are present. Colloidal clay can be completely decomposed by treatment with a mixture of dilute acid and neutral chloride.

Soil Organic Matter.—S. A. Waksman and K. R. Stevens (*Soil Sci.*, 1928, **26**, 113–138) have distinguished between different types of peat, notably between fen peat and upland peat, by methods of fractionation which account for about 90 per cent. of the organic matter present. In fen peat the true celluloses have completely disappeared, the surviving hemi-celluloses being of hexosan character. The protein content is higher than in the original plant materials on account of the activity of nitrogen fixing organisms. In upland peat, on the other hand, considerable amounts of cellulose and hemicelluloses survive and the protein content is lower than in the original plant materials, owing to the nitrogenous compounds having been drawn upon for the microbial decomposition of the carbohydrates. S. A. Waksman and F. G. Tenney (*Soil Sci.*, 1928, **26**, 155–171) show the influence of plant species, age of material and other circumstances on the decomposition of organic matter in the soil. W. L. Davies (*J. Agric. Sci.*, 1928, **18**, 682–690) has attempted a fractionation of the nitrogenous compounds of different types of peat. Both the hydrochloric acid hydrolysate and the hydrogen peroxide extract have been examined, and indications have been obtained of differences in the distribution of the fractions. The results show that protein degradation is more rapid under fen conditions than under upland peat or heath conditions. U. Springer (*Z. Pflanz. Düng.*, 1928, **11**A, 313–358) proposes the use of acetyl bromide as a reagent for distinguishing the humified from the non-humified organic matter of soils.

Soil Solution.—L. J. H. Teakle (*Soil Sci.*, 1928, **25**, 143–162), in a study of the factors affecting the concentration of phosphates in the soil solution, finds that slightly acid conditions depress the solubility of aluminium and manganese phosphates. Iron phosphate is least soluble at *p*H 3·0. Calcium phosphate is insoluble under alkaline conditions. Calcium is the most important base in its effect on phosphate solubility. J. S. Joffe and H. C. McLean (*Soil Sci.*, 1928, **26**, 47–59) find that

the solubility of aluminium in the soil solution is not controlled by the pH alone but also by the anions present. Gel formation occurs at different pH values in the presence of different anions. The formations of gels excludes the presence in the soil solution of molecular-disperse iron and aluminium. Under ordinary soil conditions the presence of iron and aluminium in the soil solution is generally excluded by the inorganic anions present.

Soil Structure and Mechanical Analysis.—A. T. Tiulin (*Dept. Agric. Chem. Perm., Report*, 1928, 1–24; 77–122) has studied the stability of soil structure, which he correlates directly with the adsorptive capacity of the soil for calcium and inversely with the proportion of silt (0·01–0·005 mm.) present. A mechanical factor of structure stability is suggested, namely, the ratio of silt to adsorptive capacity. In a second paper Tiulin distinguishes two types of aggregates in natural soils, namely, floccules held together by bivalent and tervalent cations (true aggregates) and aggregates held together by univalent cations or by simple cohesion (false aggregates). True aggregates are stable in water, whilst false aggregates disintegrate in water. The proportion of true aggregates may be determined by sieving in water, that of false aggregates by sieving in the dry state, applying a correction in the second case for the proportion of true aggregates present. The data for true aggregates must also be corrected for actual primary particles present. F. Hardy (*J. Agric. Sci.*, 1928, **18**, 252–256) proposes as an index of the texture of soils the moisture content at point of stickiness, less one-fifth of the sand present, the latter term being introduced to correct for interstitial moisture associated with the sand.

Mechanical Analysis.—E. M. Crowther (*Proc. First Congr. Soil Sci.*, 1928, Vol. I, 394–398) describes a manometric method for deriving continuous curves for the mechanical composition of soils. The method depends on the measurement of the difference in hydrostatic pressure between two levels of a settling suspension. M. Köhn (*Landw. Jahrb.*, 1928, 485–526) discusses and compares the different methods proposed for mechanical analysis. From mathematical analysis and from actual photographs of stream lines with suspensions of lead iodide, he concludes that the pipette technique is more readily controllable and trustworthy than sedimentation methods. Miss R. C. Groves (*J. Agric. Sci.*, 1928, **18**, 200–205) finds that repeated rubbing with a rubber pestle, using ammoniacal hydrogen peroxide as a deflocculant, gives a satisfactory dispersion of laterites, which have hitherto proved difficult to disperse in mechanical analysis. M. D. Thomas (*Soil Sci.*, 1928, **25**, 419–427) finds that sodium-saturated soils give the greatest dispersion in mechanical analysis. This dispersion

may be most readily effected by preliminary acid treatment, followed by treatment with sodium carbonate. Cases are found in which ammonium soils show the best dispersion. S. Mattson (*Proc. First Congr. Soil Sci.*, 1928, Vol. II, 185–198) also finds that sodium-saturated soil colloids show the highest dispersion.

Effect of Drying on Soil Properties.—J. L. Steenkamp (*Soil Sci.*, 1928, **25**, 163–182 ; 239–251 ; 327–332) has examined the effect of drying on various analytical determinations on soils, including exchangeable base content, lime requirement, and Hissink's T-S value. The differences obtained, not all in the same direction, emphasise the importance of moisture changes on the colloidal properties of soils. The behaviour of cultivated soils differs from that of partly weathered soils.

Soil Physics. Single-value Determinations.—The need has long been felt for single-value constants which shall give numerical expression for those properties of the soil associated with its character as a colloidal system. B. A. Keen and J. R. H. Coutts (*J. Agric. Sci.*, 1928, **18**, 740–765) have published an important work giving the more important physical data for a comprehensive series of soils of differing characters. The effect of the organic matter was studied by carrying out a separate series of determinations on soils treated with hydrogen peroxide. It is interesting to note that the values obtained for the pore space of kneaded blocks approximate to that calculated for an "ideal soil," namely 26 per cent.—a figure reduced somewhat by removal of peroxide-soluble organic matter. The correlation between the different physical constants obtained have been worked out. The correlation between the moisture content at the point of stickiness (S) and the content of colloidal material is well shown. The hygroscopic moisture in equilibrium with an atmosphere of 50 per cent. humidity appears to be largely controlled by the clay content. A correlation with organic matter would also have been expected. The S value tends to a minimum of 16 per cent.—approximating to 14·6 per cent. calculated for the interstitial moisture of an "ideal soil" with closest packing. The S value may thus be held to consist of 16 per cent. of non-colloidal moisture and the remainder associated with the soil colloids. It appears that, as a first approximation, the colloidal organic matter takes up 4·4 times its weight of water and the colloidal clay 2·7 per cent.

Molecular Water Capacity of Soils.—A. F. Lebedeff (*Pédologie*, 1928, 49–69) has found that in the determination of the moisture equivalent of soils, an increase in the velocity of centrifuging gives progressively smaller values for the water retained until, when a centrifugal force equal to about

18,000 g is reached, no further diminution is obtained. The amount of water held by a soil against such a force is considered to represent the maximum molecular water capacity. This value appears to be additive among the mechanical fractions of the soil.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

Turbulence in the Atmosphere.—Vertical movements in the Earth's atmosphere are made evident by the growth of the ordinary cumulus cloud. The circulation of surface winds around an anticyclone or a depression show on the one hand an area of outflow and on the other of inflow, often with little change in the total weight of the atmosphere over the area in question, as revealed by the barometric pressure on the ground. Such systems of outward and inward flow imply descent and ascent of air; vertical movement on a larger scale than that of the cumulus cloud is revealed. The attention of meteorologists has been turned towards such phenomena ever since they have been recognised, while the more obvious but apparently still more complicated vertical movements that can be detected when a wisp of the low fragmentary cloud known as "scud" or "fracto-cumulus" is closely watched on a day of strong wind, or when the trajectory of any object floating in the air is observed, seldom received much attention until aviation began. Even then it was not always understood that this irregular motion, which is technically known as "turbulence," is of importance not only in regard to minor peculiarities of wind-structure, but is actually capable of affecting the weather so often and so powerfully as to constitute an important factor in determining the climate of a place. "Turbulence," or "eddy-motion," must not be confused with the convection of warm and cold air, which may also be somewhat turbulent in character. True turbulence is set up when smooth motion of the wind is interfered with by any obstacle; it may be regarded as composed normally of an almost infinite variety of shapes and size of eddy. The normal "lapse" of temperature in the atmosphere of a large proportion of the solid Earth is such that vertical motion is opposed by restoring forces due to the fact that air in ascending immediately becomes colder and heavier than its environment, while descending air becomes warmer and lighter; this shows that turbulence acts in opposition to those very forces which favour simple thermal convection due to replacement of warm air of small density by colder and denser air. It is not proposed to deal in detail with the mathematical treatment of turbulence, but it may be recalled that attempts have been made in this

country by G. I. Taylor to use mathematical equations developed in the theory of conductivity of heat for a study of the vertical transfer of heat due to the action of turbulence. These attempts have been based upon the idea that although individual eddies are too complicated for mathematical treatment, the interchange of air between different levels resulting from very numerous eddies causes simple thermal changes that can be treated as due to ordinary conductivity—that corresponding with the coefficient of thermal conductivity of a solid there is in fact a coefficient called the "eddy-conductivity." Such investigations have greatly increased our understanding of many meteorological processes, although the analogy between the two kinds of heat transference must clearly not be pushed too far.

These notes have been written to draw the attention of the general reader to the practical importance of turbulence for a wide range of meteorological phenomena, this introductory matter leading up to a brief account of some recent observations made in Germany, the significance of which would at first sight appear to be of more limited importance to meteorology than is actually the case. The following phenomena are all effects of turbulence, and are all of importance in the study of weather if not of climate.

(1) A rise of temperature with onset of wind on still nights when temperature has become low on the ground and for some distance above it, on account of outward radiation under a clear sky. Under such conditions the temperature usually increases with height ("inversion" of the usual "lapse" with height). The stirring due to eddy-motion is thus particularly effective in raising surface temperature, because the change of pressure of air due to its descending from high to low levels, which produces the usual rise of temperature according to the "dry adiabatic" rate, acts upon air which at the higher level is already warmer than the air near the chilled ground. A rise of many degrees sometimes takes place in a short time, even with the sky remaining quite clear.

(2) Formation of cloud sheets when cold winds with a moderate lapse of temperature with height travel long distances over relatively warm water. The lower surface of such a cloud sheet represents the level at which turbulence brings moistened and warmed air to a height where it is cooled below its dew-point. The frequent cloudiness of the weather when northerly winds reach the east coast of England is due to this process.

(3) "Scud," or fracto-cumulus cloud, appearing in the first few hours after sunrise in damp but sunny weather with a strong wind, especially in summer. The process here is taken

to resemble that just described, but is more rapid owing to the powerful heating of the ground.

(4) Gustiness of inland winds compared with winds out at sea. This is due to the turbulence set up by objects on land. A. H. R. Goldie has shown, however, that ocean waves sometimes form eddies, which tend to have a "period" equal to that of the waves, *i.e.* maxima of wind speed tend to occur at intervals of time equal to those separating the largest waves.

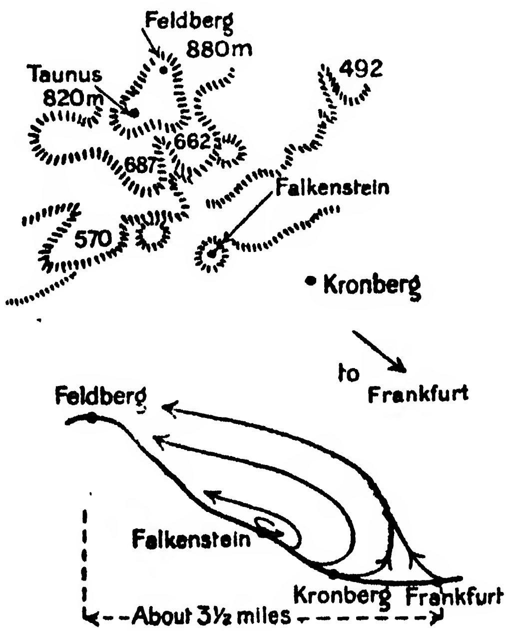
(5) The diurnal variations of the average speed and direction of the wind on land. This is due mainly to corresponding variations of turbulence. During the warmest part of the day the "lapse-rate" of temperature with height is at its greatest, and there is little to oppose the formation of upward and downward currents—turbulence is therefore readily set up. The greater the turbulence the more rapidly is air which has been retarded by friction near the ground diluted with faster moving air from above. At night the opposite thermal structure tends to occur, especially if the sky is clear and cooling near the ground consequently rapid. The air above the level of any obstruction then tends to move onwards without eddy-formation, the lower layers becoming more and more stagnant.

The five examples of the results of turbulence given above suffice to show that the causes of many weather phenomena would be obscure without some knowledge of this process and it may well be that even more phenomena will be explained in the future when the process is better understood. In mountainous districts, for example, the comparatively simple relationship between surface and upper winds which obtains as a rule over flat country, and even more over the open sea, is replaced by complexities, due to some extent to turbulence set up by the mountains, which make it hard to estimate the one from the other. The existence of valley winds and of the föhn has of course long been recognised, but the turbulence set up by hills or mountains has not often received attention. It is very satisfactory, therefore, to see a paper by Dr. W. Georgii ("Veröffentlichungen des Forschungs-Institutes der Rhön-Rossitten-Gesellschaft e.V Nr. 1," *Jahrbuch* 1926/27, published 1928, pp. 1–4) dealing with this subject, and one which appears to include some important results.

Georgii points out that most of the pictures obtained of the eddies set up on the lee side of an obstacle refer to artificial wind channels, where boundary surfaces occur to which there is usually nothing analogous in the free atmosphere; moreover, when the obstacle is as large as a mountain, thermodynamical changes of temperature and pressure due to ascent or descent of air become important, and introduce further discrepancies.

Theory would lead one to expect that when air is cooled dynamically on ascending a mountain-side, a rise of pressure should result on the windward slope, owing to the greater density of the cooled air, and this should cause a horizontal gradient of pressure near the ground such that a wind may be caused in the reverse direction to that of the general current, blowing down the slope, provided that the " lapse-rate " is less than the " dry adiabatic," *i.e.* less than 1° C. per 100 metres.[1]

Such an eddy would have no analogue in wind-channel experiments. It is pointed out that whereas P. Raethjen,[2]



" filming " rocket smoke on certain hill slopes, found no evidence of this effect, Lammert[3] found a flattened eddy on the windward slopes of the Southern Alps, with the convergence and divergence lines which Wenger regards as characteristic of the horizontal flow of air near mountains. These eddies formed on days when the horizontal pressure gradient was too strong for downflowing " mountain breezes "

[1] With a lapse-rate exceeding that value, rising air would of course not tend to become colder than its environment, but warmer, while the " dry adiabatic " itself would lead to up-and-down movement with the moving air always keeping the same temperature as its surroundings, provided that formation of clouds and liberation of latent heat did not take place.

[2] *Z.F.M.*, 1926, p. 185.

[3] *Veröff des Geophysik. Instituts Leipsig*, 2[2] Series, Vol. 2, part 7.

to form. Georgii, in the paper under review, describes observations yielding additional proof of the existence of such eddies. They were made in the neighbourhood of Frankfurt, during the period June to September 1918, using observing stations on a line running from N.W. to S.E., including the peak of Feldberg (880 metres), the Taunus Observatory (820 m.), the hill Falkenstein (400 m.), Kronberg (about 200 m.) and Frankfurt (117 m.), and covering altogether a distance of about four miles (see figure). Simultaneous observations with kites furnished free-atmosphere values of wind and temperature at heights of 1,000 m., 1,500 m., and 2,000 m. The characteristic eddy was found, with wind blowing downhill, near Falkenstein (see figure), when the free-air winds were from some point between S.E. and W.S.W. (through S.; see figure).

The eddy—proof of the existence of which it should be noted is based largely on the wind at Falkenstein—was especially well shown when the free-air wind was from S.W., *i.e.* at about 90° to the line of stations. It occurred in 57 per cent. of all cases of free-air wind between 0 and 5 metres/sec., in 50 per cent. with 5–10 metres/sec., and in only 13 per cent. for winds exceeding 10 metres/sec.

The following table shows the mean lapse-rate at various heights for each class of wind-speed:

LAPSE-RATE IN DEGREES C. PER 100 METRES

LAPSE-RATE

Speed at 1,000 metres (metres/sec.)	Falkenstein (400 m.)	Taunus (820 m.)	Over the mountains. 1,000 m.	1,500 m.	2,000 m.	No. of Obs.
0–5 . .	0·49	0·67	0·85	0·63	0·67	28
5–10 . .	0·55	0·69	1·12	0·42	0·60	20
Over 10 . .	0·86	0·74	0·33	0·64	0·47	31

The downward-flowing wind at Falkenstein requires a suitable pressure gradient along the slope, which, as has already been shown, may result from increased pressure on the upper part of the slope due to the cooling of the ascending winds, provided that the lapse-rate is less than the "dry adiabatic." The above table shows that in the two classes of wind strength which showed frequent development of the eddy, the lapse-rate at Falkenstein and Taunus was appropriate, and that in the case of stronger winds, when the eddy was rarely developed, the lapse-rate was approaching the dry adiabatic rate (1° C. per 100 metres).

A more detailed analysis of individual cases showed that the limits of the wind-speed on the mountain top which were

favourable for the eddy were 2 to 10 metres/sec. The latter speed would appear therefore to be a critical one, above which the atmosphere becomes churned up into irregular "turbulence." This speed, it is interesting to note, has been found to be a critical one by Peppler, Wenger, and Bingel, when considering wave-motion in the free atmosphere, and H. von Ficker[1] found an abrupt change in the difference between the temperature on the Zugspitze and in the free-air at the same height, on passing from winds of 9 metres/sec. to those of 11 metres/sec., as the following table shows :

Wind Speed (metres/sec.)	0	2	3	5	7	9	11	13-20
Temperature difference (Zugspitze—Free air) deg. Centigrade	−0·42	−1·25	−2·65	−1·74	−2·02	−2·50	−0·81	−0·93

TORIC ARCHÆOLOGY. By L. J. P. Gaskin, Librarian to the Royal Anthropological Institute.

Anzeiger für schweizerische Altertumskunde, 1928.—Contains the continuation and conclusion of Herr G. Kraft's article on the position of the Swiss in the Bronze Age cultures of Central Europe, reviewed in Science Progress of July last. The final portion of this paper deals with the Bronze Age Lake-dwellings and the connection with Italy. In conclusion the author attempts a chronology of the Swiss Bronze Age in relation to the associated cultures of Europe. The article is well illustrated.

Antiquity, December 1928.—In a long article on "The Oldest Swiss Lake-dwellings," M. Paul Vouga classifies the succession of Neolithic cultures (or layers) excavated, as Lower (or early Neolithic) ; Middle Neolithic Upper (or late) Neolithic, and Eneolithic (or Copper) age, and proceeds to describe in detail the differences in the common objects found in each layer (or culture). These objects he groups together under the headings of (*a*) Axe-holders (*gaines de haches*), (*b*) Flint, and (*c*) Pottery.

This paper is particularly well illustrated and has a map of Lake Neuchâtel.

Mr. O. G. S. Crawford contributes an illustrated article on Stone Cists. He thinks that they were probably introduced into Britain by the Beaker-folk about 1800 B.C., and that they were used for over 2,000 years, as examples have been found containing objects belonging to the Iron Age. The author con-

[1] *Met. Zeit.*, 1913, p. 285.

siders that Cist-burial was the commonest method of interment in Prehistoric times, when suitable material was available.

Mr. J. M. de Navarro, in an article on "Massilia and Early Celtic Culture," surveys in detail the archæological remains of the Hallstatt and La Tène periods which extend over France and Central Europe, and particularly those having Græco-Italian affinities. By these means he establishes the probable routes taken by the early Grecian traders. There is a good map and some excellent illustrations.

Mitteilungen der anthropologischen Gesellschaft in Wien (**58**, part 5).—Herr J. Bayer reports on a late Bronze Age cremation burial at Nieder-Russbach, Lower Austria. The age of the finds has been determined by the pottery, which was well baked and of a brown and black colour. Herr Bayer associates the finds with the "Lausitz Culture," one of the "urnfield" cultures of Europe which mark the later phases of the Bronze Age.

Præhistorische Zeitschrift (**19**, parts 1–2).—Herr Lothar F. Zotz writes on the palæolithic settlement of "Der Teufelskuchen," at Oelberg near Kuckucksbad. Herr Zotz, after a careful survey of the geological evidence, archæological material, fauna and flora, and history of the site, comes to the conclusion that it belongs to the Magdalenian Period. A series of excellent plates are attached to the article.

In an article on the spiral ornamentation of pottery, Herr J. Boehlau deals with (1) The Bohemian-Moravian Pottery, (2) The Butmir Pottery, and (3) Painted Pottery. The article is well illustrated.

Bulletin de la Société préhistorique française, November 1928.—Contains an article by Dr. Pontier on the discovery of a statuette of female form at Lumbres (Pas-de-Calais). The particular interest of this figure lies in the fact that it belongs to the Magdalenian Period, in which, up to the present, no other representation of the female form has been found.

M. A. G. Poulain contributes a useful article on the Palæolithic and Neolithic sites in the vicinity of Vernon (Eure). The practice of compiling inventories of this description is one to be commended, and it is hoped that others will follow this example.

Zimbabwe.—On December 13 Miss G. Caton-Thompson left England to take charge of an archæological expedition to Southern Rhodesia. The expedition, which has been sponsored by the British Association for the Advancement of Science, will investigate the ruins at Zimbabwe and Dhlo-Dhlo. It is hoped that work will be begun in March and that Miss Thompson will be able to lecture on Zimbabwe to the Association when it meets in South Africa next summer.

A short, well-illustrated account of Zimbabwe, by Mr.

G. D. Anderson, who visited the ruins recently, appears in the *China Journal of Science and Arts*, December 1928. Other works of interest are Randall MacIver's *Mediæval Rhodesia*, J. T. Bent's *Ruined Cities of Mashonaland*, and M. C. Burkitt's *South Africa's Past in Stone and Paint* (Chapter X).

Revue Anthropologique, October–December 1928.—M. Poisson continues his article on the " Neolithic Cultures of France." This portion of his article is divided into two parts: (*a*) Southern influences, (*b*) Nordic influences. In (*a*) the author deals with the Azilian and Tardenoisian Cultures, the anthropological characters of the Southern migration, and the particular features of the Southern civilisation.

M. Poisson lays stress upon the influence of Africa on the Pre-Neolithic population of Europe, and instances in this connection the Capsian civilisation in Spain and the Azilian and Tardenoisian civilisations in France.

In (*b*) the Kitchen Midden, Campignian, and Megalithic Cultures are dealt with, and this part of the article is concluded with a few remarks by the author on the ethnical characteristics of the peoples of the Northern civilisation. The value of this paper is considerably diminished by the lack of illustrations.

Count Bégouen writes on his new discoveries in the cave of " Trois Frères," at Montesquieu-Arantes (Ariège). The finds comprise (1) a small ibex head with inlaid eye, carved in reindeer horn; (2) a piece of carved bison bone; and (3) a broken handle of reindeer horn engraved with a bison's head. The most important discovery was that of the bison bone (2), on which was carved the first known representation of a grasshopper in Palæolithic art; the piece, which measured 10 cm. long by 5 cm. broad, was in good condition. Count Bégouen claims a Magdalenian origin for these finds.

Fornvännen, 1928, part 6.—Contains a long and well-illustrated article by Andreas Oldeberg on a Bronze Age find at Hjärpetan (Värmland).

M. Oldeberg compares the finds, which consist of a variety of objects, including pins, bracelets, swords, lance-head fragments, and shields, with those of corresponding Bronze Age cultures in England, Germany, Austria, and Denmark. The author gives it as his opinion that the lance-head fragments are of older origin than the remainder of the finds.

L'Homme Préhistorique, January–March 1928.—Messrs. Hamal-Nandrin and Servais write on flint daggers and lance-heads in Belgium, and point out the difficulty of distinguishing between them. Some are of Grand Pressigny flint, while others resemble the daggers of the English Beaker-folk period. The bibliography is limited to Belgium and Switzerland.

April–May 1928.—The Marquis de Baye contributes a paper on the later Iron Age helmets and their distribution in Europe, and comes to the conclusion that they were worn only by the great chiefs, whether Gothic, Frankish, Burgundian, or Alemannic. The ornamentation of the helmets shows traces of Scythian as well as Early Christian elements, and this is important because it adds to the accumulation of evidence for a cultural migration from the East.

Archives de l Institut de Paléontologie Humaine.—Four numbers have appeared: No. 1, "La Grotte de l'Observatoire à Monaco," by M. Boule and L. de Villeneuve; No. 2, "Les Poissons, Les Batraciens et Les Reptiles dans l'Art Quaternaire," by H. Breuil and R. de Saint-Périer; No. 3, "Le Paléolithique Italien," by R. Vaufrey; No. 4, "Le Paléolithique de la Chine," by M. Boule. Published by the generosity of the Prince of Monaco, these sumptuously illustrated volumes form a valuable addition to Prehistoric Archæology.

Pin-hole Cave, Creswell.—In a lecture at the Royal Anthropological Institute on November 20 Mr. L. Armstrong described the work of excavation in the inner chamber of this cave. The dominant culture is Aurignacian. An important find was the discovery of a rib (of reindeer), upon which was the engraved drawing of a masked human figure, resembling those of Hornos and Altamira. The importance of this find lies in the fact that it is the first Palæolithic drawing of the human figure found in England.

Nature, November 10, 1928.—Contains an article on recent excavations at the Cheddar Caves, by R. F. Parry. The cave showed signs of occupation during Palæolithic, Early Iron Age, and Romano-British periods. There were no signs of occupation during the Bronze or Neolithic periods. The finds of Palæolithic date include a large number of flints and a *bâton de commandement* of reindeer antler. Parts of two human skulls were found, and the animal remains include wolf, bear, reindeer, Irish elk, arctic fox, and English varying hare. A detailed report of the excavations will be found in the next issue of the *Proceedings of the Somersetshire Archæological and Natural History Society*.

Journal russe anthropologique, Tome 16.—Contains an article by A. Pavlov on the prehistoric sites at Lakes Ladoga and L'Oka and their place in the Neolithic culture of Europe.

In this article M. Pavlov makes a careful survey of the archæological material, and the animal and human remains of the two sites, and claims a Middle Neolithic origin for the culture. The article, written in Russian and French, is well illustrated.

Proceedings of the Society of Antiquaries of Scotland, vol. 62. —Mr. Eckford discusses the Terrace formations in the south of Scotland and on the English side of the border, and supports the view that the Terraces are of artificial origin. The paper is illustrated and contains a list of the Terraces and their location.

Mr. Corri writes on Bronze Rapiers from Kirkcudbrightshire, and assigns them to the middle of the Bronze Age period. The article is illustrated and contains a list of the counties in which rapier-like blades have been found.

Mr. J. Graham Callander reports on a collection of stone and flint implements from Berwickshire. Of particular interest are the lopsided and triangular arrow-heads of flint from Airhouse. The question of their use in Neolithic times seems to be one of much confusion. The illustrations, particularly of the arrow-heads mentioned, are excellent.

The Times, January 4, 1929.—In a letter to *The Times* of the above date, Mr. S. Langdon, the Director of the Oxford-Field Museum Expedition, records the new finds at Kish. In the early Sumerian stratum a series of brick-vaulted tombs was excavated containing two- and four-wheeled chariots, stone jars, and copper implements. The painted ware found in this stratum is like that found at 'Al-Ubaid, near Ur. By means of an accurate series of archæological stratification of the excavation it has been possible to establish a scientific classification of the various periods upwards to the Neo-Babylonian period, marked by the temple of Nabunidus.

There are roughly seven stages (lettered *a–g*) which have the following chronological and archæological identifications :

(*g*) 16–19 metres below mound level, *circa* 4200–3500 B.C.

(*f*) 13–16 metres below mound level. Brick-tomb area. Early period at Ur, 3200 B.C.

(*e*) Thin layer of stamped earth laid over the ruins of the great Sumerian age, *circa* 3200 B.C.

(*d*) Layer of period of decadent civilisation marked by broken sherds, 3100–3000 B.C.

(*c*) A red stratum extending over the entire mound. Rich in gold, silver, precious stones, plain pottery, copper, and inscribed tablets, 3200–2800 B.C.

(*b*) A deep stratum 5 metres thick in which the ruins of the Sargonite period prevail confused with the later debris of the Hammurabi and Cassite period, 2800–1200 B.C.

Above this lie the ruins of the Neo-Babylonian Empire.

The Times, January 11, contains a letter from Mr. L. Woolley on this season's work on the Royal Tombs at Ur.

The " death-pit " inseparable from the tomb of a king

was found, and in it were the slaughtered remains of 39 women and one man. Two daggers with gold blades and gold-studded handles and a cylinder seal inscribed "Mes-Kalam-dug," the king, were found. Other graves have been unearthed; of particular interest is one which contains the remains of a baby princess. Here was found a golden headdress, similar to that of Queen Shub-ad, and a set of miniature silver vessels. A harp was also discovered, and, surviving as lines of white fibrous powder, its ten cat-gut strings.

In a further letter to *The Times* Mr. Woolley describes the discoveries in another and larger "death-pit," containing 45 bodies. Of interest was the discovery of a silver harp shaped like a boat, a silver statue of a stag, nearly two feet high, and a pair of statues in the round of rampant rams; the heads and legs are of gold, the horns of lapis lazuli, the fleece of white shell, and the belly of silver.

It is confidently expected that the tomb of the king will be found in the near future. Illustrations of these finds will be found in the *Illustrated London News* of January 26.

The Times, January 12.—Mr. Leakey, the leader of the East African Archæological Expedition, writes on his discoveries at "Gamble's Cave," in El Menteita last month. Mr. Leakey considers that the glacial periods in Europe were accompanied by severe pluvial periods in Africa. On this assumption he thinks that he has discovered in "Gamble's Cave" a specimen of the earliest predecessor of Aurignacian man yet found. The skeleton was intact except for a damaged skull, and was buried in the sitting posture, together with the associated cultural remains of the period.

ARTICLES

THE EVOLUTION OF THE NEW QUANTUM MECHANICS

By N. M. BLIGH, A.R.C.Sc., A.I.C.

A REFERENCE to contemporary advances in theoretical physics cannot fail to bring to notice the large output of work dealing with the development and very extensive applications of the quantum theory in its newest form. This has resulted from the epoch-making advances in the last three years associated more especially with the names of Heisenberg, Schrödinger and Dirac. Their new system of quantum mechanics, allied with the notion of the quantised spinning electron, which had just previously been reintroduced, appears to afford a solution to formidable and long-standing difficulties. Though the whole matter is at present in an early stage of development, as a contribution to the quantum theory as a whole, its importance cannot be over-estimated. For a connected review of the new work it will be necessary to make a few introductory observations on the quantum numbers and on the general problem of spectra.

Bohr, in 1913, defined the orbits in which an electron might travel round the atomic nucleus, on the fundamental assumption that they were those for which the moment of momentum of the electron was an integral multiple of $h/2\pi$, h being Planck's constant. This multiple, the quantum number n, assumes successive integral values for the members of a series of concentric orbits. The second quantum number was introduced for the fine structure of spectral lines, by Sommerfeld, who, in 1915, generalised the periodic motion of an electron moving according to the inverse square law, and took into consideration its elliptic orbits. This so-called azimuthal quantum number defines the eccentricity of the orbit, and hence its shape; a radial number r defines the variation of the radius vector of an orbit. Thus we now have the principal number n which is the sum of the azimuthal and radial numbers, and determines the length of the major axis of an ellipse, and the azimuthal or subordinate number k. The latter also defines the moment of momentum of the electron about the nucleus, and thus corre-

sponds to what Bohr originally denoted by n. Orbits are denoted by the notation n_k; thus when $n = k$ we have orbits of zero eccentricity or circular orbits. The third quantum number, introduced by Sommerfeld in 1920 (the inner quantum number j) was required to account for doublets, triplets and more complex groups forming spectral multiplets. A fourth or magnetic quantum number was subsequently found necessary to define the observed lines of the anomalous Zeeman effect.

Non-Integral Quantum Numbers

It was soon found that the empirical treatment of spectral multiplets demanded non-integral quantum numbers which found no theoretical place in the older theory. As we shall see, they are a natural consequence of the theoretical deductions of the new mechanics. Arc and spark spectra contain lines most of which fall into four series characterised as S, P, D, and F series, the lines of which consist in general of multiplets. Trying to express the frequencies of the observed series of multiplet lines by relations of terms we find that for the odd multiplicities, triplets can be represented by a sequence of single S terms, and by sequences of triple P, D, and F terms. Other series of odd multiplicities can be represented by an odd number of terms in the different sequences S, P, D, F, increasing to the "permanence number" or maximum multiplicity r. When, however, the moments of momentum of the core and series electron are compounded vectorially to give the total moment of momentum of the atom, it is found that in order to get the correct number of terms we must now take for the moment of momentum of the core a vector $s = \frac{1}{2}(r - 1)$ and for the moment of momentum of the series electron a vector $j_a = k - 1$. Values of s for certain multiplicities are illustrated by the scheme:

$$\text{Singlet}_{(r=1)}\ s = \tfrac{1}{2}(1 - 1) = 0 \qquad \text{Doublet}_{(r=2)}\ s = \tfrac{1}{2}(2 - 1) = \tfrac{1}{2}$$
$$\text{Triplet}_{(r=3)}\ s = \tfrac{1}{2}(3 - 1) = 1 \qquad \text{Quartet}_{(r=4)}\ s = \tfrac{1}{2}(4 - 1) = \tfrac{3}{2}$$

This clearly shows, in the simplest outline, how non-integral quantum numbers are found to arise.

There is an intimate relation between the multiplets of optical spectra and the anomalous Zeeman effect. The normal effect had been interpreted by Lorentz on the classical electrodynamic theory. Bohr considered the perturbation due to an imposed magnetic field and was able to deduce an explanation in accordance with the quantum theory, and with the help of the correspondence principle defined the observed components and their polarisation. The anomalous effect is shown when, in a field of moderate strength, originally multiplet

lines further split up into groups of lines for the representation of which groups of terms can be allotted satisfying the correspondence principle. Here again we are faced with half-integral quantum numbers, for as the numbers must differ by unity for a particular group of terms, the magnetic quantum number m must be half-integral for spectral terms containing an even number of m terms, and integral for an odd number of m terms. Finally, to obtain the magnitude of the displacements, m is multiplied by a factor constant for a particular type of multiplet terms. This factor is g, the Landé "splitting factor" or g-formula. The existence of this factor and also the necessity of four quantum numbers instead of three to specify uniquely an electron orbit are instances of theoretical difficulties which will shortly be dealt with in the light of the new mechanics and the notion of the spinning electron. The experiments of Gerlach and Stern on the magnetic moment of atoms by direct observations on the deflections of atoms ejected from boiling metals, under the action of a magnetic field, threw light on a difficulty associated with the Landé splitting factor. This factor has different values for different types of multiplet terms, whereas its value would be unity, and the magnitude of the terms in the anomalous Zeeman effect would be given by m independent of g for a classical interpretation of the moment of momentum in a magnetic field. The necessity for g indicates an abnormal value, either of the magnetic or mechanical moment due to some non-mechanical force or relation between the atomic core and the series electron. The work of Gerlach and Stern indicated the reality of this anomaly and established it as being magnetic and not mechanical.

The Spinning Electron

Towards the end of 1925 a suggestion made a few years previously was developed and applied by two Dutch physicists, Goudsmit and Uhlenbeck.[1] The basis of this new idea was to the effect that an electron should not be regarded only as a structureless point-mass, but that it should have an axis of symmetry about which it could spin, and about which it could have mechanical momentum and magnetic moment. It is assumed that the mechanical momentum can be quantised, and to each electron is assigned an axial momentum of $\frac{1}{2}h/2\pi$. Corresponding to different states of the atom are different orientations of the spin axis relative to the orbital plane, and it can be shown that the energy difference of these states is proportional to the fourth power of the nuclear charge. As we have seen, the anomaly of the Landé splitting factor could

[1] *Naturwissensch*, **17**, p. 953, 1925; *Nature*, **117**, p. 264, 1926.

be explained if some source of magnetic moment of the atom, additional to that due to orbital revolution, could be found, for which the ratio (magnetic moment/mechanical momentum) was different from that due to orbital motion. The spin of the electron just satisfies this requirement, and, moreover, a correct quantitative result requires that the ratio should be twice the ratio corresponding to orbital motion, and this the theory is found to satisfy.

A rule which is of great importance in assigning quantum numbers and in the analysis of complex spectra is given by Pauli's exclusion principle, which states that an electronic orbit is uniquely specified by four quantum numbers, values for which cannot be the same for two electronic orbits ; or in other words, there is only one possible orbit of a given size, shape, and position. Now an electron regarded as a point-mass can have only three degrees of freedom, whereas four degrees of freedom are a necessary consequence of four quantum numbers to specify an orbit. The problem of assigning this extra degree of freedom is overcome by the spinning electron. On the new assumption of a rigid structure the electron has lost its simple character and can no longer be regarded as a point-mass. An extra degree of freedom, that of rotation, is supplied and the difficulty is eliminated.

Among the peculiarities of X-ray spectra is the appearance of so-called screening and relativity doublets. A serious theoretical difficulty was associated with these doublets owing to the fact that their explanation required a mutual contradiction in the allotment of azimuthal quantum numbers for the orbits corresponding to the energy levels concerned, which alternately brought us into conflict with the principle of the application of the relativity separation formula or with the selection and correspondence principles. The spinning electron furnishes an explanation, the relativity doublets being regarded as spin doublets due to a difference in the orientation of the spin axis relative to the orbital plane. A modification is proposed for the explanation of the fine structure of hydrogen-like spectra, the spin now splitting the levels expected in the absence of spin into two, which fall into the places required. Light is thrown on the analogy between optical multiplets and X-ray spectral structure, for the alkali doublets can be regarded as spin doublets and their interpretation by quantum numbers and the application of the correspondence principle follows naturally.

Heisenberg's New Mechanics

The necessity of establishing a fresh outlook on the general problem of atomic processes and of formulating a new method

of mathematical treatment was gradually forced upon physicists by an accumulation of difficulties resulting from the employment of the older theory. The Bohr-Sommerfeld theory, as we have seen, presents us with a picture of the processes underlying the emission of radiation but not of the act of emission itself, the picture being that of one or more electrons jumping between one possible orbit or set of orbits and another. The motions of the system are controlled by the ordinary laws of classical mechanics expressed as Hamiltonian equations and subject to the restriction that not all the orbits allowed by these equations are permissible, but only those subject to certain quantum conditions. The intrinsically unsatisfactory feature of the theoretical position might be described as the lack of a system of mechanics which accorded with the fundamental quantum discontinuity. Our atom model postulated electrons moving in their orbits according to classical laws, but governed in their transitions between orbits by a law involving a discontinuity entirely out of harmony with classical mechanics. It will not be denied that the model was one of great utility, in particular for the interpretation of spectra with the help of the correspondence principle which is based on the harmonic components into which the motion of the atom may be resolved, and regulating the transitions between stationary states. But the fact of having to appeal to these principles was in itself a confession of weakness in calling on the classical theory for results which the quantum theory was of itself unable to supply. The latter as it stood gave us no information about the intensities of the frequencies emitted, and when we come to consider the more complicated types of atoms, the difficulties are further multiplied by the mutual influences of the electrons and by the inherent difficulties of the many-body problem. The further problem of occurrence of non-integral quantum numbers has already been noted. It would seem that the concept of quanta must invade the field of pure mechanics and that modern mathematical methods would come to the rescue of the situation by providing a new system of mechanics which would be a true generalisation, for small-scale phenomena, of classical mechanics with the latter as a limiting case, just as the theory of relativity provided a generalisation of the mechanics of large-scale phenomena.

The work of attempting to formulate a general theory of atomic processes which should provide a means of overcoming these difficulties was initiated by Heisenberg [1] in 1925. A description of the principles and mathematical basis of this work has appeared in these pages [2] and to it the reader is

[1] *Zeits. f. Phys.*, 33, p. 879, 1925.
[2] Flint, *Science Progress*, July 1926, p. 48.

referred. It will therefore be sufficient to recall here, in brief, the nature of the new line of thought. Heisenberg's fundamental postulate is that only such things as are experimentally observable should have a place in mathematical formulation.

We thus virtually abandon our mechanical picture of an atomic model, and such theoretical concepts as positions of electrons, orbital frequencies, and periods which are not subject to experimental observation. Instead it is proposed to devise a means of deriving observable transition frequencies, intensities and polarisations by reference to the stationary states, and not as on the Bohr theory by an application of the correspondence principle to orbital motion. In the general problem of determining the frequencies and amplitudes of emitted radiation Heisenberg started from Kramer's dispersion formula, and it is of interest to note that the classical expression for spectral frequencies involves a differential, while the quantum expression contains a difference, which suggests a clue to the probable necessary modification of classical results. On the classical theory the amplitudes and frequencies of a multiply periodic system can be found by expanding each particular co-ordinate as a Fourier series or one-dimensional set of terms. An atom requires a two-dimensional set of terms made up of quantities associated with possible transitions between stationary states. The simple harmonic frequencies comprising the spectra of the atoms satisfy the frequency condition $E_m - E_n = \nu_{mn}h$. The positional co-ordinate q and the conjugate impulse co-ordinate p can be written in the forms $q_{mn}\, e^{2\pi i\nu_{mn}t}$ and $p_{mn}\, e^{2\pi i\nu_{mn}t}$, which were shown to be two-dimensional sets of terms known as matrices,[1] and represent the harmonic components defining all possible vibrations of the system. Algebraic rules for the purpose of dealing with matrices have been worked out, and it is found that these, in particular the multiplication rule, together with the essential properties of the matrices themselves, are peculiarly suited to the treatment of the atomic problems under investigation, and provide us with a calculus of directly observable magnitudes. If we multiply together two matrices having equal frequencies in corresponding terms, the frequencies of the particular terms in the resulting matrix will be the same as those of the corresponding terms of the two original matrices.

We have, moreover, the now well-known non-commutative multiplication rule which so strikingly allows us to adapt the analysis to quantum requirements and is expressed by the relations $pq - qp = h/2\pi i$.

The solution of our matrix equations is restricted by the

[1] Born and Jordan, *Zeits. f. Phys.*, **34**, p. 858, 1925.

Hamiltonian equation of the system and the above relation to give us ν_{mn} satisfying the Bohr frequency condition, *i.e.* in terms of a suitably assigned set of constants E for the atom, which are, in fact, expressed in terms of a diagonal matrix, which is a matrix function of the q and p, thus ensuring that they shall be characterised by one set of integers.

When the solutions are substituted in the Hamiltonian equations the time factor cancels out in each equation and we have just the equations required to determine the amplitudes and frequencies, one frequency being associated with any particular co-ordinate.

The new theory, providing such a powerful means of attacking an extensive branch of physical problems, naturally attracted the close attention of a very large number of investigators, out of whom the names of Born and Jordan may, perhaps, be selected for special mention in view of their contributions to the firm foundation of essential principles. In collaboration with Heisenberg they extended the theory to systems of several degrees of freedom,[1] and it is noteworthy that the choice of the Hamiltonian function, which controls the whole procedure, necessitates writing for it the same matrix function of p's and q's as would be chosen for their ordinary algebraic function on the old Bohr theory. This and subsequent developments favour the contention that " it is not the equations of classical mechanics that are at fault, but that the mathematical operations by which physical results are deduced from them require modification."

We look to the new theory to resolve difficulties, to which attention has been drawn. In the case of non-integral quantum numbers we can here instance the work of Born and Jordan[2] on the problem of the simple harmonic oscillator. They have shown that the values of E associated with it are $\frac{1}{2}h\nu$, $\frac{3}{2}h\nu$. . . . , and that the only frequencies permitted are those corresponding to the quantum jumps of the older theory. This was shown by Heisenberg in his original paper by less rigorous methods, thus giving the first dynamical theory leading to the half-integral quantum numbers demanded by the theory of multiplets. Another striking feature of the new analysis is that instead of placing us under the necessity of appealing to the correspondence principle to decide on the permissible frequencies out of those possible, the new theory decides this of itself by predicting zero intensities for the absent frequencies. A short survey of the more important achievements in respect of actual atomic problems will be reserved until after we have discussed two further entirely different lines of advance.

[1] *Zeits. f. Phys.*, **35**, p.557, 1926.
[2] *Zeit. f. Phys.*, **34**, p. 858, 1925.

Wave Mechanics

A most suggestive point of view was put forward by de Broglie [1] in his work on wave mechanics, and was to the effect that every mass particle, element of energy, electron, etc., is to be regarded as a singularity of a pulsation which for any given time is in the same phase throughout space. If we transform to a space-time system of reference moving relative to the observer the pulsations will now appear as a phase wave travelling with the velocity c^2/v, which, though in general greater than that of light, does not bring us into conflict with Relativity since only a phase and not a particle is involved. The possibilities suggested in this work were developed by Schrödinger early [2] in 1926 with far-reaching results, and have provided us with a system of wave mechanics which, as in the case of Heisenberg's theory, can be said to have revolutionised quantum mechanics. In Hamiltonian mechanics there is a fundamental analogy between Fermat's Principle of Least Time in optics by which a light-ray path can be traced provided the wave-length of the light is small compared to the dimensions of the space in which it travels and the obstacles which it meets, and Maupertuis' Principle of Least Action in dynamics, by which the paths of material particles can be traced provided the linear dimensions of the paths are large. The wave theory of optics deals with the breakdown of Fermat's principle resulting in diffraction effects, thus geometrical optics can be regarded as a limiting case of wave optics for very small wavelengths. The possibility is suggested that the failure of ordinary mechanics when applied to systems of atomic dimensions is of the same nature as the failure of geometrical optics when applied to such phenomena as diffraction and interference. We had previously no Hamiltonian analogy to the failure of Maupertuis' principle in atomic regions, and it is this analogy between the mechanics of small-scale phenomena and the wave optics of small regions which Schrödinger's theory proposes to supply. He makes the radical assumption that an atom can be represented by a stationary wave in co-ordinate space instead of by a point in space, and proceeds to deduce for the wave function ψ his fundamental equation which takes the form

$$\nabla^2\psi + \frac{8\pi^2 m}{h^2}(E - V)\psi = 0$$

where ∇^2 is the usual Laplace operator, and V the potential energy of the particle of mass m under consideration. It is not here practicable to detail the various assumptions and

[1] *Ann. de Phys.*, **10**, p. 22, 1925.
[2] *Ann. der Phys.*, **79**, p. 361, p. 489, 1926.

steps used in the derivation of this equation, but instead we will consider its properties and interpretation. It has the property that only for certain values of the constant E are there solutions for ψ which are finite, continuous, single valued, and differentiable throughout space. The selection of values of the wave function ψ which shall satisfy these conditions constitutes Schrödinger's "quantisation as a problem of characteristic values," and these values are the eigenfunctions corresponding to a set of discrete values of E, the eigenwerte which are the energies of the stationary states.

In the case of hydrogen $V = -e^2/r$ and ψ can be suitably chosen for all positive values of E, corresponding to a continuous series of hyperbolic orbits, but for a discrete set only of negative values of E given by $E_n = -2\pi^2 me^4/n^2h^2$ determining the Bohr stationary orbits. The practical utility of the method is thus at once illustrated.

Schrödinger extends his theory[1] to the calculation of the intensities of the lines produced by the Stark effect in hydrogen, and shows that fairly satisfactory agreement with the results of experiment is obtained. In addition to the solution of quantum problems in which the potential energy is a function of the co-ordinate alone and does not involve the time explicitly, a modification of the wave equation has been developed allowing V to involve the time explicitly and introducing a time derivative.[2] This permits the new equation to be applied to the problem of the perturbations of a conservative system due to a potential energy function which varies continuously with the time, thus providing a means of dealing with the problem of dispersion. A dispersion formula identical with that of Kramers and Heisenberg is obtained, which involves a term giving the secondary radiation to which the original wave gives rise, and containing the combination frequencies indicated by the Heisenberg analysis; information also is obtained as to the specific conditions corresponding to the radiation of these frequencies.

Dirac's Quantum Algebra

We have now considered shortly two entirely different systems of mechanics, of which the wave theory has the advantage of being the more highly developed branch of analysis. The method of matrices is rather less familiar to the physicist, and the matrix calculations in the more complicated cases are not unattended by difficulties. We now turn to the important contributions made in this country by Dirac,

[1] *Ann. der Phys.*, **80**, p. 437, 1926.
[2] *Ann. der Phys.*, **81**, p. 109, 1926.

whose work may be described as a quantum algebra, which has given valuable assistance in co-ordinating and simplifying the theories of Heisenberg and Schrödinger.

Dirac's initial paper[1] appeared towards the end of 1925, soon after the appearance of Heisenberg's original memorandum, and showed that the quantum conditions for a multiply periodic system can be expressed in terms of a quantity analogous to a certain expression known in classical mechanics as a Poisson Bracket expression (hereafter referred to as a P.B. expression). An investigation is made of the fundamental theorems which are required for application to a dynamical system to take the place of the classical theorems, and are based on the non-commutative multiplication rule of two quantum variables. The relations which are to be taken over from the classical theory are defined, and the quantum conditions are established which must be satisfied by the canonical variables of the system. It is assumed that the equations of motion can be taken over, and in conformity with the non-commutative rule we can take over any equation deducible from the equations of motion by any process not involving interchange of factors of a product. It is found that the most general operation analogous to differentiation which can be performed on a quantum variable is to take the difference of its Heisenberg product with some other quantum variable. Dirac then inquires to what this difference, *i.e.* an expression of the form $(xy - yx)$ corresponds on the classical theory, and arrives at the fundamental relation that the difference between the Heisenberg product of two quantum variables is $ih/2\pi$ times their P.B. expression $[x,y]$, thus we may write

$$xy - yx = \frac{ih}{2\pi}[x,y]$$

The properties of the classical P.B. allows the deduction of the conditions which must be satisfied by the canonical variables of a system, and these are summarised in the equations

$$\left.\begin{aligned} q_r q_s - q_s q_r &= 0 \\ p_r p_s - p_s p_r &= 0 \\ q_r p_s - p_s q_r &= 0 \text{ if } r \neq s \\ q_r p_r - p_r q_r &= ih/2\pi \text{ (i.e. } r=s) \end{aligned}\right\}$$

which are equivalent to Heisenberg's conditions.

An interesting result of the non-commutative property of such variables is that they cannot be numbers in the ordinary mathematical sense, and hence Dirac proposes to call them *q*-numbers as opposed to ordinary numbers which by distinction may be termed *c*-numbers; thus as we have no definite picture

[1] *Proc. Roy. Soc.*, A. **109**, p. 642, 1925.

of the nature of q-numbers we must suppose that when appearing in the solution of a quantum problem they are to be regarded as appearing analogously to a c-number variable in classical theory, while the interpretation of physical results must be made in terms of c-numbers. Dirac applies his theory to the study of practical problems, and dealing in this manner with the orbital motion of the electron in the hydrogen atom [1] he is able to arrive at the well-known formula for the Balmer terms of the hydrogen spectrum. This has been arrived at independently by Pauli using the Heisenberg matrices.[2]

A closely reasoned development of the established principles allows of the extension of the theory to more complicated problems, and it must be sufficient here to summarise Dirac's outstanding results. The investigation of a number of electrons moving in a central field of force [3] allowed the calculation of the necessary half-integral quantum numbers corresponding to the various series of spectroscopic terms, as well as the formula for the energies of the stationary states, and the relative intensities of multiplet lines and their components in a weak magnetic field. Dirac also attacks the problem of relativity quantum mechanics [4] by adopting the principle of introducing canonical variables into the classical theory and reformulating it with P.B.'s in place of differential equations, he also for this investigation assumes that the time, t, may be treated as a q-number variable. This extension to the mechanics of moving systems enables him to deal with the well-known Compton effect. It will be recalled that when X-rays are scattered by a thin layer or plate the scattered radiation undergoes a lowering of frequency. According to Compton's light-quantum theory of scattering, an X-ray quantum of energy $h\nu$ and momentum $h\nu/c$ collides with an electron at rest and recoils at an angle with its original direction and with diminished energy and momentum, while the electron acquires a velocity and recoils in accordance with the laws of conservation of energy and momentum; the frequency of the X-ray quantum is therefore diminished. Dirac obtains results for the frequency and intensity of the scattered radiation in good agreement with Compton's theory. The theory of the effect has also been successfully treated independently by Schrödinger.[5] The anomalous Zeeman effect has, with the help of the spinning electron, yielded satisfactory quantitative results (including a deduction of the Landé g-formula) in the hands of Heisenberg

[1] *Proc. Roy. Soc.*, A. **110**, p. 561, 1926.
[2] *Zeits. f. Phys.*, **36**, p. 336, 1926.
[3] *Proc. Roy. Soc.*, A. **111**, p. 281, 1926.
[4] *Proc. Roy. Soc.*, A. **111**, p. 405, 1926.
[5] *Ann. der Phys.*, **82**, p. 257, 1927.

and Jordan,[1] who have also brought the calculation of the spin doublets within the range of the analysis. The former has made a systematic attack on the problem of several bodies[2] for the elucidation of the more complex atomic systems, and for the representation of the essential characteristics of spectra. His application of the problem of the two-electron system solved the well-known difficulty of the ortho and para terms of the spectrum of neutral helium,[3] while it has also been extended to the theory of molecules and their band spectra.[4] Heisenberg's resonance theory has been applied by Foster[5] to a theory of the Stark effect in helium. The quantum treatment of impact processes has been attacked by Born,[6] with the help of the wave equation, and a solution found for the combination atom electron, from which transition probabilities can be determined.

One of the most striking features of lines of progress so fundamentally different as the matrix and the wave theory is their essential equivalence. Dirac, as we have remarked, with the help of his symbolic algebra deduced a simplified means of calculating the Heisenberg matrices.[7] It is possible only to give the briefest outline of the method adopted, which was to introduce, with the help of his fundamental postulate, differential operators into Schrödinger's equation. Any constant of integration of the dynamical system was shown to be represented by a matrix, while to obtain Heisenberg's unique matrices the eigenfunctions were so chosen as to make the matrix representing the total energy a diagonal matrix. The solution of a problem in Heisenberg's mechanics furnishes a multiplicity of matrix schemes satisfying Dirac's quantum conditions, while the physical interpretation of a q-number requires that it should be expressed as a diagonal matrix whose diagonal terms are the characteristic values of the q-number. These values are c-numbers and can therefore be compared with experimental results. Dirac deduced general transformation equations[8] between two matrix schemes and from these showed, in particular, how it is possible to find the matrix scheme which makes any function of the dynamical variables a diagonal matrix. We may now summarise a few of the results set out in Dirac's most recent papers, bearing in mind certain topics which were previously mentioned as requiring

[1] *Zeits. f. Phys.*, **37**, p. 263, 1926.
[2] *Zeits. f. Phys.*, **38**, p. 411, 1926.
[3] *Zeits. f. Phys.*, **39**, p. 499, 1926.
[4] *Zeits. f. Phys.*, **41**, p. 239, 1927.
[5] *Proc. Roy. Soc.*, A. **117**, p. 137, 1927.
[6] *Zeits. f. Phys.*, **37**, p. 863; **38**, p. 803, 1926.
[7] *Proc. Roy. Soc.*, A. **112**, p. 661, 1926.
[8] *Proc. Roy. Soc.*, A. **113**, p. 621, 1927.

interpretation in terms of the new mechanics. Pauli's exclusion principle has been confirmed [1] for two electrons ; a theory of emission and absorption of radiation has been advanced,[2] and the Kramers-Heisenberg dispersion formula has been deduced.[3] It was shown that by the correct application of the q-number theory to a point-charge electron the results previously obtained from the spinning electron could be deduced naturally without actually assuming a spin, although it was not necessary to abandon this conception.[4] The beautiful feature of the new theories taken as a whole is that so many of the empirical principles of the older theory find a natural outcome, and are deduced just as they are required without the need of arbitrary assumptions or of postulating them first and justifying them afterwards. Unfortunately we appear to be no nearer a mechanical picture of the fundamental nature and workings of the quantum, which seems to be intimately connected with the very existence of electric charges and the characteristics of the electro-magnetic field. The new analysis is symbolic in nature and, as we have seen, involves essentially the use of imaginary quantities.

Touching on philosophical aspects it may be remarked that the difficulties of a causal space-time description of quantum processes is closely connected with the important part played by statistical mechanics in recent developments. The principle of causality in physics takes the form that the future motion of a system can be determined if its state at any instant is known, while successive observations of the latter render our knowledge of the former more accurate. Heisenberg, recently reviewing [5] the general relationship of quantum theoretical postulates and the principles of experimental observation has shown that a reciprocal uncertainty affects all measurements of atomic quantities, and has deduced an expression for the maximum precision with which the space-time co-ordinates and the momentum-energy co-ordinates can be measured simultaneously. On quantum considerations the impossibility of neglecting the influence and interaction of the agency and the measurement introduces a new uncontrolled element into every observation. The characteristic inexactness with which canonical conjugate quantities[4] can be determined simultaneously is the particular basis for the appearance of statistical connections in quantum mechanics. From the fact that the electrical energy of a system can ordinarily be expressed as

[1] *Proc. Roy. Soc.*, A. **112**, p. 661, 1926.
[2] *Proc. Roy. Soc.*, A. **114**, p. 243, 1927.
[3] *Proc. Roy. Soc.*, A. **114**, p. 710, 1927.
[4] *Proc. Roy. Soc.*, A. **117**, p. 610 ; **118**, p. 351, 1928.
[5] *Zeits. f. Phys.*, **43**, p. 172, 1927.

a volume integral Schrödinger made the ingenious proposal [1] of interpreting $\psi \bar{\psi}$ where $\bar{\psi}$ is the conjugate of ψ as the electric volume density, thus replacing the electrons in the atom by an electric density. Statistically this may be utilised as a measure of the probability for electrons being present within given space regions of the atom. In terms of the wave theory the Bose-Einstein statistics may be regarded as a justification for regarding a gas as a system of vibrations. A knowledge of the wave function, however, for an atomic system enables us to follow the course of a physical process so far as it is quantum mechanically determinate, not in a causal but in a statistical sense.

Rapidly as the new mechanics is progressing we are far from any suggestion of finality. The ultimate aim must be the welding of the many-sided aspects of the laws of mechanics into a coherent and perfectly generalised system.

[1] *Ann. der Phys.*, **80**, p. 476, 1926.

PROTEIN METABOLISM AND ORGANIC EVOLUTION

By JOSEPH NEEDHAM, M.A., Ph.D.
Fellow of Caius College, Cambridge, and University Demonstrator in Biochemistry

In 1908 Keith Lucas [37] deplored in this journal the lack of contact between physiological thought and the theory of evolution. He rightly felt that the study of the evolution of function held within itself the promise of great advances in biology, advances for which we should wait in vain if we adhered to the outlook generally permeating physiological laboratories. Too many physiologists and biochemists are provincial in time, and spare little thought for the evolutionary development of the functions with which they are dealing. The admirable researches of McCallum on the " palæo-chemical ratios " in animals and of Barcroft and his associates on the evolution of the functions of blood have done much to remove this reproach from physiology, but biochemists have hardly begun to take a comparative and historical view. Perhaps the suggestion that closer relations should exist between not only zoology and biochemistry, but between palæontology and biochemistry is obscure without an illustration. If this is so, I hope that the present paper will give an instance of the fruitfulness of their contact, for it aims at discussing in this way the problem of nitrogen excretion in the animal kingdom. One of the questions always asked by students in biochemistry is why some animals should excrete urea, some ammonia, some uric acid. As far as I know they have not so far been accustomed to receive any reasonable reply, and the problem has been set down as one of those arbitrary dispositions of fate which make the elementary class despair of biochemistry. I hope to show here that an answer can be given.

Fiske and Boyden [17], in their memoir on the nitrogen metabolism of the hen's egg, raised an interesting point when they calculated that 15 per cent. of all the water in the egg at the beginning is needed to excrete the 5 mgms. odd of uric acid which are present in the allantoic liquid by the eleventh day of development. From that time onwards reabsorption of water vigorously proceeds, no doubt for the reason that without

it all the water in the residues and in the body of the embryo would be required to get rid of the uric acid that is to be formed. It is as if the water acted as an endless belt conveyor, transferring uric acid from the cells of the embryo into the allantoic liquid and then returning to transfer more. The fowl is always good at absorbing water from its excretions, for as Wiener [65] and Sharpe [60] have shown, the glomerular urine in the adult is quite liquid and the cloaca absorb great quantities of water. All terrestrial animals do this to some extent if the views of Cushny [8] about the function of the mammalian kidney tubules are correct. But in the hen's egg it is obvious how closely the process as a whole is bound up with the properties of uric acid. " A substance as soluble and diffusible as urea," say Fiske and Boyden, " could not possibly replace it as an end-product when the organism and its excretions are confined to a closed system, the walls of which are only permeable to matter in the gaseous state."

This is a very important consideration. There appear to be only three substances which are available in the animal kingdom for carrying away the nitrogenous waste resulting from protein breakdown—ammonia, urea, and uric acid. The first two of these compounds are very soluble and diffusible ; uric acid is not. Quantitative expression of this fact has been given by Chauffard, Brodin, and Grigaut [7], who found a dialysis coefficient of 93 for urea but only 74 for sodium urate. The hæmatoencephalic barrier, according to them, allows urea to pass easily but not uric acid. Shut up as it is in its closed box, the chick embryo would evidently find uric acid by far the most convenient excretory product, for the two former would tend to diffuse throughout the egg and to establish themselves in equal concentration in all its constituent regions, instead of being packed into a small store. As it happens, we find in the work of Kamei [33] a striking verification of this view-point, for he has recently shown that in the amniotic liquid of the chick, although the uric acid concentration never rises above a certain very low level, the ammonia and the urea rise continuously throughout development. It is easy to guess, therefore, what would happen if all the nitrogen excreted by the embryo were in the form of urea. As an illustrative calculation we may take the uric acid present in the allantois at the end of incubation as 100 mgms. (data of Fiske and Boyden [17], Needham [43], Targonski [62], and others)—*i.e.* about 33 mgms. of uric acid nitrogen or 66 mgms. of urea. This, distributed over an egg of contents approximately 40 gms. would be 165 mgms. per cent.—a definitely pathological figure. The egg would be " uræmic," in the strict sense of the word. The normal figure for the urea-content of human and bovine

blood is about 14 mgms. per cent. and the highest figure on record obtained by ingesting solid urea is just under 100 mgms. per cent. In severe renal obstruction or nephritis, it rises above 100 and may reach 300 or 400, but 165 is undoubtedly of the pathological order of magnitude, and if the avian embryo had to suffer from a constant headache and other symptoms before hatching, natural selection would hardly have preserved it for our entertainment. These consequences can be avoided by the use of uric acid.

Such considerations lead to the suggestion that the form of excretion of nitrogen adopted by an animal depends principally on the conditions under which its embryo has to live. There is much evidence that the combustion of protein substances as a source of energy is more marked in aquatic than in terrestrial embryos (Needham [45]). Table I, constructed

TABLE I.

—	Material burned as source of energy in per cent. of the total material so burned.		
	Carbohydrate.	Protein.	Fat.
TERRESTRIAL :			
Gallus domesticus (hen)	3·02	5·57	91·4
Murray [40]			
Needham [42]			
AQUATIC :			
Rana temporaria (frog)	6·84	70·70	22·4
Barthélémy and Bonnet [3]			
Fauré-Fremiet and Dragoiu [15]			
Bialascewicz and Mincovna [4]			
Needham [45]			
AQUATIC :			
Savalinus fontinalis (brook-trout) .	—	63	37
Gortner [23]			
TERRESTRIAL :			
Bombyx mori (silkworm)	—	10	64
Tichomirov [63]			
Farkas [14]			
AQUATIC :			
Pleuronectes platessa (plaice) . .	—	90	—
Dakin and Dakin [9]			

The figures above the middle line are those most accurately known.

from as much of the information as is trustworthy, shows the partition between the substances comprising the total material catabolised. Thus only 5 or 6 per cent. of the total matter combusted by the chick embyro is protein, but the frog embryo combusts as much as 71 per cent. during its embryonic life. Everything points to very deep-seated differences between eggs which develop in the water and eggs which develop on

land. Not only do aquatic embryos burn much more protein in per cent. of the total material burned ; but also in per cent. of the initial store of protein. Table II shows this very clearly. The embryos of the chick and the silkworm are the only terrestrial ones for which we have dependable figures, and they agree in burning about 4 per cent. of their inital store of protein.

TABLE II

Animal.	Investigator.	Aquatic or terrestrial embryo.	Protein nitrogen combusted during development in per cent. of the total protein nitrogen present at the beginning.		
PISCES :					
Brook-trout .	Gortner [23]	A	To hatching	3·4	
	,,		To end of yolk-sac period		21·9
	Pearse [50]		To end of yolk-sac period		17·0
Plaice . .	Dakin and Dakin [9]	A	To end of development		18·3
AMPHIBIA :					
Frog . .	Barthélémy and Bonnet [3]	A	To disapp. of ext. gills		25·7
	Bialascewicz and Mincovna [4]		To hatching	9·1	
			To end of yolk-sac period		40·0
	Fauré-Fremiet and Dragoiu [15[		To hatching	10·6	
			To end of yolk-sac period		23·1
	Fauré-Fremiet and du Streel [16]		To hatching	9·2	
Salamander .	Gortner [24]	A	To hatching	4·9	
INSECTA :					
Silkworm .	Russo [55]	T	Whole development		3·9
AVES :					
Chick . .	Idzumi [32]	T	Indirect calcs.		5·8
(All figures are for the whole of development)	Sakuragi [56]	T	By protein lost		8·0
	Needham [44]	T	By end-products found		1·1
	Fiske and Boyden [17]	T	By end-products found		3·4

Among aquatic embryos, the frog, the trout, and the plaice agree in burning about 25 per cent.—about six times as much. In the case of embryos which hatch only half-way through their development, as most of the aquatic ones do, it is interesting to find that up to hatching their protein utilisation is not high, but that for the whole embryonic period it undoubtedly much exceeds that of terrestrial embryos. Thus there is reason for supposing that the terrestrial environment of the embryo has two effects on its protein metabolism : firstly, to suppress the

production of nitrogenous waste by removing the means of its easy disposal, and secondly, to elevate uric acid to the place of importance as a means of excreting nitrogen. From this point of view, the invention of viviparity was a " Back-to-the sea " movement on the part of the embryo, for even if, as McCallum [38] would have us believe, the maternal sea-water is practically pre-Cambrian, it is at any rate as good as any other sea-water for the disposal of nitrogenous waste-products, and from the embryonic point of view, a boundless ocean. In other words, the continuous perfusion system of the vivipara provides an artificial sea and avoids the necessity of a uricotelic metabolism. Is it surprising, in view of these facts, that fishes turn the ammonia from their protein breakdown into urea, birds and reptiles into uric acid, and mammals once more into urea ?

The thought may be stated in another way. Perhaps the sauropsida excrete their nitrogen mainly as uric acid because they had to learn how to do so in order to pack their embryos into solid- and liquid-tight boxes, and never afterwards forgot. Even the eggs of water-birds, which might be supposed to have the opportunity of excreting substances into the water around them, have impenetrable fat-impregnated shells, as Loisel [36] has shown. The highest avian groups, exemplifying as they do the most complicated form of nitrogen excretion, would thus represent the crowning achievements of the uricotelic line of evolution. And the fact that between the second and fifth days in the chick's development it excretes ammonia and urea with no uric acid, would thus be a recapitulation of its pre-terrestrial or aquatic ancestry, entirely analogous with its gill-clefts. Moreover, the coincidence is exact, for it is just between the second and fifth days that the embryo manifests its morphologically piscine characteristics. Fig. 1 shows the mgms. of ammonia, urea, and uric acid, present in embryo, amniotic, and allantoic liquid throughout incubation, expressed in terms of 100 gms. dry weight of embryo ; in other words, it shows what 100 gms. dry weight of embryo has manufactured in the way of nitrogenous end-products by any given time. Table III shows the relations between these substances in another way. Ammonia, urea, and uric acid are excreted by the chick embryo during its development, but the two first-named molecules only account for an insignificant part of the total nitrogen which it excretes. It is as if it attained its adult habits very early in development. As Table III shows, there is a progression in ontogeny from the smallest to the largest molecule, and from the most to the least efficient excretory product. And as had already been suggested, the whole sequence bears a recapitulatory stamp. It could be argued, of course, that if the chick can excrete urea early on in its

development, why should it not return to this practice after hatching; but this would be to neglect one of the most characteristic features of embryonic life, namely its continual tendency

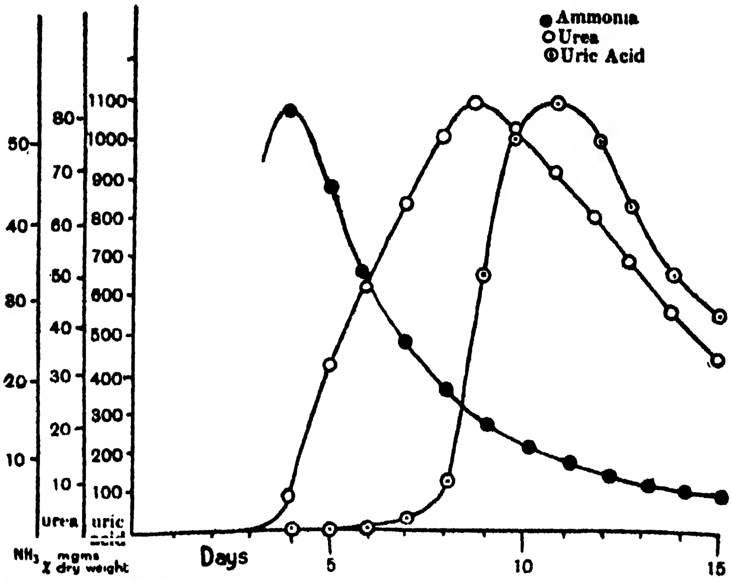


FIG. 1.—Relative intensity of production of nitrogenous waste-products by the chick embryo. (Needham [43].)

to lose totipotence and to move towards a stable, "crystalline," or set state. The chick embryo begins to excrete urea about the sixth day. It has recently been demonstrated by Przyleck and Rogalski [54] that before that time it possesses uricase, but after that time it does not. Perhaps, then, when the reptiles came ashore they found it was as well to leave their uricase behind them. But the presence of uricase is very erratic among the marine invertebrates, and our knowledge of its occurrence in amphibia and in reptiles is too limited to allow further conjectures in this direction.

TABLE III

	Molecular weight of the compound.	Percentage of nitrogen in the compound.	Time of peak of maximum production in the chick's development in days.	Absolute mgms. of nitrogen excreted during the whole of the chick's development.	Percentage of the total nitrogen excreted during the whole of the chick's development.
Ammonia .	17	82·3	4	0·120	1·07
Urea .	60	46·6	9	0·843	7·58
Uric acid .	168	33·3	11	10·161	91·35
				11·124	

There remains the consideration that no animal would excrete uric acid as its main nitrogenous end-product unless it was driven to it, for of the three in actual use it is much the most wasteful. Ammonia is clearly the most efficient end-product, for it involves no wastage of carbon, but of the other two, the carbon : nitrogen ratio is 1 : 2 in urea and 1 : 0·9 in uric acid. In other words, two atoms of nitrogen can be got rid of at the expense of only one carbon atom in urea, but only 0·9 in uric acid. It is just the same with oxygen. Each nitrogen atom takes away 2·2 atoms of oxygen with it in uric acid and only 1·5 in urea. These amounts may not be individually considerable, but collectively they may make all the difference between an efficient and an inefficient species. Uric acid, then, as the main end-product of protein metabolism, may be said to be more ingenious than the other two, but less efficient.

The proposition that the circumstances in which the embryonic life has to be passed ultimately govern the form in which the nitrogen is excreted, is thus not so far-fetched as it sounds. During the last fifty years much attention has been paid to the comparative study of nitrogen-excretion, but the methods of the older workers such as Krukenberg and Griffiths were so unreliable that the earlier literature may be neglected. More recently, the researches of Przylecki [53], Delaunay [11], and others have begun the erection of a solid structure of knowledge about the forms in which nitrogen is excreted. We are thus acquiring, as it were, a wide series of phylogenetical base-lines on which ontogenetic phenomena can be superimposed. The general conclusions of these workers agree strikingly with the idea of an association between aquatic life and the excretion of ammonia and urea on the one hand and between terrestrial life and the excretion of uric acid on the other hand. But the important point is that it is the life of the embryo that is the key, not the life of the adult. An animal may live all its life in the sea, but if its eggs are laid and develop on land it may be predicted that its main nitrogenous end-product will be uric acid. Mammals, from the chemico-embryological viewpoint, count as aquatic animals since the excretion of nitrogenous waste-products through the placenta is analogous to their excretion into water.

In Table IV are collected together a number of figures for nitrogen excretion in various animals. All the older work has been excluded and as far as possible only quantitative investigations of the percentage distribution of the excretory nitrogen appear. As a general rule the marine invertebrates excrete most of their nitrogen as ammonia—a simple and easy procedure considering their environment. But with the increasing

TABLE IV

QUANTITATIVE DATA FOR NITROGEN PARTITION IN URINE

Animal.	Aquatic or Terrestrial Egg.	Nitrogen Partition in per cent. of the total nitrogen excreted. Ammonia.	Urea.	Uric acid.	Amino-acids, creatine and creatinine.	Purines other than uric acid.	Investigator.
ANNELIDA:							
Aphrodite aculeata (sea-mouse)	A	80·0	0·2	0·8	—	—	Delaunay [11]
Hirudo officinalis (leech)	A	76·4	5·4	None	3·2	3·6	Delaunay [11]
Lumbricus agricola (earthworm)	T	20·4	38·1	Trace	15·8	9·3	Delaunay [11]
GEPHYREA:							
Sipunculus nudus (worm)	A	50·0	9·7	None	16·6	4·1	Delaunay [11]
ARTHROPODA:							
CRUSTACEA:							
Carcinus mænas (crab)	A	67·8	2·9	0·7	8·7	2·3	Delaunay [11]
Maia squinado (crab)	A	42·9	5·2	2·7	20·2	3·5	Delaunay [11]
Astacus fluviatilis (crayfish)	A	59·6	11·2	0·8	10·1	3·7	Delaunay [11]
INSECTA:							
Bombyx mori (silkworm)	T	—	None	85·8	—	—	Farkas [14]
In general	T	"Als characterisches Stoffwechselendprodukt des Insektenorganismus ist die Harnsäure zu betrachten" (von Fürth, p. 295).					

MOLLUSCA:							
GASTROPODA:							
Aplysia limacina (sea-hare)	A	33·5	8·7	4·6	13·0	9·3	Delaunay [11]
Helix pomatia (land-snail)	T	13·7	20·0	10·7	6·0	5·8	Delaunay [11]
Limax agrestis (slug)	T	4·6	70·8	6·9	1·7	5·9	Delaunay [11]
LAMELLIBRANCHIATA:							
Mya arenaria (clam)	A	21·5	4·5	Trace	18·0	5·0	Delaunay [11]
Gryphaea angulata (oyster)	A	7·3	3·2	0·2	—	—	Delaunay [11]
Anodonta cygnaea (pond-mussel)	A	63·0	—	—	—	—	Przylecki [53]
CEPHALOPODA:							
Octopus vulgaris (squid)	A	33·3	—	1·4	12·5	23·6	Delaunay [11]
		41·7	15·0	—	20·7	—	Von Fürth [20]
Sepia officinalis (squid)	A	67·0	1·7	2·1	7·8	1·9	Delaunay [11]
ECHINODERMATA:							
Asterias rubens (starfish)	A	39·3	11·7	Trace	23·8	6·8	Delaunay [11]
Paracentrotus lividus (sea-urchin)	A	28·1	7·5	1·0	28·0	10·0	Delaunay [11]
Holothuria tubulosa (sea-cucumber)	A	40·0	6·0	Trace	—	—	Delaunay [11]

TABLE IV—*continued*

Quantitative Data for Nitrogen Partition in Urine

Animal.	Aquatic or Terrestrial Egg.	Nitrogen Partition in per cent. of the total nitrogen excreted.					Investigator.
		Ammonia.	Urea.	Uric acid.	Amino-acids, creatine and creatinine.	Purines other than uric acid.	
VERTEBRATA							
Pisces :							
Lophius piscatorius (goosefish) . . .	A	13·0	62·2	0·1	—	—	Denis [12]
Mustelus canis (dogfish) . . .	A	7·3	80·7	0·2	—	—	Denis [12]
Scyllium canicula (dogfish) . . .	A	—	80.0	0.0	—	—	Herter [27]
Amphibia :							
Rana temporaria (frog) . . .	A	15·0	82·0	Trace	—	—	Przylecki [53]
Rana virescens (frog)	A	3·2	84·0	0·4	—	—	van der Heyde [28]
Bufo vulgaris (toad)	A	—	84·5	Trace	—	—	Burian [6]
Reptilia :							
Alligator mississipiensis (alligator) .	A	75·0	3·0	14·0	—	—	Hopping [29]
Chelone mydas (marine turtle) . .	A	16·1	45·1	19·1	—	—	Lewis [35]
Phyton (tropical snake) . . .	T	—	—	80·0	—	—	Boussingault [5]
Boa (tropical snake)	T	—	—	80·0	—	—	Boussingault [5]
Tropidonotus natrix (grass-snake) .	T	Trace	Trace	80·0	—	—	Girod [22]
Lacerta viridis (lizard) . . .	T	—	—	91·0	—	—	von Schreiber [59]
Aves :							
Gallus domesticus (hen)	T	1·5	0·9	70·0	—	—	Salaskin and Kovalevski [57]
		—	1·1	62·9	—	—	Minkovski [39]
		17·3	10·4	62·9	9·4	—	Davis [10]
Cygnus (swan)	T	15·8	2·6	68·7	—	—	Salaskin and Kovalevski [57]
Anas (duck)	T	3·2	4·2	71·9	—	—	Szalagyi and Kriwuscha [61]
Anser (goose)	T	13·5	—	80·0	—	—	Paton [49]

Mammalia:							
Homo sapiens (man)	A	4·3	87·5	0·8	—	—	Folin [18]
Canis vulgaris (dog)	A	3·0	89·0	1·0	—	—	Osterberg and Wolf [48]
Felis vulgaris (cat)	A	4·9	68·1	0·1	—	—	Hammett [26]
Toxidia texus (badger)	A	—	—	8·0	—	—	Hunter, Givens, and Guion [30]
Procyon lotor (raccoon)	A	—	—	3·0	—	—	Hunter, Givens, and Guion [30]
Cavia (guinea-pig)	A	—	—	3·5	—		Hunter, Givens, and Guion [30]
Mus rattus (rat)	A	—	—	5·0	—	—	Hunter, Givens, and Guion [30]
Didelphys virginiana (opossum)	A	—	—	1·5	—	—	Hunter, Givens, and Guion [30]
Equus (horse)	A	—	—	3·2	—	—	Hunter, Givens, and Guion [30]
Ovis (sheep)	A	—	—	3·3	—	—	Hunter, Givens, and Guion [30]
Balæna mysticetus (whale)	A	1·5	90·0	3·0	—	—	Schmidt-Nielsen and Holmsen [58]
	A	3·5	91·0	0·3	1·5	—	Ichimi, and his associates [31]
Xantharpyia collaris (Egyptian bat)	A	0·6	77·8	1·2	—	—	Popp [52]
Camelus bactrianus (camel)	A	0·5	97·1	2·4	—	—	Petri [51]
(Weasel)	A	0·9	85·2	0·2	2·1	0·6	Fuse [21]
(Tiger)	A	3·7	89·1	0·04	1·9	0·2	Fuse [21]
(Hyæna)	A	3·8	89·3	0·10	1·5	0·2	Fuse [21]
Monotreme:							
Echidna aculeata (spiny anteater)	—	6·9	81·2	None	—	—	Neumeister [47]

complexity of the body, ammonia excretion disappears, for it is incompatible with a kidney, even in a very undeveloped form. Excretory structures—structures which have to live, as it were, in an excretory atmosphere—cannot deal with highly alkaline liquids, and the great disadvantage about simple ammonia excretion is that a constant supply of acid is required to neutralise it. This acid is nothing but waste, and so among the marine invertebrates themselves we see urea superseding ammonia. Among the invertebrates the only ones at present known which have a high percentage of uric acid are the pulmonate gastropods, the snail and the slug, which live on land and have terrestrial embryos. Delaunay himself pointed out that the invertebrates could be separated into an aquatic and a terrestrial group, the former excreting much ammonia and the latter little, and he also remarked on the association between uric acid and terrestrial life. But this might remain enigmatic if we did not consider the needs of the *embryo* ; able, in the one case, to get rid of its nitrogenous waste easily into the surrounding water, and forced, in the other case, to keep it close at hand in very restricted quarters. Delaunay's generalisation alone would not explain the mammals. It is the conditions under which the *embryo* has to live that govern what form of nitrogen shall be excreted throughout the life-span. As Table IV shows, the fishes and mammals (even the whale) with their urea and the birds with their uric acid, fit in with the theory here propounded. The insects also are in perfect accord, for although we have no quantitative data concerning the nitrogen partition of their urine, yet it has been generally recognised for many years that uric acid is the most prominent constituent of their excreta (see von Fürth [19]) and there is doubt if urea has ever even been shown to be present. A quite parallel case is that of the hymenoptera and diptera which excrete uric acid during metamorphosis into their fat-bodies, according to Fabre [13]. Thus the insects, coming to live on land earlier than the reptiles, had the same chemico-embryological problem to face, and solved it in the same way. The reptiles themselves form an interesting group in Table IV, for the two aquatic ones, the crocodile and the green turtle, show a high ammonia and urea excretion, while the snakes and the lizard, with their terrestrial embryos, show a high uric acid excretion. It is, of course, among the reptilian group that such a difference would be likely to show itself, as they were the first vertebrates to conquer the land.

The eggs of aquatic animals seem to divide into two classes. The frog and the plaice develop within membranes which readily allow the nitrogenous end-products to escape ; the trout and Ascaris [34] do not. But hatching always occurs long

before the end of development in these aquatic forms, so that the permeability of the egg-membranes is unimportant from the point of view of nitrogen excretion. The trout at hatching gets rid of what has accumulated and for the rest of its embryonic life can excrete directly into the water. The elasmobranchs, which excrete their urea into their yolk (Needham and Needham [46]), would form a third class; but they are notable exceptions in many ways, and to describe their behaviour in terms of the language here used would be to say that they use their own blood and yolk as the sea, and pile up the end-products of their protein metabolism within themselves.

The amphibia never broke loose from the fishes; they always retained a piscine larval stage and laid their eggs in water. But when the first reptiles left the sea, they were faced with one or two very difficult embryological problems. To begin with, they had to find out how to abandon metamorphosis; but, that accomplished, they had to discover a way of arranging a water-supply for their embryos. As Gray [25] has shown, aquatic embryos always depend upon their environment for a supply of water; in other words, the fertilised egg contains enough solid but not enough water to make the finished larva. The first terrestrial eggs, therefore, had to contain enough water as well as enough solid, and as arrangements to prevent undue evaporation were essential, the closed-box system inevitably developed. The mechanism by which a constant pressure-head of water was provided in the terrestrial egg, namely, the egg-white, can be seen functioning at the present time in the as yet unidentified acid which, introduced by the embryo's metabolism into the egg-white as Vladimirov [64] has shown, gradually brings the latter to its isoelectric point and liberates water by degrees from the colloidal albumen. All the economy of the successful terrestrial egg had to be directed towards conserving the water, and while a great bath would have been required to keep the urea concentration down within bearable limits, if all the nitrogen was excreted in that form, only 20 per cent. of the water in the egg need be set aside for handling uric acid. Another way out of the difficulty would have been to burn no protein at all, and therefore to avoid all incombustible residues, and it is possible that some of the extinct saurians explored the possibilities in this direction; but I suspect that some factor which as yet we cannot quite define dictates from within the cells that life without protein combustion is not possible. It is therefore likely that such reptilian experiments did not proceed very far. In this way the closed-box system with its partial suppression of protein metabolism and its uricotelic qualities came into being.

There is another aspect to the need for water which was

perhaps the main problem of the first terrestrial embryos. Babcock [2] and others have calculated that 100 gms. of fat on combustion yield 107·1 gms. of water, 100 gms. of carbohydrate 55·5 gms. of water, and 100 gms. of protein only 41·3 gms. of water. From Table I it is clear that terrestrial embryos burn much more fat than aquatic ones (average of 80 per cent. of the total foodstuff combusted as against 20 per cent.), and this again may be regarded as a consequence of their special needs. Energy is the same, no doubt, from whatever source it comes, but one source may be more convenient than another. In the hen's egg, according to Murray's data [41], 6·32 gms. of water are lost from the system during development, and 2·01 gms. of solid as Co_2. As 91 per cent. of the solid lost is fat, about 2·1 gms. of "metabolic water" are added to the egg, so that 3·4 gms. of water would have been lost if fat had not been burnt, or 40 per cent. more than what actually is lost. The chick in its closed box cannot afford to despise this extra two grams of metabolic water.

No doubt the first terrestrial vertebrates laid their eggs in water, and probably, like the trout, they hatched early. But although this system may suffice in the sea, an embryonic reptile with a bag of yolk almost as big as itself would have, owing to obvious difficulties of locomotion, very little chance of survival on land. Such imperfectly mobile eggs would have been much too tempting for adults of other species. The terrestrial egg had therefore to be constructed in such a way that the young organism could stay inside a long time and hatch out substantially mature. No chance, therefore, either to get rid of nitrogenous excreta at an early stage by hatching, or to excrete them through a semipermeable membrane. The only solution of the problem was uric acid. When, at a still later date, the prototheria or metatheria branched off from the reptiles in the mammalian direction and first acquired true viviparity, the need for a uricotelic metabolism ceased.[1] For it is to be noted that there is no reason why an adult land animal should not excrete urea, if it drinks sufficient water, or even ammonia, if it has enough acid to spare. Thus Ambard [1] found that a cat or a dog on a meat diet, if left to itself, drinks exactly enough water to excrete urea at its maximum normal concentration, *i.e.* just to avoid the slightest uræmia. This is Ambard's "volume obligatoire." But what the adult does will depend on what the embryo had to do; in other words, on what its facilities then were for absorbing water and for getting rid of waste nitrogen. The existence of an albuminous solution round the yolk of the terrestrial egg is, as Gray

[1] The urine of Echidna has a typically mammalian nitrogen partition (see Table IV).

says, an admirably adapted mechanism for providing the growing embryo with water. The use of uric acid—insoluble, non-diffusible—instead of urea or ammonia, is an equally well-adapted mechanism for dealing with incombustible waste, and the reabsorption of water through the allantoic wall is the mechanism which unites the two.

A final reference may be made to the elasmobranchs. They are the only marine animals which have evolved the closed-box system or something approaching it. There are, perhaps, two possible explanations of their behaviour. They may first have discovered how to become permanently uræmic without suffering from it, and then to have utilised the associated advantage of protecting their embryos until a late stage of development. Or they may have adopted a protective closed box and then become adapted in some way to withstand the consequent uræmia. In any case, they offer an interesting comment on the terrestrial egg, for they seem to have found out a way of avoiding the uricotelic qualities of the closed-box system—a way, however, which appears to have been only suitable for a very restricted class of animals.

A generalisation or law might then be provisionally enunciated as follows: the main nitrogenous excretory product of an animal depends on the conditions under which its embryos live, ammonia and urea being associated with aquatic pre-natal life and uric acid being associated with terrestrial pre-natal life. If this should turn out to cover the facts not at present known, it will certainly be a contribution of chemical embryology to biology as a whole, for formerly there was not a trace of order or system among the facts obtained by comparative investigations of nitrogen excretion, and no answer to the question of why a uricotelic metabolism should exist at all. The answer here suggested is that terrestrial oviparous animals would have been impossible without it.

REFERENCES

1. Ambard, *Physiologie normale et pathologique des reins*, Masson, Paris, 1920.
2. Babcock, *Univ. of Wisconsin Agric. Exp. Station*, Research Bulletin, No. 22, 1912.
3. Barthélémy and Bonnet, *Bull. Soc. Chim. Biol.*, 1926, **8**, 1071.
4. Bialascewicz and Mincovna, *Trav. de l'Inst. Nencki Warsaw* (in Polish), 1921, **1**, no. 11.
5. Boussingault, *Ann. Chim. Physique*, 1850, **29** (ser. 3), 472.
6. Burian, *Archiv. f. d. ges. Physiol.*, 1910, **136**, 741.
7. Chauffard, Brodin, and Grigaut, *Annales de Med.*, 1922, **12**, 257.
8. Cushny, *The Secretion of Urine*, Longmans, Green & Co., London, 1917.
9. Dakin and Dakin, *Brit. Journ. Exp. Biol.*, 1925, **2**, 310.
10. Davis, *Journ. Biol. Chem.*, 1927, **74**, 511.
11. Delaunay, *Trav. de la Stat. Biol. d'Arcachon*, 1927.
12. Denis, *Journ. Biol. Chem.*, 1912, **13**, and 1913, **16**.

13. Fabre, *Ann. Sci. Nat.*, 1863 (ser. 4), **19**, 351.
14. Farkas, *Archiv. f. d. ges. Physiol.*, 1903, **98**, 490.
15. Fauré-Fremiet and Dragoiu, *Archives Internat. de Physiol.*, 1923, **21**, 403.
16. Fauré-Fremiet and du Streel, *Bull. Soc. Chim. Biol.*, 1921, **3**, 476.
17. Fiske and Boyden, *Journ. Biol. Chem.*, 1926, **70**, 535.
18. Folin, *Amer. Journ. Physiol.*, 1905, **13**, 45.
19. Von Fürth, *Vergleichende chemische Physiologie d. niederen Tiere*, Fischer, Jena, 1903.
20. Von Fürth, *Zeitschr. f. physiol. chem.*, 1900, **31**, 353.
21. Fuse, *Jap. Journ. Med. Sci.* (Biochem. Sectn.), 1925, **1**, 103.
22. Girod, *Comptes Rend. Acad. Sci.*, 1892, **118**.
23. Gortner, *Journ. Amer. Chem. Soc.*, 1913, **35** (1), 632.
24. Gortner, *Journ. Amer. Chem. Soc.*, 1914, **36** (2), 1556.
25. Gray, *Brit. Journ. Exp. Biol.*, 1926, **4**, 215, and 1928, **6**, 223.
26. Hammett, *Journ. Biol. Chem.*, 1915, **22**, 551.
27. Herter, *Mitteilungen d. Zool. Sta. Neapel*, 1893, **10**, 342.
28. van der Heyde, *Journ. Biol. Chem.*, 1921, **46**, 425.
29. Hopping, *Amer. Journ. Physiol.*, 1923, **66**.
30. Hunter, Givens, and Guion, *Journ. Biol. Chem.*, 1914, **18**, 387.
31. Ichimi, Morimura, Masumizu and Yazawa, *Jap. Journ. Med. Sci.* (Biochem. Sectn.), 1927, **1**, 119.
32. Idzumi, *Mitteilungen a. d. med. Fak. Univ. Tokio*, 1924, **32**, 197.
33. Kamei, *Zeitschr. f. Physiol. Chem.*, 1928, **171**, 101.
34. Kozmina, *Ber. u. d. wiss. Biol.*, 1928, **8**, 223.
35. Lewis, *Sci. N.S.*, 1918, **48** (2).
36. Loisel, *Comptes Rend. Soc. Biol.*, 1900, **52**, 661.
37. Lucas, Keith, *Sci. Prog.*, 1908, **3**.
38. McCallum, *Physiol. Rev.*, 1926, **6**, 352.
39. Minkovski, *Archiv. f. exp. path. u. pharm.*, 1886, **21**, 41.
40. Murray, *Journ. Gen. Physiol.*, 1925, **9**, 1.
41. Murray, *Journ. Gen. Physiol.*, 1926, **9**, 405.
42. Needham, *Brit. Journ. Exp. Biol.*, 1926, **3**, 189.
43. Needham, *Brit. Journ. Exp. Biol.*, 1926, **4**, 114.
44. Needham, *Brit. Journ. Exp. Biol.*, 1926, **4**, 145.
45. Needham, *Quart. Journ. Exp. Physiol.*, 1927, **18**, 153.
46. Needham and Needham, unpublished work, 1927.
47. Neumeister, *Zeitschr. f. Biol.*, 1898, **36**, 77.
48. Osterberg and Wolf, *Biochem. Zeitschr.*, 1907, **5**, 304.
49. Paton, *Journ. Physiol.*, 1909, **39**, 485.
50. Pearse, *Ecology*, 1925, **6**, 7.
51. Petri, *Zeitschr. f. Physiol. Chem.*, 1927, **166**, 125.
52. Popp, *Ann. Chem. Pharm.*, 1870, **155**, 351.
53. Przylecki, *Arch. Internat. Physiol.*, 1926, **26** and **27**.
54. Przylecki and Rogalski, *Arch. Internat. Physiol.*, 1928, **29**, 423.
55. Russo, *Atti d. Accad. Sci. Nat. Catania*, 1922 (ser. 5), **13**, 1.
56. Sakuragi, *Journ. Tokio Med. Soc.* (in Japanese), 1917, **31**, 1.
57. Salaskin and Kovalevski, *Zeitschr. f. Physiol. Chem.*, 1902, **35**, 552.
58. Schmidt-Nielsen and Holmsen, *Arch. Internat. Physiol.*, 1921, **18**, 128.
59. von Schreiber, *Annalen d. Physik* (Gilbert's), 1818, **13**, 83.
60. Sharpe, *Amer. Journ. Physiol.*, 1912, **31**, 75.
61. Szalagyi and Kriwuscha, *Biochem. Zeitschr.*, 1914, **66**.
62. Targonski, *Trav. de l'Inst. Nenchi*, Warsaw (in Polish), 1927, **4**, no. 2.
63. Tichomirov, *Zeitschr. f. Physiol. Chem.*, 1882, **9**, 566.
64. Vladimirov, *Biochem. Zeitschr.*, 1926, **177**, 280.
65. Wiener, *Beitr. a. chem. physiol. u. path.*, 1902, **2**, 56.

THE MODERN VIEWPOINT IN REGARD TO THEORIES OF AROMATIC SUBSTITUTION

By W. A. Waters, M.A., Ph.D.
Lecturer in Chemistry, University of Durham

No theoretical aspect of organic chemistry has been so profoundly influenced by the electronic theories of chemical combination as the subject of aromatic substitution, and the great flood of recent research publications dealing with this one subject bears witness to the special suitability of this field of investigation for the testing of the theoretical speculations as to the nature of "valency." So many and so diverse in nature are the suggestions that have been made, and so rapidly have the various theories been modified to keep pace with new experimental observations, that few more complex subjects are to be found in modern chemical literature. It is pleasing therefore to realise that from the midst of this great controversy is emerging a general consensus of opinion, indicating that a fundamental theory can at last be proposed which links up much aromatic chemistry with current electronic views on atomic structure and chemical constitution.

On considering the reactivity of all organic compounds containing the non-ionising "covalent bond" of two shared electrons it has long been realised that the initial stage in any reaction must be the loosening of the attachment of the atoms to each other, often with incipient ion formation. For example, the chlorine molecule when reacting with benzene tends initially to split up into both a positive and a negative chlorine ion, whilst the benzene molecule also must, before substitution, tend to separate into a positively polarised hydrogen atom and a residual phenyl group that contains a negatively polarised carbon atom.[1] After the substitution reaction has actually occurred the two initially polarised molecules will have interacted so as to give free hydrogen chloride, from the separated ions, and also chlorobenzene from the residual radicals, which become linked together by a new covalent bond. As, in practically all known aromatic substitution reactions, pairs of free ions are produced from non-ionic "bonded" molecules, this

[1] Cf. *Chem. Soc., Annual Reports*, 1926, p. 135.

tendency towards ionisation may be taken as an indication of what must be the fundamental mechanism of reaction. The additive processes, involving either intermediate addition reactions or intramolecular transformations, cannot be used to explain successfully more than a small fraction of the aromatic substitution reactions already known.

Since aromatic molecules react in virtue of this tendency to eliminate ions, the local electrical fields in the neighbourhoods of the various atoms—called the "local polarisation"—must be investigated before the actual substitution processes can be further explained. Such an investigation can be carried out in certain cases by measurement of the electrostatic dipole moment of the molecule,[1] and though the electrical polarisation of the normal "resting form" of the molecule, which is that measurable by purely physical methods, may be radically different from that of the "activated form" of the molecule at the moment of reaction, there is a remarkable agreement between the theoretical deductions drawn from such measurements and the known facts of aromatic substitution. Benzene itself, being highly symmetrical, is electrostatically neutral, but substitution derivatives in general behave as molecular dipoles owing to the presence of the substituent group. The electrical potential, or "resultant polarisation" near any atom within the molecule will in such cases differ from that of the standard zero of the unsubstituted benzene, and the replaceable hydrogen atoms of the compound will therefore show different degrees of reactivity in different positions, according to the extent to which ionisation as H^+ is favoured.

Since, in any aromatic substitution reaction, the hydrogen atom of the benzene derivative must separate as a positive ion, and the carbon atom of the aromatic ring must attract the positively polarised entering group by means of its negative charge, it can easily be understood why substituent groups which attract electrons diminish reactivity and vice versa. For example, a phenyl-ammonium salt (*e.g.* $\mathrm{Ph\,\overset{+}{N}\,Me_3}|\overset{-}{\mathrm{NO_3}}$ — Figure 1) contains, attached directly to the benzene ring, a positive ion (N^+) which will attract to itself the mobile electrons of the aromatic ring system, leaving each carbon atom with a resultant positive potential, and therefore *not* in a condition favourable for reaction.

The negatively charged oxygen atom of the phenoxide ion, that certainly exists in all solutions of phenols in hydroxylic solvents, will, in contrast, endow the carbon atoms of the aromatic ring with a negative potential and render them more

[1] Cf. *Chem. Soc., Annual Reports*, 1926, pp. 144–9.

reactive than in benzene itself. Non-ionic substituent groups which attract electrons (as chlorine) or repel them (as the methyl group) will act in the manner of positive ion and negative ion substituent groups respectively, though generally to a much smaller extent. A simple, but consistent, electronic explanation of the great differences in reactivity between the various classes of aromatic substitution compounds is therefore at hand.

It may be stated that this intramolecular electrostatic polarisation influence is usually termed the "general polarisation effect" by the Manchester school of investigators.[1] By Ingold[2] it has been called an "Inductive Disturbance," whilst by Flürscheim it has been considered to be the only true "polar factor" influencing molecular reactivity.[3] It may perhaps be pictured most easily as a general drift of electrons towards or away from the "directing" substituent group.

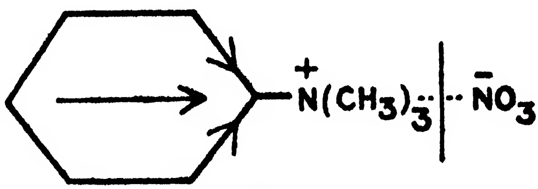


FIG. 1

The specific attraction of entering substituting groups to the carbon atoms in both the ortho and the para positions, or else only to those in the meta positions relative to a "directing substituent group" already present in the molecule, is, however, the most fundamental fact of aromatic chemistry. It can only be explained by postulating the existence of an "alternating polarity effect" superposed on the general polarisation effect. This second type of polarisation influence must, at the instant of reaction, if at no other time, produce at certain specific carbon atoms in the aromatic ring intense, though local, negative electrical fields, to which the entering positive radicals must be attracted so strongly as to be able to displace the hydrogen atoms previously there present. Within the aromatic ring system there must occur a regular alternation of available chemical energy from atom to atom, originating from some electrically polarised "key atom" situated within the directing group.

An electrostatic basis for theories of alternate polarity may be obtained by considering the chemical molecule to be built

[1] *J. Chem. Soc.*, 1926, 401.
[2] *J. Chem. Soc.*, 1926, 1310; 1927, 2918.
[3] *J. Chem. Soc.*, 1926, 1562; 1928, 1607, 2230.

up of a number of electrical conductors situated within an insulating dielectric. If a charged atom, or a dipole group, be introduced at any one point in such a system, local polarisation fields will be set up on each conducting atom, which will most probably alternate in sign in the requisite consecutive manner.[1] These local polarisation fields may occasionally be so strongly differentiated as to correspond with a system of alternating "semi-polar bonds," as postulated by Lowry,[2] in which bound ions and not neutral atoms constitute the skeleton of the benzene ring. Another theory of this same type is that of Flürscheim,[3] who considers the alternating polar effect to be a quantitative factor, and measurable by the amount of available affinity (or free negative charge) on any of the carbon atoms of the benzene ring.

Intense local electrical fields, however, cannot be detected within aromatic molecules in their normal unreactive "resting states," and electrodynamic theories to explain the production of such fields at the moment of reaction only consequently seem to be more in accordance with experimental fact. Aromatic compounds usually need to be activated by catalysts before they can be substituted, and it seems probable that the additional energy quanta then available become concentrated in the immediate neighbourhoods of the reacting atoms, where a high degree of local electrical polarisation may be attained. The "directing substituent group" must play a great part in guiding this extra free energy to the selected positions of attack, and is usually supposed to do so by initiating some tautomeric (or rather electromeric) transference of electrons. The benzene ring system is peculiarly adapted to be able to transmit intramolecular movements of electrons from atom to atom without alteration of its intrinsic structural form, and an electron pulse set up by one activated atom could result in the production of a free negative field on a distant atom. For example, the chlorine atom in chlorobenzene (Figure 2), having a strong attraction for extra electrons, might receive a free electron (*a*) from an external catalyst, and, in such a case, would then have to expel another (*b*) in order to preserve its octet stability. In this way would be originated an electron pulse throughout the benzene ring which, as illustrated in the diagram, would eventually lead to the momentary production of an intense negative electrical field at either carbon atom (2) or else at carbon atom (4). This would immediately be followed by substitution at the activated point.

As the rate of substitution at the various positions around

[1] *Cf.* Lapworth, *J. Chem. Soc.*, 1922, **121**, 416; Kermack and Robinson, *ibid.*, p. 427.

[2] *J. Chem. Soc.*, 1923, **123**, 822.

[3] *Loc. cit.*

the aromatic ring will be proportional to the frequency of reception of these electron pulses, the type of substitution induced in any aromatic compound will depend upon both the direction of the movement and the frequency of emission of the electron pulses from the activating directing group. The most active directive agents will be those groups which can most easily expel electrons by means of these tautomeric pulses which could activate both the ortho and the para positions. Conversely, groups which collect electrons will tend to prevent this local electronegative activation in the ortho and para positions, for they will be liable to withdraw to themselves pulses of electrons originating elsewhere within the molecule. As the carbon atoms in the meta positions are the least affected by tautomeric changes it can be stated that whilst electron-expelling groups produce ortho-para activation

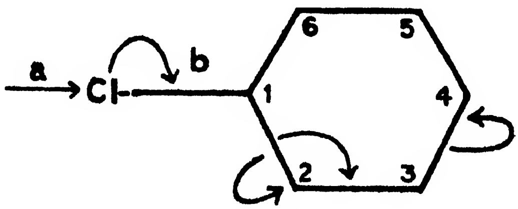


FIG. 2

(and substitution) electron-attracting groups will lead to meta substitution, though merely by inhibiting this localised polarisation.

The question of whether a substituent group will act as an activating " electron source " or as a deactivating " electron sink " with respect to these electromeric pulses can usually be decided from a knowledge of the electrochemical natures of its constituent atoms *under the special reaction conditions employed*. In the case of complex substituent groups, however, both the intensities and the frequencies of the electron pulses transmitted to the aromatic ring are resultants of effects due to every atom present. As the effective resultant electron charge transmission from any atom falls off very rapidly with increase of distance, it can be stated that the atoms nearest to the aromatic ring have the preponderating influence. Outer atoms, whether forming part of the substituent group, or existing in locally attracted solvent molecules or ions, can, however, exert upon the electron pulse activation an appreciable modifying electrostatic action which has been termed " electrostriction."[1] In addition the normal " general polarity " or " electron drift " polarisation within the molecule

[1] Cf. *J. Chem. Soc.*, 1927, 813, 2772.

does influence, and sometimes may perhaps even induce, the type of momentary electron pulses set up by the directing group. At all the possible reaction positions within the molecule the resultant induced activation will of course be the algebraic sum of the magnitudes of both of these polar effects, one or other of which may predominate according to the circumstances.

Most modern researches upon aromatic substitution are concerned with the evaluation of the relative strengths of these polar influences for different types of substituent groups. Many valuable results have been obtained from reaction velocity measurements, particularly by Olivier and his colleagues in Holland, which seem to indicate that the general electron drift influence is usually the predominating factor in determining aromatic reactivity.[1] Other measurements indicate that groups preserve similar relative activating powers under widely differing reaction conditions,[2] thus justifying the validity of the many deductions that have been drawn from the examination of the substitution reactions of already highly substituted aromatic compounds. The quantitative effects of systematic modification of the standard types of substituent groups have also been studied extensively in this country, with the object of ascertaining the actual points of origin of the electropolar influences.

From many indications it appears that it will not be long before it will be possible to draw up tables of quantitative values for each type of polarity-inducing force, from which might be deduced both the courses of aromatic substitution reactions and also the percentage yields of the various probable isomers. Much experimental verification of theoretical conceptions, and elimination of discrepancies between various points of view, however, must be attained before aromatic substitution laws can be expressed by theorems as concise as those current in the domain of physical chemistry.

[1] *Cf.* Shoesmith and Slater, *J. Chem. Soc.*, 1926, 216. Olivier, *Rec. trav. chim. des Pays-Bas*, 1922, **41**, 646; 1923, **42**, 516. Berger and Olivier, *ibid.*, 1927, **46**, 517. Berger, *ibid.*, 1927, **46**, 545.

[2] Bradfield and Jones, *J. Chem. Soc.*, 1928, 1010.

POPULAR SCIENCE

MODERN VIEWS OF MARS

By J. A. LLOYD, F.R.A.S., Telpyn Observatory, North Wales.

A NEAR approach of Mars to the earth is ever an event of momentous significance. It is then that the wireless enthusiast dons his headphones and listens with breathless eagerness for any stray signals which might possibly emanate from Martian wireless stations. It is then that every astronomer is besieged with questions relating to the supposed inhabitants of the ruddy planet, while his observatory is generally crowded with visitors waiting for a peep through the telescope, in the hope of catching a glimpse of our celestial neighbours.

These approaches of two worlds, the theory of which lies outside the scope of the present article, are called, by astronomers, "oppositions," and they occur, on an average, every twenty-six months. It is during an opposition that the planet can be studied to the best advantage, and consequently, every succeeding opposition marks a step forward in our knowledge of our mysterious neighbour-world.

Every opposition, however, is not equally favourable in point of nearness, and not for a long time to come will the two planets approach each other as closely as they did in 1924. Nevertheless, most oppositions afford good views of the Martian surface details, which are, broadly speaking, so similar in character to what our own earth would show, could we view it from Mars.

Every Martian opposition is, therefore, eagerly awaited by astronomers, every telescope, large and small, and every possible method of research, being brought to bear on the problem. The main object of their inquiries is to study the climate of Mars, and to endeavour to settle the vexed question of the nature and origin of those mysterious features popularly called "canals," and which some astronomers confidently hold to be the work of skilled Martian engineers. The reasons for this belief, together with certain objections to it, will be explained further on.

Meanwhile, as a preliminary, and in order to familiarise

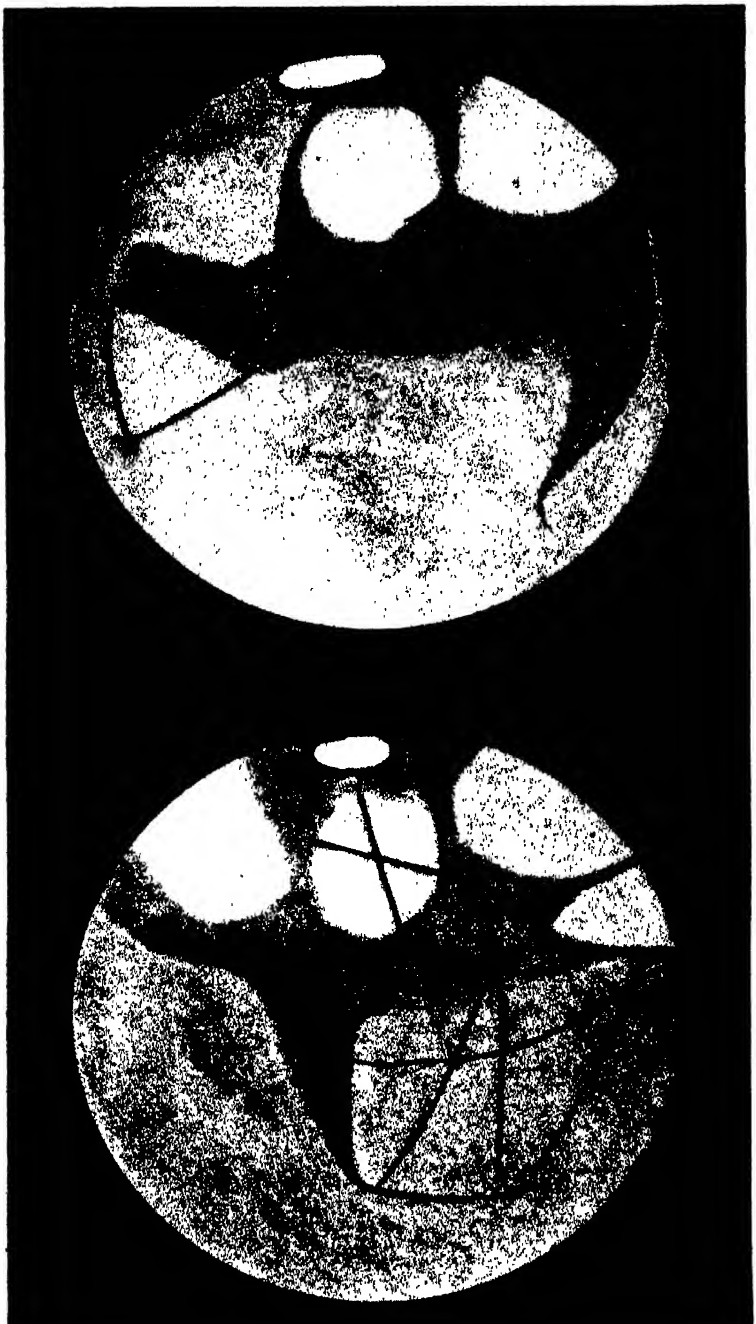
ourselves with our subject, let us just run over, very briefly' the history of Martian discovery. In this way we shall learn just how our knowledge of the planet has grown to its present extent from small beginnings. We shall thus be able to preserve a proper perspective, and we shall learn also to appreciate a little the great and manifold difficulties that lie in the way of a complete understanding of Martian phenomena, and how persevering astronomers, with mighty brains and all the resources of money and science to back them, have gradually met and overcome these obstacles one after the other. Not that our knowledge of Mars is by any means complete; it is far from being that, nor is it likely to be so for some time to come.

When the illustrious Galileo, in 1610, turned his newly constructed telescope to the heavens, it is no matter for wonder that Mars should have been among the earliest objects of his careful scrutiny. He failed, however, to detect the slightest indication of its "seas" and continents, and all he was able to state was, that Mars sometimes exhibits a slight phase, like a miniature gibbous moon. When we remember that Galileo's baby telescope magnified only thirty times, and was, besides, far from perfect optically, we must admit that he did very well.

Other contemporary observers fared little better, and it was not until 1630 that Fontana, of Naples, using a more powerful telescope than Galileo's, observed a grey marking on the planet, and which he found after a time to have changed its position. This apparent movement of the grey marking Fontana rightly attributed to the rotation of Mars on its axis.

The markings on the face of Mars were much better seen by Huyghens in 1656, and also about the same time by Hooke. To Huyghens we owe the first drawings of Mars ever made, and, crude though they are, his little sketches have proved of immense value to astronomers. For many of the markings he delineated can be identified and compared for position with modern drawings and photographs of the same objects, and have thus been the means of fixing the length of the Martian day to $\frac{1}{10}$th of a second!

The next astronomer of any note to take up the study of Mars was an Italian named Cassini, who, in 1666, using a telescope magnifying 130 times, clearly noted a number of greyish markings on the disc of Mars. Cassini found that the same markings reappeared after an interval of 24 hours and 40 minutes, and accordingly he fixed on this value as the length of the Martian day. This early determination of Mars's rotation period is very creditably near the truth. Actually it is 24h. 37m. 22·58s.



Mars as seen with a Newtonian Reflecting Telescope of $12\frac{1}{2}''$ aperture: showing Polar Caps, Continents, "Seas" and Canals.

In 1719 Maraldi drew attention to two exceptionally bright patches near the poles of Mars. These are the polar caps of Mars, probably of snow as in the case of the earth. So conspicuous are these features under certain conditions that they must have been perceived by the keen-eyed Cassini, and possibly by other observers; but no one seems to have considered them worthy of much attention. Maraldi, however, makes a special note of the fact that, while the polar caps seemed to keep to their positions, yet they varied greatly in size from time to time.

The systematic study of Mars, however, began during the latter part of the eighteenth century, when Sir William Herschel brought his powerful reflecting telescopes, and his unrivalled skill as an observer, to bear on the problem. After a lengthy study Herschel came to the conclusion that the dark or greyish markings which the earlier observers had seen on Mars were in reality seas, the brighter areas being the continents, there being far more land than water on Mars. The patches of superior brilliancy which Maraldi had observed he considered to be true polar ice-caps, which varied in form and size with the progress of the Martian seasons. That Mars possesses an atmosphere of considerable density Herschel concluded from the fact that portions of the planet's surface were liable to be temporarily obscured at certain times, presumably by dense strata of clouds.

The observation of Mars went on steadily during the nineteenth century. Beer and Madler, two painstaking German astronomers, made an intensive survey of the planet's surface during the period 1830–9. Though the instrument employed by them in their researches was only of 4 inches aperture, yet, with true Teutonic thoroughness, they used it to such good purpose that they were able to produce a very good map of the principal features of the planet—the first ever attempted, by the way. A better map was later constructed by Proctor, who worked from a magnificent series of drawings made by the celebrated observer, W. R. Dawes.

It would be quite impossible, in the space at our disposal, to enumerate all the devoted observers who worked on the problem of Mars during this period. The names of Terby, Lockyer, Kaiser, Nasmyth, De La Rue, Browning, and Green, appear among a host of telescopists, both amateur and professional, who did splendid service in unfolding to us the topography of our wonderful neighbour planet.

Nor were other lines of attack neglected. That most powerful weapon of the scientist, the spectroscope, which is really an instrument for analysing light, was directed to Mars by Huggins in 1867. His object was to search for traces of water

vapour in the atmosphere of Mars, and he considered the verdict of the spectroscope to be conclusively affirmative. Vogel, in 1873, obtained the same result. Strangely enough, subsequent observers obtained negative results; for instance, Prof. Campbell, of the Lick Observatory, both in 1894 and in 1896, failed to detect, both visually and photographically, the slightest evidence of aqueous vapour in the atmosphere of Mars. It was not until 1914 that definite proof of its presence was obtained by Slipher at the Lowell Observatory.

The year 1877 was a memorable one in the history of Martian discovery. In the first place, Mars was in an exceptionally favourable position for observation, thus affording a much clearer view of its surface than usual. Advantage was taken of this fact by Signor Schiaparelli, of Milan, to examine Mars on every possible occasion with a very perfect, though not a very large, telescope. His perseverance was rewarded by a discovery so very singular that many years were to elapse before the astronomical world deigned to accept it as a fact.

For Schiaparelli found that, in the rare moments of the best "seeing," the land areas of the Martian surface were crossed in every direction by a network of fine, dark lines. The most marvellous thing about these lines was that they were geometrically straight, varying in length up to more than 3,000 miles. Schiaparelli immediately assumed that these bewildering features were neither more nor less than channels for water, since they evidently joined the Martian "seas" one with another, and he called them by the word "canali," which is merely the Italian for channels. Unfortunately, the word was translated into English as "canals," thus implying that the markings observed by Schiaparelli were artificial, an implication which their discoverer was careful to refute: still, somehow or other, the name has stuck. Nowadays, of course, the name does not sound so strange.

In 1879 Schiaparelli again saw the canals, and yet again in 1881–2, when he observed that twenty of them appeared double, like the twin lines of a railway track, a distance of from 200 to 400 miles separating the components. The strangeness of this observation involved Schiaparelli in a storm of ridicule, and his eyesight, if not his good faith, was questioned in no uncertain terms.

But, very gradually, other observers in various countries began to report that they also had seen Schiaparelli's canals. They were perceived, for instance, by Perrotin and Tholon, at Nice, by Burton in America, and by Stanley Williams in England. Schiaparelli's observations were thus seen to have at least some basis of reality; whatever the true nature of

the canals, at any rate they were not entirely illusory, especially since these other observers could also see the duplicated canals.

Prof. W. H. Pickering, in 1892, carried the matter a step further. For he found that the canal tracks were not at all restricted to the Martian continents, as had originally been supposed, for at least one of the "seas," namely that known as the Mare Erythræum, was distinctly seen to be crossed by several canals. This, of course, proved conclusively that the Martian oceans contained no water, whatever may have been the case in the past.

There is one celebrated observer, however, who, in regard to Mars, stands quite in a class by himself. This was the late Prof. Percival Lowell, who, being possessed of great wealth, and a natural genius for astronomical observation, combined with boundless enthusiasm, erected and equipped a magnificent observatory, expressly for the study of Mars. This was in 1894, and the site chosen was at Flagstaff, Arizona, where the finest observing conditions in the world are experienced.

The first telescope was an 18-inch refractor, constructed by the famous firm of Alvan Clark; but this was soon replaced by a larger one, 24 inches in diameter, also by Alvan Clark. Other instruments were subsequently acquired, including a fine reflecting telescope of 40 inches aperture, and the Lowell Observatory is now one of the most important in the world.

Here, then, Dr. Lowell and a devoted band of assistants, which included Pickering, Douglass, and the brothers Slipher, were engaged in the unremitting study of Mars until Lowell's death in 1916. Discovery after discovery rewarded their efforts, and, as a result of his observations, Lowell convinced himself, and a few other astronomers, that Mars is inhabited by intelligent beings, and that the canals are the handiwork of these inhabitants.

Another observer of much the same type is M. Jarry Desloges, a wealthy French amateur, who has developed a remarkable system of mobile observatories, of which he possesses several, equipped with moderate-sized, but very perfect, telescopes. His observatories are moved, *à la* circus, from place to place as occasion requires, so that this unique star-gazer is able to keep in touch with Mars practically all the time.

Here, however, we must conclude our very imperfect sketch of the history of "Areography," as the study of the surface of Mars is called, and devote the remainder of this article to the consideration of modern ideas regarding the features visible on the face of the planet. We shall thus see how the foregoing observations, dating back at least to Herschel's

time, are to be reconciled one with another in order to give a consistent view of Mars as a world.

Suppose, then, the reader can gain access to an astronomical observatory, and is given the opportunity to observe Mars through a telescope of considerable power. What may be expect to see? He will, if conditions are favourable, see a circular disc of a bright orange colour, and he would probably judge this disc to be about the size of a large marble. On the disc is presented a map-like arrangement of alternately light and dark areas. If the observations are continued night after night, different arrangements of markings will become visible as different regions of the planet are brought round into view by rotation.

These markings are permanent, except for changes of colour as the Martian seasons come and go, and for temporary obscurations sometimes by white, and sometimes by yellow clouds. The white obscuring patches are believed to be true clouds of water vapour in the atmosphere of Mars, while the yellow clouds are considered by some astronomers to be sand-storms, but there are grounds for believing these also to be true clouds of water vapour. The heights of some of them have been measured at the Lowell Observatory, and they were found to average about 15,000 feet, the heights of the white clouds being very much more. It is likely, therefore, that the yellow clouds are in reality white, but appear yellow because they are seen through a greater thickness of the Martian atmosphere.

In general, the dark areas, conventionally called seas, exhibit a greenish-grey shade, while the remainder of the planet is of an orange colour, and is believed by most observers to be desert pure and simple. The proportion of "sea" to land on Mars is as 3 to 5.

The polar caps, already referred to, are remarkably variable. During the Martian winter the cap in the affected hemisphere is very large, covering an immense area of the planet's surface. But, as soon as the Martian spring comes in, the cap begins to shrink rapidly, until, by the end of the Martian summer, it has practically disappeared. Indeed, the southern polar cap does occasionally vanish entirely, though not always, whereas the northern cap has never been known to do so. This cycle goes on continuously year after year.

It seems most natural to suppose that the polar caps of Mars are indeed composed of snow, though some critics have suggested that they may be solid carbon dioxide. Other astronomers, however, believe them to be successive deposits of hoar-frost, at most not more than a few inches thick, rather than immensely thick sheets of ice similar to our own polar ice-fields. One important fact which does seem to corroborate

this theory is that, when Mars is in quadrature, white patches are often seen near the terminator, which quickly disappear when they are brought by rotation towards the centre of the disc. The terminator is, of course, the sunrise line, where we should expect to see traces of hoar-frost which had been deposited during the night.

Another fact which seems to point strongly to the same conclusion is that the polar caps do not shrink in a particularly symmetrical manner. It happens that the shrinking takes place in patches which get left behind as the central portion of the cap dwindles away. These patches persist for quite long periods, and seem to occupy the same areographical positions every year. There is only one way in which we can account for this phenomenon, and that is by supposing that these isolated white spots occupy the high mountainous regions of the planet, where we naturally expect to find traces of snow lingering long after it had disappeared from the more low-lying areas.

That the greyish regions of Mars are not seas in the strict sense of the term had long been suspected. The main reason for the suspicion lay in the fact that the most pronounced seasonal changes were seen to occur in these regions. Though these dark areas preserve their form and extent unchanged, yet they vary greatly from time to time in their degree of visibility, sometimes being very difficult to observe. Some regions undergo most remarkable changes of colour, which repeat themselves over and over again, and are clearly dependent on the Martian seasons. The suspicion became practical certainty, however, when Prof. Pickering announced the discovery of canals crossing the so-called seas, for it was impossible to imagine markings of this sort on anything but the driest of dry land. A negative proof is also provided by the fact that, if the seas were really liquid, or if there were a sheet of water of any reasonable size on Mars, it would, under certain conditions, show a reflection of the sun as a brilliant point of light, which would be easily visible in large telescopes. It has been calculated that a lake six miles in diameter would suffice to show this optical effect; but, though such a point has been repeatedly looked for, it has never once been seen.

The canals are, however, the *pièce de résistance* of Martian observation, and upon their true nature devolves the answer to the well-worn question, Is Mars inhabited? At one time the canals of Mars were regarded as mere optical illusions on the part of the observer, or even as projections on the image in the telescope of the blood-vessels in the eye. But there can now be no question of the objective reality of those markings, since the drawings made by many skilled observers working

quite independently, and in various parts of the world, show the canals as occupying the same identical positions.

What is the smallest telescope that will show the canals? This is a question that is very difficult to answer, for much depends on the observer, and more still on atmospheric conditions. Schiaparelli discovered the canals in the first instance with an 8-inch refractor; but that was in the clear air of Milan. Stanley Williams afterwards saw them with a 6½-inch reflector, while Prof. Pickering could even see some of the more conspicuous canals with a 4-inch refractor at Arequipa, in Peru; but this is an extreme case. The writer has repeatedly seen a large number using a reflecting telescope with a 12½-inch mirror.

On the other hand, the late Prof. Barnard, one of the most skilful observers who ever lived, could see no canals, even though he employed some of the greatest telescopes in the world. This is also the attitude of M. Antoniadi, who has observed Mars with the great Meudon telescope. The advocates of the canals, however, have a ready answer to this. They say, and quite rightly, that though a small telescope may be inferior to a large one in sheer magnifying power, yet its defining power may be, and often is, far superior. A very large telescope is always hampered by want of steadiness in the atmosphere, and will seldom afford such clear views of planetary detail as an instrument of moderate dimensions. Large telescopes are really for other work.

The best observers also differ greatly in their perception of the canals. The majority draw them as hard, fine dark lines, with circular dots at their intersections, and so they appear to the present writer. Other observers, equally skilled, see the canals as broad, rather diffuse shadings. It is very difficult to reconcile these discrepancies of observation, but it must be remembered that we are dealing with objects which lie at the utmost limit of visibility, and certain personal peculiarities of the observer can scarcely fail to enter into the matter. Every observer, however, recognises that the canals vary enormously in visibility from time to time, these variations following the course of the Martian seasons with almost uncanny precision.

It was at one time hoped that photography would settle the question of the Martian canals once and for all. But though hundreds of photographs have been taken with some of the largest telescopes in the world, the canals are so fine that they are, in general, masked by the grain of the plate. Only in a few instances, therefore, have anything resembling canals been recorded, and that only in an uncertain manner. Other difficulties of a technical nature render it highly probable

that photography is powerless to give a decisive judgment on the matter. Photography, however, has proved remarkably useful in following the seasonal changes in the broader features of the Martian disc.

Considerable attention has been given of late to the question of atmosphere on Mars, since this would have a most important bearing on the habitability of the planet. Given air and water, and a fairly equable climate, there is at least a reasonable chance of a planet being inhabited.

That Mars does possess an atmosphere of considerable density is known in several ways. For instance, without the presence of an atmosphere the polar caps could not be formed, for they clearly indicate the depositing of some suspended vaporous substance circulating in an atmosphere. These vapours, as has been noted above, can also be seen at times in the form of clouds floating in that atmosphere. Many other proofs could be cited if space permitted.

An interesting line of research was, however, opened up by Prof. Wright, of the Lick Observatory, in 1924, which at least deserves a passing mention. This brilliant astronomer conceived the idea of photographing Mars in light of different colours, the colours being isolated by using suitable filters combined with specially sensitised plates. The most striking results were obtained with violet and infra-red light. In the photographs taken with violet light, all surface detail is obliterated, except for the polar cap, which appears greatly enhanced. In fact, these photographs appear to be pictures of the planet's atmosphere and nothing else, in which case it is reasonable to infer that the polar caps are largely atmospheric in character. Or, on the other hand, they may be shrouded by a thick bank of clouds.

The photographs taken in infra-red light, on the other hand, show the surface features of Mars very conspicuously, while careful measurements of the two photographs indicate a height for the Martian atmosphere of at least sixty miles. The density of the atmosphere is estimated on the best authority to be about one-fifth of that of the earth.

Another matter which has a considerable bearing upon the question of life on Mars is that of temperature. Fortunately, more than one method is available for determining temperatures on Mars, and considerable attention has been devoted to the problem, especially at the Lowell Observatory, and at Mount Wilson. The observers at both places are in remarkable agreement. They state that, owing to the comparative rarity of the air on Mars, great differences of temperature must obtain on the planet, and that, though the days are fairly warm, the temperature reaching about 50 degrees Fahrenheit

at the equator at noon, yet the nights must be very cold, with a temperature far below freezing-point.

We cannot, however, settle the question of whether Mars is inhabited, until we have determined the nature and origin of the planet's intricate canal system. If, as Lowell supposes, the canals are artificial, then they must have been constructed by the inhabitants of Mars for some specific purpose. What purpose, then, could they serve?

Lowell argued his case as follows. Mars, as is conceded by all competent observers, is a world from which the water-supply has long since disappeared; its seas have become dry land. Therefore, all the water available for the support of life on the planet is contained in the polar caps, and little enough it is in all conscience.

But just because it is so scanty, that is the very reason why it must be conserved to the last drop, and made to irrigate as large an area as possible of the planet. And so the intelligent population of Mars, realising that its case was desperate indeed, banded itself together at some far-back date, and constructed this mighty network of something like 800,000 miles of canals, in order to lead the water from the melting polar caps to irrigate their parched and arid deserts.

That such a work would necessitate a colossal effort cannot be doubted, but the force of gravity on Mars being only one-third of what it is on earth, we may conceive that such a feat could be accomplished on Mars with a third of the effort needed to perform a similar work on earth.

The canals do certainly seem as though they were conveyers of water, because, in the first place, the commencement of the canal system is at the polar caps, from which they appear to radiate with a geometrical straightness strongly suggestive of intelligent design. Moreover, the melting snow-caps of Mars are always edged by a bluish ring following the contour of the cap, and which observations by Pickering with an instrument known as a "polariscope" have shown to be certainly water, or at any rate, liquid. In the second place, as soon as the Martian spring sets in, and the polar caps begin to melt, then the canals become more and more conspicuous, and from this fact Lowell again argues that it is not the canals themselves we see so much as the vegetation growing along their banks.

One serious objection to this theory is the difficulty of conveying water across vast distances without loss by evaporation; but the advocates of intelligent construction have a reply ready to hand. They say that there is nothing to prove that the canals are open troughs; they may in fact be covered conduits, the water being pumped out of them through pumping stations situated at regular intervals, more or less, along the

course. But, in any event, it is gravely doubtful whether there is a sufficient supply of water in the polar caps to minister to the needs of a whole world. Against this it may be argued that the population of Mars is now pretty thin, and is confined mostly to the spots round the intersections of two or more canals, called " oases " by Pickering, who first described them. These places would correspond to our great terrestrial cities.

This vivid picture of a great social organisation on another planet has not yet been generally accepted by astronomers. But, whether we believe in it or not, the theory must ever command our respect and admiration as a fine piece of logical and deductive thinking. If there is life on Mars (and no one doubts that life is possible there), it is only indirectly that we can become aware of it. Direct telescopic study of forms of life, even when Mars is nearest, is a dream that can never be realised.

One other possibility remains to us. Perhaps, some day, an intrepid voyager, the Lindbergh of space, will succeed in bridging the vast abyss which separates the two worlds, and will survive to tell the tale. Till then, all that we can say in reply to the question, Are there men on Mars ? is—perhaps.

NOTES

Synchronised Reproduction of Sound and Scene (S. K. Lewer)

Talking films form the subject of a number of articles in the *Bell Laboratories Record* for November 1928. Although written from the point of view of a telephone engineer, three of them are of general scientific interest. These deal with the recording of sound on wax, photographic recording of sound, and speed control in reproduction, respectively.

In *Recent Advances in Wax Recording*, H. A. Frederick considers the lateral cut disc records used in the Western Electric Company method of synchronised motion-pictures. For these records it is found necessary for the recorder to have a characteristic which falls at frequencies below about 250 cycles, so that the amplitude at lower frequencies is approximately constant at the maximum permissible (about 0·002 in.) without cutting over from groove to groove. In practice, this is largely overcome by a correspondingly larger response in the pick-ups which are used.

In the recording, the highly polished wax disc is run at a constant speed (giving a linear speed of 140 feet per minute at the outside of the spiral), and is maintained at the proper temperature. The cutting stylus is supported at the proper height by means of an advance ball which rides lightly on the surface. An electric " playback " of small mechanical impedance can be used for direct reproduction from the newly cut record. No great injury results from playing the record several times with such a pick-up. The " master," a negative of the original wax, is made by electroplating the wax after it has been carefully brushed with an extremely fine conducting powder. From this, two test pressings are made by moulding. If these are satisfactory, the " master " is electroplated. From the positive, commonly called an original, there is plated a second negative, a metal mould known as a " stamper." From it duplicate originals may be plated, and from them, duplicate " stampers." By means of recent refinements, finished pressings may be obtained within three hours after the delivery of the wax.

The amplitude of the needle trace at low frequencies, as already pointed out, is limited by the spacing of the groove.

At high frequencies, the curvature of the undulation must be considered. For the intensity corresponding to this limiting amplitude, the undulation would have the same curvature as the needle point at about 7,000 cycles. This, however, does not represent the practical limit, for in speech and music these high frequencies are characterised by lower intensity. Present commercial needle points are therefore responsive up to frequencies of at least 10,000 cycles.

Apart from the improvements in the record itself, there have been great advances, in the last two or three years, in the design of pick-ups, notably in the reduction of needle point impedance and the almost complete elimination of resonance. This has resulted in a longer life for both needles and records.

Further, the rearranging of the recorded material which becomes necessary in the production of a talking film is entirely practicable, so that the wax record appears to be a formidable rival of the photographic record for the synchronised accompaniment of motion-pictures.

In *Sound Recording with the Light Valve*, N. MacKenzie describes a mechanical valve designed to produce a light trace by varying the width of a light beam of constant intensity. There are several ways of recording sound on a film, but this is considered to offer the most immediate promise. The light valve consists of a loop of duralumin tape suspended in a plane at right angles to a magnetic field. Normally, the two sides of the loop constitute a slit 0·002 in. by 0·256 in. The loop is directly connected to the output of the recording microphone amplifier. Under full modulation, the width of the slip varies from zero to 0·004 in. in phase with the fluctuating current. For several reasons the tension of the loop is adjusted to give a natural frequency of 7,000 cycles, and under these conditions the power required for full modulation is about ten milliwatts. A ribbon-filament lamp is used as a source of light. A carefully designed optical system produces an image of the slit on the film as a transverse line 0·001 in. by 0·128 in. The frequency limit, at which the film would develop to a uniform intensity, is determined by the width of the image on the film and by the speed of the film (90 feet per minute), and is about 18,000 cycles per second. The falling characteristic as this frequency is approached is largely offset by tuning the loop to a frequency near 7,000 cycles, resulting in an approximately uniform response over a very large range of frequency.

A photoelectric cell, used in conjunction with an amplifier, is mounted inside the recording machine behind the film at the line of exposure. The modulation of the transmitted light thus provides a convenient method of observing the recording

as it is actually being made on the film. Provision is made for the combination of several microphones under the control of the mixer operator in the monitoring room.

In printing these sound negatives in combination with picture negatives, it is usual to print one negative at a time, masking the space needed for the other. When distortion is present, the harmonics thereby introduced can be reduced to an undetectable intensity by suitable development of the negative and positive. The correction is available only over that part of the photographic range where exposure is correct, but fortunately there is considerable latitude in the average exposure.

For either wax or photographic sound records, very accurate speed control is necessary in addition to synchronisation between sound and picture, since the musical pitch varies directly with the speed of the record. A maximum fluctuation of 0·2 per cent. has been agreed upon. H. M. Stoller describes in *Speed Control for the Sound-picture System* the details of an electrical method which meets the requirements. As in all types of governing apparatus, a change of speed introduces a tendency in the prime mover to counteract this change, and if the governing is to be made very close, a dashpot equivalent must be provided to prevent " hunting." The prime mover in this system is an electric motor (running on either D.C. or A.C. supply), the speed of which is controlled by changing the impedance in the armature circuit. This impedance forms the outer windings of a three-legged reactance coil, and is changed by the direct current flowing in the centre winding. The heart of the governor circuit is a special bridge circuit fed by an auxiliary 720-cycle alternator coupled to the main driving motor. Any small changes from the desired speed cause abrupt phase changes in the output from the bridge. The link between the bridge and the three-legged reactance coil is a thermionic valve circuit which causes more direct current to flow through the centre winding on account of the change of phase as the motor speed tends to fall. In this way the impedance of the outer windings is changed so that the motor speed tends to rise. The dashpot equivalent to prevent oscillation of speed consists of a resistance-capacity network. This comparatively simple and compact governing system has been found to provide a satisfactory speed control of the driving motor under all ordinary conditions to within the required limit of 0·2 per cent.

Course-indicator and Receiving Apparatus for Aircraft Radio-beacon (S. K. Lewer)

As part of the research programme of the United States Bureau of Standards on aids to air navigation, special radio

receivers have been developed in conjunction with the double-modulation type of directive radio-beacon, and the tuned-reed visual course-indicator. This beacon system provides a convenient and reliable means of informing the pilot at a single glance at the indicator as to whether or not he is flying on a specified course, and if not, to which side and by how much he has deviated.

The design of the tuned-reed indicator is described by F. W. Dunmore in the *Bureau of Standards Journal of Research*, November 1928, pp. 751–69. The indicator consists essentially of two vibrating reeds, the relative amplitudes of which indicate the position of the aircraft with respect to the beacon course. The radio-beacon is a transmitting station which sends out 290-kilocycle waves on two loop aerials. These aerials are at right angles to each other, the waves from each being modulated but at two different low frequencies of between 50 and 100 cycles. A figure-of-eight characteristic is produced in space, and there are two lines (or four directions) along which the two modulated signals are of equal intensity. These directions are the courses where the two reeds of the indicator vibrate with equal amplitudes.

Several types of indicator were tested, each having its own advantages and disadvantages. The indicator best suited to general flying conditions consists of a pair of thin steel reeds having small bimetallic strips soldered to their free ends to overcome any changes in frequency arising from changes in temperature. The two reeds, tuned to frequencies of 60 and 85 cycles respectively, vibrate between two pairs of electromagnets situated near their clamped ends. The choice of frequencies involves many considerations, and these were chosen as being the most suitable after a long series of experiments. The four electromagnets are connected in place of the telephones of a six-valve set. Two strong permanent magnets are used to polarise the reeds, and thereby greatly increase the sensitivity. Each reed is fitted with a small damping vane to flatten the resonance curve, so that any slight change of modulation frequency at the beacon will not appreciably affect the amplitude of the reeds. Even with large damping it is possible to obtain a proper working deflection of 8 mm. with only 3·6 volts applied to the reed indicator driving coils and only 1 milliamp. in these coils. The indicator unit is arranged to plug into a shock-proof mounting on the instrument board of the aeroplane. With mounting its dimensions are $3\frac{1}{8}$ by $2\frac{3}{4}$ by $4\frac{1}{8}$ in., and weighs under 2 pounds.

The receiving apparatus used in conjunction with these indicators is described by H. Pratt and H. Diamond in "Receiving Sets for Aircraft Beacon and Telephony" (*Journal*

of Research, October 1928, pp. 543–63). The receivers are designed to operate over the two wave-bands, 285–315 kilocycles for beacons, and 315–50 kilocycles for ordinary aircraft communication. The dominating requirement in design is that of small weight and size. In addition, a simple remote control system is needed so that the set can be installed anywhere on the aircraft; the tuning system must be of a uni-control type covering the two bands, and a volume control is required since the distance over which the signals are travelling is constantly changing. The set must be shielded from the engine ignition system, and must be built to withstand continuous mechanical vibration. The maximum output of the set should be about 10 volts of audio-frequency power with a load impedance of from 4,000 to 7,000 ohms. A uniform amplication covering a frequency range of 40 to 3,000 cycles is desirable; the lower frequencies are important as they are utilised in the beacon signals. Sensitivity and selectivity must both be high, particularly the former, to permit short vertical pole aerials to be used. These have replaced the dangerous trailing wire aerials. The choice of valves has an important bearing on the total weight and bulk of the equipment. Some of the smaller standard valves are not sufficiently sturdy, and special valves have been used; but there is still much need for better valves for aircraft sets.

The conditions of sensitivity, selectivity, and uni-control are met by using three or four tuned neutralised radio-frequency amplifier circuits with gang variable condensers. The coupling transformers have been very carefully designed for the purpose. Partial shielding with copper was found to be quite satisfactory. Complete shielding with aluminium, which was somewhat difficult owing to the involved mechanical assembly, resulted in objectional eddy-current losses. For the detector valve, grid rectification was chosen since it is considerably more sensitive than plate detection. Two stages of low-frequency amplification were included, making a total of six valves. The volume control consists of a rheostat connected in the filament circuit of the radio-frequency amplifier. Although there is a considerable reduction in weight and bulk gained by using 1-volt or 3-volt valves, a much better general performance is obtained from the larger 5-volt valves which require heavier batteries. For convenience in operation, adjustable stops were provided to allow the uni-control handle to be rapidly turned from one predetermined setting in the beacon wave-band to one in the telephone wave-band.

During extended tests, satisfactory day-time beacon signals were received on an aeroplane at a distance of 100 miles with an aerial 10 feet high.

Further details of the experiments outlined here are given in "Development of Radio Aids to Air Navigation," J. H. Dellinger and H. Pratt (*Proc. I.R.E.*, **16**, pp. 890–920, July 1928).

Adders in November: Birth in Captivity (N. Morrison, D.Sc., F.Z.S.)

The adder (*Vipera berus*) has her young about the end of August and the first week in September. The gestation period is 4½ months.

It is perhaps a unique experience for a herpetologist to have a captured snake giving birth to young immediately after being caught. Such has been my experience on two occasions, the first on August 29, 1905, and the second quite recently on October 1, 1928. From my introductory statement it would be observed that the latter birth fell on a later date than normal. The adder in this case was a healthy specimen measuring 23 inches in length. On being caught I noticed that it was a bit sluggish and lethargic in its movements, making very little effort to escape. There was nothing in its contour to suggest pregnancy beyond being slightly corpulent, and I was certainly very much surprised to find, on opening its cage one morning eight days after capture, a brood of eight young adders. They measured 4½ inches in length and six are females and two males. They were all normal specimens, except one, which had malformation of the maxilla and palate. It was not so active as the others, which very soon after birth developed a marked biting tendency. The viciousness of these creatures prompted me to test their toxicological properties. So I got them to bite an ordinary microscope slide, when I found that no secretion was produced until the third day after birth. I mixed some of the venom with fresh pig's blood, and a microscopic examination of the slide revealed that the blood underwent a comparatively rapid hæmolytic and agglutinative change, indicating that the poison in this short period had acquired its maximum potency.

This in itself is an interesting observation and worthy of being placed on record, since it established that the degree of virulence of adder's poison is not proportionately in keeping with the degree of the physical development of the creature.

I have said in another place that adders will not feed in captivity, but I thought that these youngsters might be induced to take food, because in their case they had no knowledge of the freedom of wild life—their world was circumscribed by the confines of their cage. One would therefore imagine that they would respond to nature's call for sustenance; consequently I made special efforts to coax them to take suitable nourishment, but my efforts were fruitless. They

stolidly refused to touch a morsel of food, absolutely loyal to the wild instinct of their species.

Mother and family are still alive and well. Of course adders live for a long time without taking any food. The hibernation period begins early in October, and although it is now November 16, these creatures show no sign of becoming sleepy or torpid. Adders will not hibernate in captivity.

I should like to mention that the female adder did not show any maternal instinct towards her offspring. This may have been due to the reptile being in captivity. It is further interesting to record that, although not feeding, the young adders have stretched almost half an inch in length in twenty-six days.

A Bar to Medical Research (R. R.)

For a long time a very silly notion has been prevailing among certain medical minds that while medical men may charge fees for treating individuals, they should not expect any payment for scientific work, even if it be of great advantage to millions of persons now living, or who may live in the future. This doctrine is often enunciated by persons who themselves have made no researches which could by any possibility be deserving of any such reward. There are 54,146 British doctors, but we doubt whether one in a thousand has ever added materially to public knowledge in medicine.

Of course no such law exists in medical ethics. No one has the smallest right to say that the public shall not give rewards for discovery, medical or other. Yet people who have done nothing deserving of reward often pretend that there is some such occult proviso in medical affairs.

Such teaching is not borne out by history. Early last century Parliament gave Edward Jenner £30,000 for his discovery of vaccination against small-pox, and recently the Canadians gave a fixed income for the invention of insulin; and the Nobel Medical Prize was established in 1901 for the express purpose of giving an annual prize for medical discovery. We suppose that parliaments have the right to reward anyone they please, even those who have conferred great benefits on the whole world, and in spite of doctors' prejudices.

Some time ago, when Parliament decided to reward persons who had made inventions which were of use to this nation during the war, medical men were expressly excluded from any such benefit by the lawyers who administered the said reward. By what right medical men were so excluded, I do not know; but we must suppose that when lawyers cannot find a sound reason for excluding any inconvenient right, they can always invent one.

Common sense suggests that if rewards must not be given even to those who have made discoveries of world-wide value, they should not be given at all in the form of patients' fees to any doctors—in which case our numerous patients would have to frequent, not hospitals, but workhouses, for their medical advisers. It would scarcely be believed, or thought possible, that sane persons should adopt such a ridiculous line of argument against world-benefactors, or that a civilised nation should be so easily gulled by an obviously foolish doctrine, evidently prompted rather by jealousy than by truth.

Tardy Recognition (R. R.)

In the July Number of SCIENCE PROGRESS, No. 89, 1928, I gave a short Obituary of my two brothers, Dr. E. H. Ross and Dr. H. C. Ross. Those who are still interested in them might like to see the Journal of *Tropical Medicine and Hygiene* for December 15, 1928, containing an article by Albert Wilson, M.D., with remarks on the work of H. C. Ross and his colleagues on Cancer. It has taken the British public many years to understand that there was anything at all in that work. My own experiences have been similar, for the same public does not in the least comprehend, even yet after thirty-two years, the importance of my own teaching regarding malaria. Is it that game-playing and party politics are gradually sapping the British intellect?

Inevitable Discovery (R. R.)

In his essay on Dryden, Macaulay developed the hypothesis that scientific discoveries come to mankind somewhat as the sun rises on the just and the unjust alike, and whether the discoveries are sought for or not. So far as I know, Macaulay himself never made any; and there is one strong reason why we should not accept any such view. That is, if it were true, every possible scientific discovery should have been made long ago. But we still hope for advances in scientific knowledge in spite of Macaulay.

The late Prof. Starling seems to have believed in Macaulay's eccentric persuasion, for in *Nature* of April 26, 1924, he used the same argument in order to depreciate the labours of men who pride themselves on successful investigations. I was the first victim of his wrath. He declared that if I had not proved that malaria and mosquitoes are connected someone else would have done so in a few months or years. So far as I know, Manson and I were the first who tried to find how malaria is carried. I doubt whether anyone else would have touched the theme if we had failed. Our failure would

have been written up in medical literature as a warning against a repetition of our efforts—as in fact some foreign gentlemen held up a previous attempt of their own which failed.

When we come to think of it, very few people indeed ever really try to make any advances in science. How many out of the whole human race, numbering 1,840,000,000 persons, ever attempt to gain any new knowledge at all for the use of their fellow-men? All the five hundred Fellows of the Royal Society, with even the members of other scientific bodies, would constitute a drop in the ocean compared with those who take no interest in such matters or who do not attempt original work. Think, not only of the British Isles, but of all the people living in the rest of the world who are not only indifferent to science but to all human knowledge, except perhaps knowledge of politics, money-making, or food-getting. Scientific advances are, therefore, very rare indeed. For example, Archimedes very nearly discovered the Calculus, but about 2,000 years elapsed before Barrow and Newton did so. I could mention many things which have been strenuously sought for, but without success during the last thirty years, for example, the carrier of the *Leishmania* parasites, and the germ of yellow fever, in spite of the money spent on financing such investigations. I doubt whether anyone would have succeeded in discovering how malaria is carried, in spite of Starling's forecast, for centuries hence, if at all, without Manson and myself. True, much parasitological work is now being done in all directions; but we have no right to assume that it is as good as it was, or that it will continue to be as good as it was. Many young gentlemen tell us of the advances which they are going to make in the future, but that is generally the end of it.

Joint Memorial to Benjamin Neeve Peach, LL.D., F.R.S., and John Horne, LL.D., F.R.S.

Benjamin Neeve Peach died in January 1926, and his life-long friend and fellow-worker, John Horne, followed him in May 1928. In response to a widely-expressed desire that the eminent services rendered to Scottish geology by these two distinguished scientists should be recognised in some appropriate form, a representative committee was convened, and met in Edinburgh on November 28th, 1928. This committee, which included delegates from the Geological Survey and the Royal Scottish Museum, from the scientific societies with which Drs. Peach and Horne were most closely associated, and from the Geological Departments of the Scottish Universities, has decided to take steps to raise a memorial.

The memorial will take the form of a bronze plaque to be

placed in a suitable position in the Royal Scottish Museum, and of a commemorative inscription upon some conspicuous rock-face or boulder at one of the classical geological localities in the North-West Highlands. An outlay of £200 to £300 is contemplated. Subscriptions of any amount, large or small, will be welcome and should be sent to Mr. M. Macgregor, H.M. Geological Survey, 19 Grange Terrace, Edinburgh.

The Gorgas Memorial Institute of Tropical and Preventive Medicine

The following is an extract from *Science*, December 21, 1928:

"This institute has been established by act of Congress as a governmental tribute to the public health and sanitation work conducted by the late General William C. Gorgas on the Isthmus of Panama, which made possible the construction of the Panama Canal. Dr. Clark, who spent several years under General Gorgas in the Canal Zone, will carry on research work to make possible a greater economic development of tropical America.

"Congress last spring authorised a permanent appropriation of $50,000 a year for the maintenance of the institute. Latin-American Governments have been invited to contribute, but it has been stipulated by Congress that the total of their contributions should not exceed 75 per cent. of the total contributed by the United States. The participating Latin-American Governments are to be represented with the United States on the board of directors. President Coolidge is honorary president of the institute and Rear-Admiral Cary T. Grayson is the president.

"The Republic of Panama has ceded a site for a permanent building for the laboratory, but temporary headquarters will be established in a building newly constructed by that Government for a medical school."

Mosquito Breeding in Queensland

The *Proceedings of the Royal Society of Queensland*, vol. xl, No. 8, 1928, contains a valuable article on the breeding habits of mosquitoes in Queensland by R. Hamlyn-Harris, D.Sc., City Entomologist, Brisbane. The article contains many details which are as usual of not much interest in British science, but which should be read by workers on the subject in most other countries where mosquitoes abound much more than in England—by good luck or bad luck for the empire. Those who are interested are advised to write to Dr. Hamlyn-Harris himself, or to the Royal Society of Queensland.

Professor Fernand Vidal M.D. (R. R.)

We regret to notice that this distinguished French scientist died on the night of January 14, of cerebral hemorrhage. He was famous for having first verified the discovery of Serum Diagnosis in typhoid, and he acquired such fame for this that serum diagnosis in typhoid has often since been called the "Vidal Test." The test, however, was originally devised by the late Albert S. Grünbaum in Prof. Grüber's laboratory in Vienna, March 1896, but, owing to the paucity of cases there, Grünbaum was unable to complete the work at the time. Grüber, however, announced the work at a medical congress and Vidal heard him speaking, and, having many cases on his own hands, he verified Grünbaum's invention before it was complete and got the credit for it in Britain and in many other countries. I am glad to say that Vidal himself was not in any way to blame for this miscarriage of justice, and was much to be praised for the able manner in which he tested the test in practice. See a paper by me in the *Lancet* of February 1, 1902.

Biologists for the Empire

The Times of January 13 prints an interesting article on this theme and quotes two leaflets prepared by the Imperial Agricultural Research Conference, 10 Whitehall Place, S.W.1. There is a large and growing demand throughout the British Empire for men trained in biology. The Universities have not yet succeeded in satisfying this demand; almost every one of the dominions and colonies has expanded its staff of trained biologists during the last ten years to such an extent that the demand now exceeds the supply. Even the colonies which stand on the threshold of full development have recruited over five hundred biologists during the last eight and a half years and the numbers of directors and of assistants now exceed 1,200. Commercial and other concerns such as the Empire Cotton Growing Corporation and the Rubber Association of Malaya and Ceylon are also making increased use of research workers.

The cause of this shortage is attributed in part to lack of appropriate teaching in schools and universities, the physical side having predominated to the detriment of the biological side. To overcome these disabilities many suggestions have been put forward, such as, better co-operation between schools and universities and specialisation after graduation, the student to obtain a thorough grounding in the principles of biological science, chemistry, physics and practical agriculture. The article in *The Times* concludes with the following paragraph :

"The Empire needs biologists to combat insect and fungoid diseases, to breed improved crop varieties, to grade up livestock and advise on pedigree breeding, to improve meat and dairy produce, to fight animal diseases, and to prevent loss in the storage and transport of agricultural produce. Prospects before the able young biologist, therefore, promise to be very bright for some years to come. He need not shut himself up in a laboratory for the rest of his days, and his work takes him out-of-doors into the field and plantation." An added inducement to enlist in this kind of work is the offer of salaries ranging from £300 to £800 at home, £500 to £950 in the Colonies, and £1,000 to £2,000 for the senior appointments.

Abusing Benefactors (R. R.)

The Native Press in India, and sometimes in other countries, has long been inclined towards satirising strangers who arrive with any sanitary propaganda in the hope of saving the health and lives of the inhabitants. This habit is scarcely one to be commended, though it may increase the sales of the periodicals concerned. For example, when the Indian Government sent me to the Civil and Military Cantonment at Bangalore in 1896 in order to advise regarding sanitary improvements there, the local press often attacked me unmercifully, though the offensive articles were evidently written by people who had little or no knowledge of the subject. Sir Malcolm Watson, who is now visiting India in the hope of procuring some amelioration of malaria, which kills or cripples millions of people in that country, is, I perceive from press cuttings from the Indian Press, being belaboured in a similar manner by the very people whom he is trying to help, and their remarks are often foolish enough in all conscience. Thus the *Indian National Herald* of Bombay, of December 4, 1928, is very satirical indeed and accuses Sir Malcolm of absurd statements which it is unlikely he ever made, and of aiming at getting a statue to himself placed in that leading centre of human wisdom. Three days previously to that date the same paper ridiculed Sir Malcolm for teaching that the only way to control the disease was to destroy the *Anopheles* which abound in wells and tanks, and the *Herald* tells us that is just what Bombay has been doing for all these years, but that the malaria is still there. Is this true? From what I have heard and read it is *not* true; so that the malaria is naturally still there as before. Probably Bombay would be a more pleasant place to live in if Indian editors could be reduced as easily as Indian *Anopheles*. Of course most Indian papers strongly support our efforts.

Notes and News

The New Year Honour list published on March 1 instead of January 1 on account of the illness of H.M. the King included the following honours: *Baron:* Sir Berkeley Moynihan, President of the Royal College of Surgeons. *Knights Bachelor:* Prof. J. A. Fleming; Col. T. F. Purves, Engineer-in-Chief, Post Office; Mr. A. V. Roe and Mr. G. A. Julius, Chairman of the Council for Scientific and Industrial Research, Commonwealth of Australia. *Companions of Honour:* Lady Barrett, Dean of the London School of Medicine for Women; Miss Lilian M. Baylis of the Old Vic. *K.C.B.:* Mr. L. H. Saville, Civil Engineer-in-Chief, Admiralty. *G.B.E.:* Sir William McCormick, Chairman of the University Grants Committee and of the Advisory Council of the Department of Scientific and Industrial Research. *Dame Commander* (Order of British Empire): Anne L. McIlroy, Professor of Obstetrics and Gynæcology, Royal Free Hospital School of Medicine for Women.

The Council of the Royal Society has recommended the following candidates for election into the Society: A. J. Allmand, Professor of Physical and Inorganic Chemistry, King's College, London; A. H. R. Buller, Professor of Botany, University of Manitoba; C. D. Ellis, University Lecturer in Physics, University of Cambridge; R. A. Fisher, Head of the Statistical Department, Rothamsted Experimental Station; G. R. Goldsborough, Professor of Mathematics, Armstrong College, Newcastle-on-Tyne; James Gray, Lecturer in Comparative Anatomy, University of Cambridge; C. N. Hinshelwood, Tutor of Trinity College, Oxford; A. D. Imms, Head of Entomology Department, Rothamsted Experimental Station; P. Kapitza, Assistant Director of Magnetic Research, Cavendish Laboratory, Cambridge; W. D. Lang, Keeper of the Department of Zoology, British Museum; J. Mellanby, Professor of Physiology, University of London; H. S. Raper, Professor of Physiology, University of Manchester; H. R. Ricardo, consulting engineer; H. R. Robinson, Professor of Physics, University College of South Wales, Cardiff; F. W. Twort, Superintendent of the Brown Animal Institution, London.

The Gold Medal of the Royal Astronomical Society has been awarded to Prof. Ejnar Hertzspring of the Leyden Observatory for his fundamental researches in stellar astronomy.

The Duddell Memorial Medal for 1928 has been awarded by the Council of the Physical Society to Dr. Charles Édouard Guillaume, the Director of the Bureau International des Poids et Mesures, Sèvres. Dr. Guillaume is well known as the discoverer of the nickel iron alloys invar, elinvar, and platinite. Elinvar is used for the hair-springs of watches and platinite

wire coated with copper serves to lead the electric current through the glass bulbs of filament lamps.

The Buchan Prize of the Royal Meteorological Society for 1929 has been awarded to Dr. Harold Jeffreys for the papers he contributed to the *Quarterly Journal* of the Society during the years 1924–7.

The Council of the Geological Society has awarded the Wollaston Medal to Prof. F. J. Becke of Vienna for his work on petrology; the Murchison Medal to Dr. C. A. Motley for his researches on the stratigraphical geology of various parts of the British Empire; the Lyell Medal to Dr. A. Morley Davies for his researches in palæontology, and the Bigsby Medal to Prof. P. G. H. Boswell for his work on the sedimentary rocks.

We have noted with regret the announcement of the death of the following men well known in the scientific world: Prof. J. H. Coulter, botanist at the University of Chicago; Sir W. Boyd Dawkins, F.R.S., palæontologist and archæologist; Dr. H. J. H. Fenton, F.R.S., chemist; Dr. J. W. L. Glaisher, F.R.S., mathematician; Prof. F. P. Leavenworth, astronomer; Prof. E. H. L. Schwarz, professor of geology, Rhodes University College, Grahamstown, S.A.; Sir W. Thiselton-Dyer, formerly Director of the Royal Botanic Gardens, Kew; Sir Henry Trueman Wood, secretary of the Royal Society of Arts 1879–1917.

The Guthrie Lecture of the Physical Society for 1929 will be delivered by Prof. P. W. Bridgman of Harvard University on April 19 at the Imperial College of Science, South Kensington.

The list of sectional presidents of the British Association for the meeting in South Africa this year is as follows: Section A (Mathematical and Physical Sciences), Right Hon. Lord Rayleigh; Section B (Chemistry), Prof. G. Barger; Section C (Geology), Sir Albert Kitson; Section D (Zoology), Prof. D. M. S. Watson; Section E (Geography), Brigadier E. M. Jack; Section F (Economics), Prof. Henry Clay; Section G (Engineering), Prof. F. C. Lea; Section H (Anthropology), Mr. H. Balfour; Section I (Physiology), Prof. W. E. Dixon; Section J (Psychology), Mr. F. C. Bartlett; Section K (Botany), Prof. A. C. Seward; Section L (Education), Dr. C. W. Kimmins; Section M (Agriculture), Sir Robert Greig.

A large number of foreign representatives have accepted invitations to attend the meeting, including Prof. E. J. Cohen and Prof. W. de Sitter.

Sir Richard Gregory has been elected President of the Royal Meteorological Society and Mr. J. E. Barnard, President of the Microscopical Society.

Some little time ago an appeal was made for funds to establish a memorial to the late Sir William M. Bayliss and Prof. E. H. Starling. From the money so obtained a sum of

£2,500 is to be given to the University of London to hold in trust for the maintenance of a Bayliss-Starling studentship to be awarded to graduates in science or medicine for training in physiological or biochemical research at University College, London.

One post-war development has been the realisation of the importance of the science of Acoustics in many branches of industry, and it is a little surprising that hitherto there should have been no society making that subject its special concern. Prospective members of such a society are more numerous in the United States than in this country, and at a meeting held in New York on December 27 it was resolved to form the Acoustical Society of America, to hold meetings for the discussion of acoustical matters and to publish a journal of pure and applied acoustics. Dr. Harvey Fletcher, of the Bell Telephone Laboratories, has been elected temporary president, Prof. V. O. Knudsen vice-president, and Mr. Wallace Waterfall, of the Celotex Co., secretary.

In *Nature* (January 19) is a very brief account of a repetition of the Michelson-Morley experiment by Prof. Michelson, assisted by Dr. F. G. Pearse and Mr. F. Pearson. Three series of experiments have been made with different apparatus in different localities and in all cases negative results were obtained. The final experiments were carried out in a basement room at Mount Wilson, the utmost care being taken to eliminate effects due to temperature changes and flexure of the apparatus. The light path was 85 ft. and no displacement of fringes "as great as 1/15th of that to be expected on the supposition of an effect due to a motion of the solar system of 300 km. per second" was observed.

J. Thibaud's method for the diffraction of X-rays by a ruled metal grating set at grazing incidence has been applied by E. Rupp (*Zeitschrift für Physik*, **52**, 8) to slow-speed electrons (70–300 volts). The spectrum photographs confirm the experimental work of Davisson and G. P. Thomson, the measurements agreeing to within a few per cent. with those expected from the de Broglie theory. No evidence for any polarisation of the electron waves was obtained. Thibaud has been awarded the Hughes prize of the Paris Academy of Sciences for his work on the subject.

The eclipse of the sun on May 9 is important on account of the long duration of totality (5 minutes) at places on the centre of the track which passes over the Malay Peninsula and Siam. To observe the eclipse an expedition left Liverpool for the East on February 16. The main party, consisting of Dr. John Jackson, Dr. Carroll, and Dr. Aston, are to erect a station at Alor Star, Kedah, Malay, while Prof. F. J. M. Stratton and

Mr. P. J. Melotte are taking the large astrographic telescope from Greenwich Observatory to Pattani on the east coast of Southern Siam. Measurements of the deflection of light by the gravitational field of the sun again forms an important feature of the programme, and it is proposed to take a series of photographs (i) of stars close to the sun, (ii) of stars some little distance away alternately with the same apparatus, to ensure that any shifts which may appear in the former case are not due to accidental effects such as temperature changes in the optical system. Spectrographs of the solar atmosphere and photographs of the corona will also be obtained if only terrestrial conditions permit.

In a letter to *Nature* (January 26) Prof. W. J. de Haas of Leyden gives the results of his latest observations on the super-conductivity of metals. It appears that this phenomenon is not limited to pure metals, but may occur with alloys and, what is more, with alloys of metals which alone do not exhibit this property. Thus neither gold nor bismuth is super-conducting (at least above 1·5° A.) but their eutectic is. Its resistance at 2·1° A. is 0·7 of the value at room temperature; but at a temperature 1/20° lower the resistance has become too small to measure. Combinations of antimony with tin and of bismuth with thallium (the second metal only in each pair being super-conductors) are super-conductors. Copper and tin (Cu_3Sn) does not become super-conducting, while the resistance of Ag_3Sn decreases from 3·4° A. to 1·3° A. without vanishing. Prof. de Haas considers that these results tend to confirm his view that the super-conductivity of metals depends on their atomic weight and possibly on their zero-point energy as well as on the electron configuration in the atom.

Very great interest has been aroused by the announcement that the International Education Board of New York is providing the funds for the construction and erection of a reflecting telescope with a mirror 200 inches in diameter for the California Institute of Technology. The largest telescope at present available is the 100-in. reflector erected at Mount Wilson some ten years ago, and the great advances in astronomical science during these ten years are in no small measure due to that instrument. Upon the diameter of the object-glass depends the light-gathering power of the telescope. More light means that fainter stars and fainter nebulæ can be photographed both in our own galaxy and in galaxies beyond. Determinations of the surface temperatures of the stars, their sizes, distances, motion, and constitution all depend on the size of the object-glass. It is hoped that the new telescope will reach stars of the twenty-fifth magnitude, *i.e.* stars whose brightness is about that of a single candle 40,000 miles away.

The construction of the telescope presents problems of the greatest difficulty and could only be attempted by an organisation with almost unlimited funds at its disposal. The design, the manufacture of the mirror, the mounting of the telescope, and its site will require all the knowledge, skill, and care which modern science can supply. The work is to be carried through by the co-operation of the staffs at the California Institute and at the Mount Wilson Observatory. To give maximum space-penetrating power, a short focal length—about 3·3 times the aperture—will probably be used, while it is hoped to make the mirror of a rough disc of fused quartz coated over the front surface with a thin layer of fine transparent quartz. So far no discs more than 2 ft. in diameter have been made in this way, and to make a 17-ft. disc weighing between 25 and 30 tons will obviously need much long and expensive experimentation. The surface has to be ground and polished to an accuracy of 1/500,000th of an inch, the disc has then to be mounted in its tube, so that it can be swung and observations made at the principal focus fifty-five feet from the surface of the mirror. Finally a site has to be found where the atmospheric conditions are uniform, so that the "seeing" is good or the "twinkling" of the stars a minimum. The building is to be of steel with double walls to minimise the daily range of temperature; it will be about 150 ft. in diameter and 150 ft. high to the top of the dome, with the telescope mounted on a pier some 50 ft. from the ground.

Agricultural Research in 1927 (Royal Agricultural Society of England, price 1*s*. 3*d*. post free) is the third annual summary of all scientific and economic research in agriculture likely to be of interest to the agriculturist in this country. The publication started two years ago at the instance of the Research Committee of the R.A.S. and has already proved a most valuable addition to the periodical literature of the farming industry.

Farmers are naturally at a disadvantage not only in interpreting the results of scientific work but also in knowing where to look for them. They appear in a great variety of forms, in periodicals not always accessible to the farmer and couched in language not always easily understood by him. His work lies around him on his farm; he has not the time nor always, in these days, the means to study a multiplicity of journals and pamphlets, and thus it happens that the results of research work are often too slow in reaching those who are most concerned to know about them. Collected from all available sources, put together by acknowledged authorities in non-scientific language, the contents of *Agricultural Research* should be studied by all those who wish to keep themselves abreast of the times in modern agriculture.

It is impossible to refer to all the subjects dealt with in this publication. Arable farmers will be interested in the article on varieties of English cereals which forms the opening section of Mr. Engledow's review of "Crops and Plant-breeding," whilst the section on grass mixtures is of particular importance to him at the present time when so much plough land is being laid down. Mr. Mackintosh has some useful notes on the interpretation of milk records, which may be a pitfall to the purchaser of dairy stock if taken too literally and without due allowance for certain important factors. Under the heading of "Agricultural Economics" Mr. Orwin deals with such matters as the economy of baby beef production, whilst this section also contains a brief but useful summary of the economic position and the trend of events in the agricultural industry at the present time. In these days of high labour costs and low prices the farmer is turning more and more to the use of machinery in the hope of increasing the output of labour, and the information contained in Dr. Owen's contribution on "Agricultural Engineering," dealing as it does not only with the efficiency of old and well-tried implements but also with new inventions such as those for rotary cultivation, the combined harvester, and the potato harvester, will receive close attention. Dr. Crowther, as would be expected, has much to say on recent work in "Animal Nutrition," and "Soils and Fertilisers" form the subject of a valuable review by Sir John Russell. His recent tour of farming in Australia has enabled him to make some interesting comparisons of experiences in that continent. The volume concludes with Sir John McFadyean's summary of the progress of "Veterinary Research" during the year. Unfortunately, a cure for the foot-and-mouth disease scourge is still to seek, but the summary makes it clear that research in this as in other diseases is being vigorously pressed. Dairy farmers should study carefully the advice given as to the best methods for combating Epizootic abortion.

For the assistance of students and others desirous of consulting original sources of information, each section of *Agricultural Research* concludes with a complete bibliography of papers quoted. The Royal Agricultural Society, it should also be noted, has reduced the price to 1*s.* 3*d.* post free, in order to secure the widest possible circulation.

The Report of the Building Research Board for the Year 1927 (H.M. Stationery Office, price 3*s.* net) contains a very complete summary, illustrated with numerous diagrams and plates, of the work of the Board during the year under review. Some of its contents are, of course, of interest only to the architect and surveyor; but the layman who owns a house or is building one will find much interesting and important information.

For example, there is (p. 17) a discussion of the effects of Portland cement mortar when used for bedding bricks or pointing brickwork, and (p. 19) an account of the ill effect of placing blocks of sandstone and limestone in juxtaposition. The section on structures and strength of materials contains, *inter alia*, a description of experiments on the crushing strengths of bricks, showing that Pressed Blue Staffords are much superior in this respect to any of the other types tested. An investigation of the problems of ventilation showed that a roof or wall ventilator is as effective as a fire flue when no fire is alight. The last section of the report summarises the results of six years' use of the experimental cottages erected at Amesbury, Wiltshire. Those about to build or buy houses containing any of the post-war structural novelties should certainly consult the report before proceeding further, for not all of those tried at Amesbury have been successful.

The Reverend R. J. Campbell, D.D., contributes an article to *John Bull*, January 26, 1929, in which he maintains that the pioneer should detach himself from any thought of reward in any service rendered to mankind, and illustrates this thesis by pointing out the small reward received by Columbus. On the other hand, it may be maintained that the public ought always to try to furnish some kind of reward for services rendered to it, as an act of justice. Not only this, but the writer referred to seems to draw no distinction between the general pioneer and the medical pioneer. Medical men live by the rewards which they obtain from patients, and it is considered an ungraceful act in a patient not to pay his doctor's fees, if reasonable. What is the ethical difference between a medical pioneer who shows us how to prevent or cure some world-wide disease and a doctor who confers benefits on private patients? Why should a medical pioneer who has perhaps spent his life in developing some branch of medicine receive nothing, while many doctors can apparently do very well by treating private patients? Why should the medical benefactor particularly look upon the services which he renders as their own reward and expect no other? Of course this is a very pleasant doctrine for those who receive the said benefit to escape giving anything for it, and may be called "homely wisdom" by some, but is more commonly called "chousing the doctor." Besides, this was not the treatment of Columbus by the Spaniards a very ungenerous one, which has always been held up as an example of human ingratitude and has, perhaps, diminished the honour and glory of the Spanish people in the eyes of the world ever since—like their treatment of Cervantes and many still more evil actions of other nations. Fancy sending Columbus home in chains from the scene of his discovery—much to

the horror of Ferdinand and Isabella, it is true. The British are sure to receive similar blame in the future for their similar treatment of men like Faraday and many others.

One of the most surprising things about our "civilisation" is the fact that those who rule us allow people to be imposed upon by all kinds of quack remedies which do not confer benefits on their health, but which may fill the pockets of the vendors. A particularly bad case of this kind has been investigated in the *Bulletin of the Institute for Medical Research*, Kuala Lumpur, by Dr. R. Green, who has studied laboriously the actual effects of a proprietary medicine on ten cases of malaria in Malaya, and has demonstrated that the said medicine had no effect at all in the cases, which, after remaining ill for a number of days in spite of this wonderful "cure," were at once rid of their fever by ordinary doses of quinine, as usual. The vendors of the quack medicine had added to their sin by declaring, quite untruthfully, that quinine is dangerous and that cancer can be produced by arsenic, which is also frequently prescribed by doctors. Vendors should not be allowed to protect any kind of remedy by patent or otherwise unless they can produce evidence that their nostrums are really of benefit, and Governments should not be encouraged to protect such frauds without receiving competent advice on the accuracy of the statements advertised.

We have already called attention to the subject of Malaria in Barbados in SCIENCE PROGRESS, Vol. XXIII, No. 89, p. 143. *The Tropical Diseases Bulletin* for 1929, No. 1, p. 1, describes two further papers on the subject, by Dr. E. A. Seagar, from which it is obvious that serious bungling has recently occurred in the island, allowing the disease to enter into a place where it was previously unknown.

On the 12th December last Prof. G. Franchini lectured at the Royal Society of Medicine on the subject of Marcello Malpighi, and showed a number of lantern slides illustrating him and his brilliant labours for science, including microscopy. But Dr. R. T. Gunther of Oxford wrote to *The Times* a day or two later saying that our own Robert Hooke of Westminster School and Christchurch deserves the title of the real "Father of Microscopy." It is characteristic of this country that we should give away as much of the credit as we can of our countrymen to foreigners, and it is really time that the world should try to find out to whom it is most indebted for the discovery of the microscope. There is also a third claimant, namely Leeuwenhoek, and we wish that someone who has studied the matter would undertake to adjudicate between these various workers.

Major E. E. Austen, Keeper of Entomology at the British

Museum (Natural History), Cromwell Road, S.W.7, wrote to *The Times* on the 3rd November last, calling attention to the fact that, when the sun shines through a water bottle full of water, the bottle may cause the rays to burn wooden surfaces on which they fall. He asserts that he has seen thin spirals of smoke rising behind water-bottles inadvertently left for a short time fully exposed to the sun in summer. We did not know that there was any sun in England, but suppose that it does burst out sometimes by accident. We even possess a wooden reading-desk scarred all over with small burns produced by a large magnifying reading-glass standing on a table where it was reached by the evening sunlight last October.

We are shocked to learn through the usual Annual Reports that very little effect on malaria has yet been produced by sanitary measures directed against it. For instance, we find from page 24 of the Report of the Public Health Administrator of Burma for 1927 that the deaths recorded under "Fevers" number 75,321 for the year, or 2,531 more than 1926.

Obviously the British Empire has not even yet begun to look upon malaria as a preventible disease.

CORRESPONDENCE

To the Editor of SCIENCE PROGRESS

SCIENCE AND METAPHYSICS IN BIOLOGY

From P. CHALMERS MITCHELL, D.Sc., F.R.S.

DEAR SIR,—In your October number Mr. J. H. Woodger, reviewing my Huxley Lecture, opened by a lengthy allusion to the mental behaviour of great apes. Like that of great apes and small, my attention easily wanders. In reading his Essay-Review, it wandered so often towards guesses as to why he had written certain things that the most convenient response to your kind invitation to write a " reply " will be to indicate some of the points at which I was left wandering in wonder.

First, can any serious student of science and philosophy believe with Mr. Woodger that the recent changes in the fundamental conceptions of physics are due to philosophical criticism and not to mathematical discipline and experimental observation? Einstein and Rutherford, not the epistemologists, have been the compellers.

Can any serious student of science and philosophy really believe that " the nearest approach we ever make to protoplasm is in the mixture obtained by grinding up cells with sand in a mortar "? Even Bergson did not try to " put across " crudities quite so blatant.

Lastly, can a critic be taken seriously who introduces a parade of familiar difficulties about the doctrine of an original abiogenesis by saying that I had " given scientific assent to it " when I had said that it was a " speculation without a shadow of proof "?

Yours faithfully,
P. CHALMERS MITCHELL.

ZOOLOGICAL SOCIETY OF LONDON,
December 3, 1928.

ANCIENT SYMBOLS FOR METALS

From J. WHITE, M.Sc.

DEAR SIR,—A short while ago I was much impressed by the marked similarity between our present numerals and the ancient symbols for the metals.

I give below the symbols, transcribed as faithfully as possible, from Campbell Brown's *History of Chemistry*, and the figures I derive from them.

☉	☾	♀	♂	♃	♄	♁	☿
Gold (Sun).	Silver (Moon).	Copper (Venus).	Iron (Mars).	Tin (Jupiter).	Lead (Saturn).	Antimony	Mercury
0	1	2	3	4	5	6	7

8 and 9 could be made by starting the rows again in twos as $\overset{\odot}{\odot}\ \overset{\odot}{☾}$.

I at once began to see if this order could be justified by any sequence common of old, such as the planets which ruled the days of the week, the order of value, use or rarity of the metals, etc., but so far I have found none.

Of course, I am well aware that our numerals are usually regarded as of Arabian origin, but there is much indefiniteness in the matter. Also, I know of the commonly accepted origins of the symbols, such as the caduceus of Mercury, the looking-glass of Venus, the shield and spear of Mars, etc., origins which were I believe first tentatively offered by M. Berthelot in his *Introduction à l'Étude de la Chemie des Anciens et au Moyen Âge*. But for all this I cannot help feeling that the resemblances, especially in the case of two or three, are too marked to be accidental, and at least I hope that the matter may provide interest or amusement or both to the readers of your worthy journal.

Yours faithfully,

John White (M.Sc.)

Bec School,
Beechcroft Road,
December 17, 1928.

ESSAY-REVIEW

MALARIA, THE HEALTH AND WEALTH WASTER. By Col. W. G. KING, C.I.E., I.M.S. (ret.), late Sanitary Commissioner for the Government of Madras. Being a Review of **Studies on Malaria**, by Col. Sir RONALD ROSS, K.C.B., K.C.M.G., F.R.S. [Pp. xi + 196.] (London: John Murray, 1928. Price 5s. net.)

IN the course of evolution from monkeydom, the brains of mankind, notwithstanding the anatomical similitudes to those of their forbears, display diversities of capability. Were it otherwise, there could be no room for free-will in human beings as contrasted with Robots. In adjudicating on mental powers, psychology identifies traits of character conventionally regarded and libellously entitled "animal instincts," in the effort to aggrandise the more fully developed human being at the cost of other valuable products of Creation. But when exploring the higher grades of mentality in search of a standard connoting the active co-operation of all known spheres of brain functioning, success has not been attained; the theory of free-will, or organic automatic machinery as the sole choice, explains the position. Yet there are multitudes of individuals possessed of at least one quality of mental power far above the average of their fellow mortals, and, in less numbers, many in whom several highly developed qualities co-exist. The mentality of the author of *Studies on Malaria*—Colonel Sir Ronald Ross—must certainly be classed with the latter complex. He is a recognised poet, litterateur, mathematician, and biologist, and last, but not least, an ardent *practical* philanthropist. A book whose title embraces so portentous a term as "studies" runs the risk of being put aside as being of the high-brow variety, and as of utility solely to medical men dealing with malarial fevers in the tropics. Such a decision would be erroneous. The author's ideal is that of the sanitarian—"prevention is better than cure"—and he is therefore able to convey his advice devoid of the technicalities of Curative Medicine to those who in their own interest, or that of communities committed to their charge, can, and should, do their utmost to extirpate malaria—the health and wealth waster.

Cholera and plague deal death quickly, but malaria plays with its victim as a cat does with a mouse; it permits intervals of rest between onslaughts, but no carnal "pact" is offered

that peace is assured. Surely if slowly, it saps physical and mental powers upon which the daily task depends, drives agriculturists impecunious and wellnigh childless from their villages, renders impossible or vastly increases the cost of execution of great engineering works, and forbids active interchange of commerce to the detriment of nations. The death-rate directly ascribable to attacks is not heavy; but these render sufferers devoid of resisting powers to the inroads of other diseases, and thus materially increase the totals of mortality in the areas concerned. Statistics gathered in India, where effective anti-malaria measures have been but sparsely applied, afford ample examples of such conditions. Approximately, four million deaths from "fevers" are registered in British India annually. At a very modest estimate, it is officially admitted that about one million of these are *directly* due to malaria. When based on the lowest possible data and without taking into account the value of an adult producer, this amount of mortality with its coincident period of sickness causes annually a loss in unproductive expenditure in India of £20,298,715.[1]

The Army Sanitary Commission of 1859–63 found that British soldiers died in India at the rate of 69 per mille of strength, or 60 per mille in excess of the mortality of those serving in Great Britain. This, they represented, implied that, since the first occupation of the country, "a Company of each Regiment has been sacrificed every 20 months, and, with a total strength of 70,000, there necessarily existed in hospitals 5,880 beds constantly full of sick. "From such data they estimated that the financial loss due to the mortality of previously trained men, *plus* cost of their transport to India, amounted to £1,000 daily, and that 5,037 recruits per annum would be required "to fill up the vacancies caused by death alone." In discussing causes of this heavy mortality, they held that whilst "there are many other agencies at work besides those due to climate *per se*; first among them we must place that subtle, unknown agent, or rather that cause of disease known only by its effects, malaria. . . . *It underlies the cause of many other diseases.*[1] Whether it may ever be possible to banish this scourge from India, it is hardly within our province to inquire and the question could hardly be answered." . . . *If, however, by use of suitable measures that time did arrive,*[2] they stated, "we may cherish the hope that the natural death-rate in times of peace of men of the soldier age in India will be no more that 10 per 1,000." They pointed

[1] This estimate was made by Mr. Stott, the well-known skilled accountant of Liverpool, on carefully ascertained expenditure in the now exceptional class of Indian labourers earning the minimum wage of Rs. 5 per month.

[2] Italics not in the original.

to the recognised beneficial influence—notwithstanding the then existing ignorance of the *causa causans*—which had "in England and elsewhere followed the use of soil drainage and culture." *This advocacy of drainage is several times repeated in the Report of the Commissioners.* One of these, Dr. Sutherland, afterwards served on a Commission to ascertain by what means the French had managed to reduce the appalling mortality of their soldiers in Algeria, where malaria was rife. Here he found drainage had been systematically employed with splendid results. In describing conditions in the neighbourhood of Lake Halloula, the Commissioners state "the effect on the neighbourhood has been decisive. The villages which formerly suffered severely from remittent fevers—often taking the fatal form of pernicious fevers—are now healthy, and *they have been free from swarms of mosquitoes*,[1] which during a part of the year made life almost intolerable." Thus, the Commission were "so near and yet so far" from ascertaining definitely that, in the presence of mosquitoes, there existed the link in tracing causation. A similar failure to make the necessary induction is evident in the case of Dr. Benjamin Rush in his "Account of the Bilious remitting Yellow Fever" in Philadelphia, in 1793. He regarded the effluvia from a mass of rotting coffee in the docks to be the origin of the fever, but in describing atmospheric conditions at the time states, "Moschetoes (the usual attendants of a sickly autumn) were uncommonly numerous."

In 1881, however, the mosquito ran a closer chance of being convicted as the conveying agent of yellow fever, when Dr. Charles Finlay, of the United States of America, made experimental transmission of that disease. He obtained no definite result owing to lack of knowledge (gained in later years by the U.S.A. Reed Commission, encouraged to again test mosquito agency by Ross's results against malaria) that the virus is not effective under twelve days' sojourn in the body of the mosquito. Undrained soil was so closely connected with malaria in empirical experience, that, in the absence of proof of their guilt, the latter became the subject of various hypotheses. One regarded them as sent by Providence to scare human beings from the neighbourhood of malarious soil, another by A. F. A. King (also of the United States of America) suggested that when bred in marshes they inoculated human beings with malaria by their bites—a suggestion which the author states was "nearer, but not quite, the truth." By 1894, Manson, "the Father of Tropical Medicine," impressed with the Empire importance of malaria as a factor in health and wealth, and now armed with Laveran's discovery of a malarial protozoon

[1] Italics not in the original.

in human blood, collected all available opinions and framed an hypothesis (doubtless with natural bias towards details which accrued from his original work as to the *filaria sanguinis hominis*) that the mosquito was the conveying agent by medium of air and water.

In the meantime, in accordance with the advice of the Royal Commission of 1859–63, a Sanitary Commission had been appointed for each Presidency, and, within four years, a rapid decline of mortality occurred in the British and Indian Armies and in jails, and, for the civil population, public health schemes of excellent character had been proposed. But the pace was too great for the Government of India, and their powers were steadily clipped. As to the civil population, it was apparently held that it was inadvisable to countenance large sanitary operations in the midst of uneducated populations liable to misinterpretation of their intent. Hence, education was pressed forward of males to the neglect of females—the rulers of the household. In this the policy of the Government of India and that of its predecessor—the H.E.I. Coy.—present a contrast. Under the latter in 1845, the woeful condition of agriculturists in malarious localities secured special inquiry and concerted measures for drainage, yet they accepted, without special effort towards amelioration, the ghastly death-rates of their soldiers and civil employees as inevitable in a tropical climate. It was then that Dempster[1] (not Koch as is usually believed) represented that the condition of children presented a good index of malaria incidence, as ascertained by examination of spleen enlargement.

Ross joined the Indian Medical Service in 1881, and in course of duty, necessarily was brought in contact chiefly with subjects of malaria in men of the selected physique expected of soldiers. In the Army (British and Indian) a great reduction in the death-rates had occurred between this period and the date of issue of the Report of the Royal Commission of 1859–63 above referred to, consequent upon decided improvements in accommodation, water-supply, food, clothing, etc. By that time also, the elimination of typhoid from the "fevers," as shown to be requisite by Cornish (then Sanitary Commissioner for the Government of Madras), had been accomplished, but the influence of malaria remained unabated in the civil population, and still existed in embodied men as *the principal cause of labour disability and sickness*. By 1890 he had acquired sufficient knowledge of the civil population to become deeply sympathetic on witnessing the misery and poverty which

[1] The Canal Medical Committee of which he was a member adopted his recommendation. This committee travelled 1,400 miles, visited 300 localities, and personally examined 12,000 individuals.

followed recurring attacks of malaria. The philanthropy item of his complex mentality asserted itself as a suitable ally of the poetic tendencies which had, in the intervening years, threatened to become supreme in his career, and he determined to devote himself to the problem of malaria prevention. In a diary maintained in suitable form to soothe the poetic "urge," he thus recorded his resolution :—

> "The painful faces ask, can we not cure ?
> We answer, No, not yet ; we seek the laws.
> O God, reveal thro' all this thing obscure
> The unseen, small, but million murdering cause."

That ideal he pursued firmly in the midst of glimpses of possible success and the crash of failure till, in May 1897, he devoted his hard-earned leave to visiting the notoriously malarious Sighur Ghat. Here mosquitoes, air, and water *inter alia* brought definite factors into the problem—including, to his delight, the desirable factor of being himself infected with malaria—which permitted their respective valuation by the mathematical item of the "complex." The advance thus secured was adverse to the hypothesis as to transmission by air and water framed by Sir Patrick Manson. But in 1897 the entomologist had no special interest in mosquitoes, and Ross had before him a search equivalent to an attempt to find a needle in a haystack, when endeavouring to identify, in the midst of legions of mosquitoes, the undescribed tribe of the one he had incriminated in the Sighur Ghat.

It was evident this research could not be conducted were it subjected to the interruptions occurring when on military duty ; hence the Government of India was besought by various authorities to place Ross on "special duty" for the purpose, but it refused to comply, in the face of repeated pleas, till the lapse of eighteen months. In a few weeks at Secunderabad he then was able to show that the Sighur Ghat clue of May 1897, confirmed by two subsequent specimens of a mosquito on August 20, 1897, had enabled him by this subsequent work to solve the malaria problem. This practical philanthropist on the day he obtained this clue, in full knowledge of the importance of his discovery to mankind, indulged in no visions of honours or riches accruing to himself therefrom, as shown by the following entry in his diary :—

> "This day relenting God
> Hath placed within my hand
> A wondrous thing ; and God
> Be praised. . . .
> I know this little thing
> A myriad men will save.
> O Death, where is thy sting ?
> Thy victory, O Grave."

Having proved to the satisfaction of scientists that the mosquito affords shelter for the *Plasmodium malariæ* and facilitates its transmission to human beings, according to ordinary precedent, he should have rested satisfied that he had added another page to the ever-increasing volume of science. But that would have been a mere stage in the task he had undertaken. As a unit the whole complex possessed a stern C.O.—a martinet—Duty, whose orders Poetry had held before Ross in his years of toil and doubt. He still visualised one of the incentives to his unwavering obedience as recorded in his book of poems entitled *Philosophies* :—

> . . . An Indian mother there, . . .
> A three-year child well nourished at her breast
> Wither'd with famine, still she fed and press'd
> For she was dying. "I am too poor," she said,
> "To feed him otherwise"; and with a kiss
> Fell back and died. And the soul answered,
> "In spite of all the gods and prophets—this."

In a paper[1] read before the Royal Institution of Great Britain in 1920, he stated decisively that this investigation of malaria was one to which he had no personal inclination—"I undertook it as a duty and at considerable loss to myself." This being so, the next step logically was the application of the new-found knowledge to the *prevention of malaria.* Hence he applied to the Government of India to afford him facilities for demonstration of preventive measures in a community. The Government which had put aside the giving him the opportunity of research, could not now be induced to allow this demonstration of its utility. The aggregate result of seven years of labour embodied in a Report to that Authority was therefore destined to be the further repletion of secretariat pigeon-holes. But Duty was still in command, *and at the sacrifice of acceptance of a mere increment of the pension* possible for further periods of service he retired from the I.M.S.

Freetown and Ismalia were then utilised as exemplification of protection of communities from malaria. The results left no doubt as to the efficacy of his methods which the Government of India, oblivious of the annual toll of deaths from malaria, had considered unworthy of entertainment. By this time also there arose the usual sequence of announcement of a discovery—attempts at belittlement or the claiming of priority. In the latter respect, an Italian savant—Grassi—particularly distinguished himself in the face of clear proof of his error.

Whether the reader has any interest in malaria prevention or not, there will be found in page after page of the book an

[1] *Science and Poetry*, June 4, 1920.

enjoyable quiet humour of a nature which betrays the partly Scotch descent of the author, in that whilst the *nemo me impune lacessit* dictum is rendered obvious to an adversary, it is difficult for him to define the particular prickle of the thistle which bestirred him ; in the case of Grassi, however, no prickly flowers of that ilk grown in the North are offered ; for him is the thrust of the skean-dhu.

But the reader may well wonder when Ross wrote the lines, "I know this little thing a myriad men will save," whether the mathematical item had failed to check poetic licence. As a matter of fact, the term " myriad " unless accepted as implying conventional "umpteen millions" demanded mathematical expansion, and not only so as regards the saving of lives but of millions of money, and all that the symbolism of that useful commodity effects in the prosperity of nations.

The research carried out by Ross, in the midst of apathy by the Government of India, and his ultimate success placed him definitely side by side with the world-wide benefactors of mankind, Jenner, Simpson, and Lister. That is a truism up-to-date little appreciated by a British public which has long forgotten the experience recorded by Sir Gilbert Blane that "the mortality in London from ague during 1558 was so great that the living could not bury the dead," whilst there must still be living those who have encountered indigenous cases of the disease in the Fens of Lincolnshire, and on the lower stretches of the Thames. Systematic drainage and culture of the soil for agricultural purposes has seriously interfered with the breeding facilities of Anophelines and therefore with malaria transmission, but certain areas of the country still remain suitable for the purpose. There was no lack of malaria-bearers in the thousands of men who reached the Homeland during the Great War, and these particular localities might, therefore, have become excellent centres for a malaria outburst. Why this did not occur was because the Ministry of Health, armed with the knowledge gained by Ross, was able unostentatiously to undertake anti-malarial measures, and in like manner, America, in the neighbourhood of camps for assembling troops, and, in Queensland, Australia, of those for returning troops, took precautionary measures. That the danger was real is shown by the fact that, in England, 178 indigenous cases were detected. Measures, if any, undertaken by Soviet Russia[1] could not have been such as to court success as, following entry into the country by famine refugees from Turkestan, the inhabitants of a huge tract of country stretching from " Nijni Novgorod and Moscow on the north, Rumania on the west, and

[1] M. D. Mackenzie, Senior Medical Officer, Friends' Russian Famine Relief Unit, S.E. Russia, Nov. 1923.

the Black Sea on the south" were infected to the extent of 60 to 70 and, in some districts, 90 per cent. It may, therefore, be assumed that even this little island reaped protection from the labours of Ross; "a stream can be stopped at its source by a twig. Let it flow and it will drown an elephant."

With a map of the world before him showing the enormous areas where malaria is recognised, the reader cannot fail to understand that the subject is of first-rate importance to the British Empire, of which so great an area is within the tropics, and that if it be true, as contended by the Royal Commission, where it prevails "it is found that the diseases of important organs, the consequence of malarious fevers, occasion much of the subsequent sickness, mortality and invaliding," the extermination of malaria is essential from a humanitarian, commercial, and national point of view, in whatever country it is found.[1] Reference to conditions at present existing in India should, however, suffice to convince the home-staying Briton of this truism. There can be but few families in Great Britain whose members, in this or former generations, have not encountered in India the risks attending attacks of malaria, as an *underlying cause of other diseases and labour disability*. The attitude of the Government of India, since the days of the H.E.I. Coy., has been adverse to the radical measure of drainage against malaria,[2] whilst, if they looked to the education of the people,[3] for which cause so large funds have been allotted, as a better way to lead them to undertake the necessary measures, they have been mistaken.

[1] John Huxham, M.D., F.R.S., writing in 1779 concerning results of the then existing endemic malarial fevers in Great Britain, states they may "end in dropsies, jaundice or universal obstruction of the viscera of the abdomen, and frequently in diseases of the *genus nervosum*." (*An essay on fevers by John Huxman, M.D., F.R.S., p.* 21.)

[2] "I got the impression that quinine is relied on as the sheet anchor, that screening takes the second, and antimalarial drainage the last place." (*Report on the League of Nations interchange of Health Officers in India, Jan.* 1 *to Feb.* 18, 1928.)

[3] The Madras Municipal Corporation has for its Indian Members highly educated men. The Health Officer of the City (1926) thus reports their treatment of anti-malaria precautionary measures: "That Madras is an endemic area for malaria and that there are plenty of breeding-grounds for malaria have been amply proved by an investigation, consequent on the outbreak of malaria in an epidemic form in 1912 and 1913." A campaign against malaria continued to work on actively till April 1921, when it was told that it was "an idly busy department" and was practically wiped out. Subsequent to this condition, he states, "The increase in this disease is rather striking. The high death-rate from this cause was reported to the Standing Committee (Health). . . . The Standing Committee . . . on May 6, 1925, sanctioned the Health Officer's proposals for an allotment of Rs. 5,000, and 'the Council sanctioned the proposals only in January of the current year.'" (*Journal of Tropical Medicine and Hygiene*, March 1, 1928, p. 17.)

But the medical officers of both British and Indian troops and jails have not failed in advising such minor measures as have been possible within their charges, and their efforts have been sufficiently fruitful to show the great financial saving which by this time might have been effected, had radical measures been instituted for the practical extinction of malaria amongst embodied men. The work of Gorgas during the construction of the Panama Canal and by Sir Malcolm Watson in Malaya are well-known examples of the economical adaptation of the principles enunciated by Ross. It need not be said that one of the after-effects of the Great War was an increase of admissions for malaria in the British Army Hospitals in India, and that there have been fluctuations since then due to relapses and exposure of troops incidental to manœuvres, but taking the period after the announcement in medical professional papers of the discovery by Ross, of 1898 to 1914—the year of declaration of war—it is more than a coincidence to find that whilst in the preceding decade the admissions for malaria were 485 per mille of strength, in that ending 1908 they had decreased to 244 per mille, and in the period 1910–14 they were not more than 117 per mille. Coincident with these diminishing rates of malaria admissions, there was a steady decline of death-rates from all causes, namely, from 14·84 in 1888 to 4·32 in 1914.

In the "Report of the Health State of the Army" for 1926, it is stated, "Malaria is still the principal cause of sickness among troops in India" (and in prior Reports the same fact is demonstrable), and to this dictum is added, "and consequently a great financial loss to the State." If, then, malaria be both a health and wealth waster and it be preventable—why not prevented? The reason afforded in this Report is that, "it is difficult to eradicate malaria from military stations without spending large sums of money."

Gorgas extirpated malaria at a cost of one cent per head of the population dealt with, and in Malaya Sir Malcolm Watson reports that the planters have, at their own expense, gladly undertaken drainage with resulting profit, by their labourers being retained in a working condition and their obtaining labour at far cheaper cost than could be secured when the imported coolie realised that wages, accompanied by risk of encountering malaria, must be high indeed. Rather than face the bogey of drainage, the Government of India contemplate transporting the troops to and from their stations to the hills so as to escape the markedly malarious months—*an annual* expenditure which, if multiplied, might surely cover any reasonable scheme ascertained not solely from estimates made by engineers, but by malaria experts *and* engineers *after due*

consultation.[1] A striking fact is that whilst the death-rate of the men of European troops in India was in the East India Company's days vastly greater than for officers, the reverse is now the case—a reasonable explanation of which is, seeing that malaria forms the chief single cause of hospital admissions, that the officers as the more free agents are less influenced by the minor anti-malarial measures now practised than are the men who dwell in barracks under supervision.

If then minor measures only can be expected from the Government of India, there can be no doubt that the more complete they are, the safer the individual from the bodily breakdown which repeated attacks of fever connote. In no work which has been published up to date is the whole question entered into more clearly and concisely than will be found is the case in the latter part of *Studies on Malaria.* As a writer, Sir Ronald Ross possesses the rare virtue of conveying complete ideas in a single sentence, and, as master of matters malarial, all requirements are set forth by him in detail and yet devoid of technicalities. *Hence those who have friends and relations serving in malarious areas should not hesitate to send this little book for their guidance.* In this way, they might not only save them suffering from malaria, but from pecuniary loss, and in the case of those serving in the British Army (which the Government of India cannot afford to protect from that malady), they might also save their private purses being depleted by "Hospital stoppages," which amounted to a total of Rs. 266,679 from officers in 1927. This amount went to the credit of the Government of India for admissions to hospital for treatment of maladies the chief single cause of which was malaria, for which, as a preventable disease, the Government is morally as liable for results as the owner of a factory or mine would be who satisfied his financial conscience by incomplete schemes for the prevention of accidents.

[1] Cf. *Progress of Sanitary Measures in India*, 1912, pp. 43, 58, 93.

REVIEWS

MATHEMATICS

Non-Riemannian Geometry. American Mathematical Society Colloquium Publications, Vol. VIII. By L. P. EISENHART, Professor of Mathematics in Princeton University. [Pp. viii + 184.] (New York: American Mathematical Society, 1927. Price $2.50.)

THIS book is the sequel to Prof. Eisenhart's *Riemannian Geometry* published in 1926. It describes the advances made in the generalization of Differential Geometry along the lines followed by the author with Veblen, J. M. and T. Y. Thomas in America and by Cartan and Schouten in Europe. Various geometries, suggested by Eddington, Einstein, Weyl and others as basis of a combined theory of electromagnetism and gravitation are particular cases of the geometry here considered; so also is Riemannian Geometry.

In Riemannian Geometry, following Riemann's famous inaugural lecture at Göttingen in 1854, a fundamental form $g_{ij}dx^i dx^j$ is assumed, giving the distance between two neighbouring points of the space of co-ordinates x^i; the properties of the space and its sub-spaces are then developed from this form and Levi Civita's definition of Parallelism. Levi Civita defines the vectors, whose components at any point are functions λ^i of the co-ordinates of that point, to be *parallel* with respect to the curve $x^i = f^i(t)$ when the components satisfy the equations

$$\frac{d\lambda^i}{dt} + \begin{Bmatrix} i \\ jk \end{Bmatrix} \lambda^j \frac{dx^k}{dt} = 0,$$

where $\begin{Bmatrix} i \\ jk \end{Bmatrix}$ are the Christoffel symbols of the second kind—functions of g_{ij} and $\frac{dg_{ij}}{dx^k}$ and i, j, k take all values 1, 2, ... n for space of n dimensions.

The geometry here discussed is non-Riemannian since distance is not fundamental—there is no metric. Any set of general functions of the co-ordinates, L^i_{jk} (called coefficients of connexion) obeying the same transformation laws as the symbols $\begin{Bmatrix} i \\ jk \end{Bmatrix}$ in a Riemannian space, are assigned to the space, which is then said to be *affinely connected*. Parallelism with respect to a curve is defined by the above equation with L^i_{jk} written in place of $\begin{Bmatrix} i \\ jk \end{Bmatrix}$, and all parallel vectors are found to remain parallel when the connexion of the space is changed to that given by $L'^i_{jk} = L^i_{jk} + 2\delta^i_j\psi_k$—where ψ_k are components of an arbitrary vector and δ^i_j are the Kronecker deltas. A vector displaced parallel to itself round an infinitesimal circuit returns to a position whose difference from the initial position depends on the "curvature" tensor L^i_{jkl}, a function of the coefficients L^i_{jk} and their differential coefficients with respect to the co-ordinates.

The foregoing results and others are investigated in the first chapter when

the coefficients L^i_{jk} are, in general, asymmetric (that is, $L^i_{jk} \neq L^i_{kj}$). In most of the rest of the book the connexion is symmetric.

Weyl defines an affinely connected space to be one for which there is at each point a co-ordinate system, in which the components of a vector are unaltered to within second and higher orders by an infinitesimal displacement. Eisenhart shows in his second chapter that the necessary and sufficient condition that his affinely connected space shall possess this property is that $L^i_{jk} = L^i_{kj}$.

When the tangents of a curve are parallel with respect to itself the curve is called a *path*, so that paths are analogous to geodesics in a Riemannian space. The paths of a space are unchanged when the connexion is changed in such a way that the symmetric part $\Gamma^i_{jk} = \frac{1}{2}(L^i_{jk} + L^i_{kj})$ of each coefficient L^i_{jk} becomes $\Gamma'^i_{jk} = \Gamma^i_{jk} + \delta^i_k \psi_j + \delta^i_j \psi_k$, ψ_i being the components of an arbitrary vector; the asymmetric part $\frac{1}{2}(L^i_{jk} - L^i_{kj})$ being changed in any way whatever. There will therefore be a *projective geometry of paths* consisting of those properties independent of the connexion and an *affine geometry of paths* consisting of those properties depending on the connexion.

For a symmetric connexion, projective coefficients of connexion are discovered which are invariant under any change of symmetric connexion leaving the paths unaltered. Projective normal co-ordinates for the neighbourhood of each point are also found and, in terms of these co-ordinates, the paths through a point have equations of the same form as the equations of straight lines through the origin in Euclidean space referred to Cartesian co-ordinates, viz. $x^i = \xi^i t$.

When the co-ordinates of a space with given affine connexion are transformed in *any* way, the projective normal co-ordinates for the neighbourhood of any particular point are transformed *linearly* in such a way that a certain hyperplane of the neighbourhood remains unaltered. Hence the geometry of any neighbourhood in an affinely connected space is analogous with the affine geometry of an ordinary flat space (that is, the geometry of a flat space under transformations leaving a certain hyperplane—the hyperplane "at infinity"—invariant).

The last chapter discusses geometry of sub-spaces. Unlike Riemannian Geometry where the metric of a space gives rise to a unique metric in a sub-space, the connexion of any space does not determine a unique connexion for a sub-space. There is a bibliography of a selection of the papers published on the subject to the beginning of 1928.

We are much indebted to Prof. Eisenhart and the American Mathematical Society for the publication in English and in one volume of a clear and interesting introduction to this new subject.

F. B.

Theory and Application of Infinite Series. By DR. KONRAD KNOPP, Professor of Mathematics at the University of Tübingen. Translated from the second German edition by Miss R. C. YOUNG, L. ès Sc. [Pp. xi + 571.] (London and Glasgow: Blackie & Son, 1928. Price 30s. net.)

I OUGHT to have read this book in German, but as a matter of fact I never did. This seems to indicate that many people who could read the German if necessary will welcome this translation of Knopp's classic work on a subject which, even in one's own language, is not very easy. It is well translated, and the translator sometimes evolves a certain liveliness of expression; *e.g.* (footnote on p. 15) "anything of this sort is sheer nonsense." The phrase is too rarely used in English mathematical works.

The printing and general arrangement are excellent, though the use of

numbers which look like exponents in references to footnotes sometimes gives curious effects. Only two misprints were noticed—in the last line but two of p. 226, where a numerator and denominator are interchanged; and in the second line of p. 385, where something has disappeared.

It seems unfortunate that the notation for asymptotic relations is still not completely standardized. The symbol $\sim$,which is now almost universally used for asymptotic equality (*i.e.* $f(x) \sim \phi(x)$ means f/ϕ tends to unity), is used here for asymptotic proportionality (*i.e.* f/ϕ tends to a positive limit). The symbol $\cong$ is used for asymptotic equality. These changes do not seem to bring any advantages over the notation of Hardy's "Orders of Infinity," and only tend to disturb a notation which is beginning to be generally accepted.

As to the book itself, little remains to be said except in praise of it. The first German edition appeared in 1921, and the work may now be regarded as a mathematical classic. Every mathematician has to know something about series, and every modern analyst has to know a great deal. This of course is a book for the reader who wants to know a great deal. It contains an enormous amount of material. The whole apparatus of convergent series is here. As is inevitable nowadays there is a chapter on the summation of divergent series. The author has also added to the edition a chapter on Euler's summation formula and asymptotic expansions, which as he says he only omitted with great reluctance from the German edition. The learned author, however, has all his material completely in hand, and has written a great teaching book. It is enlivened with numerous historical notes. One of the great unsolved problems of the time of James and John Bernoulli (p. 238) was the summation of the series

$$1 + \tfrac{1}{4} + \tfrac{1}{9} + \tfrac{1}{16} + \ldots$$

The solution was found by Euler in 1736. Only John Bernoulli lived to see it, and to regret that his brother had never known the solution of a problem which he had often attempted in vain. On p. 137 we hear of the curious case of Guido Grandi, who thought he saw in the equations

$$0 = (1 - 1) + (1 - 1) + \ldots = 1 - (1 - 1) - (1 - 1) - \ldots = 1$$

a mathematical proof of the possibility of the creation of the world from nothing. On p. 195 we hear of the painful case of J. M. Boormann, who calculated e to 346 places of decimals. How many mathematicians have read a little story by Aldous Huxley called "Eupompus gave splendour to art by numbers"? It would have interested Mr. Boormann.

The book is a very welcome addition to the literature of analysis in English, which is still not very extensive.

E. C. T.

Exercices d'Analyse. Par Gaston Julia. Rédigés par René Harmegnies et Roger Julia. Tome I. [Pp. 454.] (Paris: Gauthier-Villars, 1928. Price 80 fcs.)

So far as I know there is no book quite like this in English. It is a collection of worked-out examples, evolved in the course of exercise classes at the Paris Faculté des Sciences, held in connexion with the lectures of Prof. Goursat. Many of the problems are those set as exercises in Goursat's *Cours d'Analyse*, and Picard's *Traité d'Analyse* is the source of others. Others again are questions proposed by Prof. Hadamard at the Ecole Polytechnique. The arrangement of the whole follows the order of Goursat's *Cours d'Analyse*, Volume I. The main topics are differential calculus and maxima and minima, the evaluation of integrals, trigonometrical series and applications, and geometrical application of analysis; this last being a vast section occupying more than half the book.

Like other French writers, Prof. Julia knows the advantage of giving

himself plenty of room, and he is not afraid of letting an example teach us all that it can. For example, on p. 50 he proposes the problem of proving that

$$\int_0^{\frac{1}{2}\pi} \frac{dx}{\sqrt{(\sin^2 x + \epsilon \cos^2 x)}} = \log \frac{4}{\sqrt{\epsilon}} + \omega$$

where ω tends to zero with ϵ. He then gives four different solutions of the problem, occupying nine pages of the book. The opportunity of comparing different solutions of the same problem is one that books do not often give us, and is particularly valuable to the student.

Some of the evaluation of multiple integrals are specially elegant—they have a geometrical interest which ordinary integrals lack. There are some applications of Stoke's formula. But who was Ostrogradsky?

In the section on Fourier series we have an excellent account of what we may call the elementary theory of Fourier series, *i.e.* the theory as far as it can be developed without the Lebesgue integral. Naturally the theorems are not completely general, but they do introduce the reader to the main facts without overwhelming him at the beginning with subtleties. For example (pp. 180–186) we have the proofs of the theorem that if $f(x)$ is bounded and integrable, and a_0, a_1, b_1, ... are its Fourier coefficients, then

$$\frac{1}{2}a_0^2 + a_1^2 + b_1^2 + \ . \ . \ . = \frac{1}{\pi}\int_0^{2\pi} \{f(x)\}^2 dx.$$

The three proofs are all quite different, and all very instructive. There is also Hurwitz's beautiful proof by Fourier series of the theorem that, for any closed curve, the square of the length of the curve is not less than 4π times the area enclosed.

The geometrical section contains a wealth of examples of all kinds. It begins with a geometrical construction of a curve which has no tangent at any point. Most of it, however, is devoted to three-dimensional geometry, and we revisit the land of curvature and torsion, developable surfaces and edges of regression.

There must be many English University students who would find this book a most useful companion to the regular textbooks. It would be particularly useful to an isolated "external" student. It is also recommended to the lecturer in charge of an exercise class who, towards the end of the spring term, begins to run short of ideas.

E. C. T.

Invariants of Quadratic of Differential Forms. By Oswald Veblen. [Pp. 102 + viii.] (Cambridge: at the University Press. Price 6*s*. 6*d*. net.)

This book appears as one of the Cambridge Tracts in Mathematics and Mathematical Physics and introduces the reader to the theory of differential invariants. It is written for students of mathematics and physics, and sets forth very clearly the fundamental definitions and methods of the subject. In this sense the book is elementary, and this, together with the clearness of exposition, makes it one of the most readable on this subject.

In recent times the theory has been considerably developed, but the chief event in regard to it is the enormous field of new applications. It would appear that the language through which Nature finds her fullest expression is that of differential invariants. This became evident in the theory of Relativity, although there were earlier less obvious indications which writers on quaternions pointed out. Still more recently in the New Quantum Theory we find that this Calculus is pre-eminently suited for the expression of the new discoveries and for further investigation.

The laws of nature appear in a special form, *viz.* the invariant form, and great advances have been made by an appeal to this mathematical form.

Just as one asks of mathematical formulæ that they must be consistent with regard to dimensions, so now we ask that they must be consistent in this new respect.

It is no exaggeration to say that the mathematical physicist cannot do without a knowledge of this notation; to him it is at least utilitarian. It is more than this, for there is no branch of mathematics which offers such elegance of expression or makes such an artistic appeal. To this field the author paves an easy path.

The opening chapter on determinants is most useful. The details given here are so often taken for granted that the beginner is discouraged at the outset by failure to understand the notation. In the later chapters ideas are developed which lead to differential geometry and important formulæ required in physics are worked out from the beginning.

It is a most useful book for the pure mathematician and physicist.

H. T. FLINT.

PHYSICS

Modern Physics. By H. A. WILSON, M.A., D.Sc., F.R.S., Professor of Physics, Rice Institute, Houston, Texas, U.S.A. The Student's Physics—Vol. VI. [Pp. xiv + 381, with 69 figures and 2 plates.] (London: Blackie & Son, 1928. Price 30*s*. net.)

THE author of this book is well known for his contributions to the study of the properties of the electron, and, in particular, for the introduction of an auxiliary electric field in his measurement of the charge on an electron, which eventually made possible the accurate determination of this quantity. His long association with the subject renders him exceptionally qualified to write a book upon it. The book before us is based upon a course of lectures which he has given in recent years to students studying for an honours degree in physics, and its scope and arrangement are therefore of considerable interest to university lecturers in physics. Modern physics is a very elastic term, and its elasticity is a matter of grave concern to all who take part in its teaching, for they are forced to view with anxiety the ever-increasing burden which is thrust upon candidates for honours in physics. Now this book covers an immense field and is up-to-date, and the bare fact that the author has compressed his subject-matter into some 350 pages is sufficient evidence that he has not wasted words. There is, however, one omission from its pages which strikes us rather forcibly. This is the omission of detailed references to original papers, the student being advised to consult other books and treatises for further information. It is doubtful whether this omission is wise, for, in the reviewer's opinion, original papers in scientific periodicals are usually accessible in our science libraries, whereas the few compact English treatises on special branches of physics are usually missing or borrowed for indefinite periods.

The arrangement of the subject-matter is rather unusual. It opens with a chapter on the electron theory, and in some twenty-eight pages the author gives an account of the fundamental equations, the relation of the mass of an electron to its charge, metallic conduction and other properties of matter in bulk, and the modifications introduced by the quantum theory. The second chapter gives a survey of the theories of magnetism. The third chapter is a concise account of thermionic phenomena, but no applications of these phenomena are described. The phenomena of photoelectricity are explained in the fourth chapter, and it is somewhat remarkable that the explanation is unaccompanied by a single diagram. The fifth chapter, on the quantum theory, is very comprehensive, for, in a little over fifty pages,

the author deals with Planck's theory of entropy and free energy, the quantum theory of specific heats, Einstein's theory of heat radiation, Bohr's theory of the emission of spectra, Sommerfeld's treatment of the fine structure of spectra lines, the Stark and Zeeman effects, the correspondence principle, the physical interpretation of the quantum theory, and a series of introductory articles on wave mechanics, dealing chiefly with Schrödinger's work. A short description of Smyth's work on positive ray analysis is contained in the following chapter, on the critical potentials of atoms. X-rays and γ-rays are considered in Chapter VII, an adequate description of X-ray spectroscopy being given, together with sections on the quantum theory of X-ray spectra, on de Broglie's work on energy levels, and on the Compton effect. The space devoted to γ-rays is exceedingly small, and the statement, "The spectra of γ-rays from radioactive bodies have been examined by means of the Bragg crystal spectrometer or similar apparatus" should be amplified. Optical spectra are considered in Chapter VIII, which is followed by a chapter on cathode rays, β-rays, and α-rays. Here the experiments of Kaufmann and Bucherer are described, but we would have appreciated the inclusion of a description of the precision determination of e/m carried out by Busch and Wolf. In connexion with the scattering of α-rays, surprisingly little attention is paid to Chadwick's work. A short chapter is devoted to positive rays and mass spectroscopy, but no mention is made of Wien's later contributions. The subjects of radioactive transformations and the nuclear theory of the atom are briefly discussed in two succeeding chapters. Gaseous ions are discussed in Chapter XIII, but a good deal of recent work is not introduced. Townsend's experiments on the motion of electrons in gases and on ionisation by collision are described in Chapter XIV. The electrical conductivity of flames and of flames containing metallic vapours is then discussed, but Saha's work does not appear to be mentioned. The treatment of the well-known phenomena of the discharge tube is postponed until Chapter XVI, which is followed by a chapter of five pages on atmospheric electricity. Perhaps, when we consider the general neglect of this subject in English textbooks, we should be grateful for this inclusion, but the only reference given at the end of this chapter is Simpson's paper on the mechanism of a thunderstorm, and it is clear that references to the French treatise and to the two small German books on the subject should have been given. Two chapters are devoted to special and general relativity, and the book closes with a final chapter of mathematical notes, a collection of useful examples, and a short table of numerical constants.

The general impression which we form of this book is that it forms a very valuable guide for the student of advanced physics. It lays particular stress on the mathematical side of modern physics. It is, in fact, a framework with whose aid a keen and competent student may build up a satisfactory knowledge of that which is termed modern physics, and with whose aid he may come to see the paths along which we expect further advances to be made.

L. F. B.

The Theory of Light. By T. Preston, D.Sc., F.R.S. Fifth Edition. Revised by A. W. Porter, D.Sc., F.R.S. [Pp. xxiv + 643, with 248 figures in the text.] (London: Macmillan & Co., 1928. Price 25*s*. net.)

When, in July 1890, Thomas Preston wrote the preface to the first edition of his book, he was able to state that ". . . I have been induced to undertake the present work with the hope of furnishing the student with an accurate and connected account of the most important optical researches from the earliest times up to the most recent date. . . ." In 1928 the distinguished reviser of the fourth edition could have no such hope unless indeed the original character of the work was to be entirely lost, and he has, therefore,

of necessity, restricted his revision to those parts of the subject dealt with by Preston and his previous editors. One important omission has, however, been made good by the addition of a series of small-type examples dealing with the leading points in the theory of the thick lens and by a new section, extending to some nine pages, on the aberrations of lenses.

Apart from the additions mentioned in the last paragraph, the first five chapters remain as they were. In Chapter VI, on The Determination of Refractive Indices, correct methods of illuminating the slit of a spectroscope are described and there is a brief account of P. Phillips's experiments on the refractivity of carbon dioxide near its critical point. In Chapter VII on Interference there is a new treatment of the theory of the biprism and a page dealing with the pulse theory of white light. Chapter VIII now includes the calculation of the intensity of the interference bands formed by thick plates (Fabry and Perot interferometer) and a much-extended account of the theory of the Michelson interferometer. The most important changes occur in the chapter on Diffraction, much of which has been rearranged or rewritten. Diffraction phenomena are discussed under the two heads suggested by Rayleigh in his article on Light in the *Encyclopædia Britannica*, *i.e.* as phenomena of the Fresnel class or of the Fraunhöfer class. The Lummer Gehrcke interferometer is worked out in detail; Talbot's bands are treated in a novel manner as being due to an échelon grating of two openings and the resolving powers of gratings, prisms, telescopes, and microscopes are discussed at length.

Preston's treatment of the phenomena and theory of polarised light has been retained (with the exception of the few pages dealing with MacCullagh's theory of metallic reflection) and the sections on the Zeeman effect and magneto-optics in general have been left without the additions which we might perhaps have hoped to find. In the remaining chapters of the book the only important modifications occur in the sections devoted to the electromagnetic theory of dispersion. It is probable that many readers would willingly have sacrificed the Rainbow for some account of Spectroscopy!

In view of the fact that the whole of the type has been reset for this new edition, which is twenty-five pages longer than the last, it is very gratifying that the publishers have not found it necessary to increase the price. The book remains, as of old, an indispensable adjunct to the study of Light. What is now wanted is a second volume to provide, as Preston puts it, "an easy channel of communication with all the most recent discoveries" and Messrs. Macmillan would render a notable service to English students if they could persuade Professor Porter to undertake the task. D. O. W.

An Outline of Physics. By A. E. Caswell, Ph.D. [Pp. 773 + xiv, with 441 diagrams.] (New York: The Macmillan Company, 1928. Price 18*s*. net.)

A book of a standard suitable for students taking a one-year course in Physics at a University, but hardly advanced enough for those who intend to proceed further. It is particularly suited to the student who wishes to make an acquaintance with the subject in all its branches without putting any strain upon his intellectual ability. Little mathematics is used and the symbolic equations and processes which do occur are mainly quantitative expressions of things which have been fully described in words. Much of the book is therefore of a descriptive nature, and in order to make it still more adapted to the needs of the beginner it is arranged in a manner likely to reduce the necessity of re-reading to a minimum.

The first part of the book comprises an account of the elementary properties of matter in its various forms such as elasticity, their behaviour towards heat, etc., and these are explained roughly in terms of molecular states. Electricity and magnetism is treated in the same manner, and if anything too

much use is made of the " water " analogy. This subject will always present difficulty to the beginner since one must learn to visualise such things as difference of potential and so on. It cannot be said that this treatment represents the best that might be done. Electrostatics is hardly touched upon, so that there is no introduction to the idea of a field of force and its properties, though electromagnetic waves are used freely later on.

The next section of the book commences with dynamics. Simple results such as Boyle's Law are worked out on the kinetic theory. There is a good account of simple periodic motion and wave motion. With the help of these sound and light are dealt with in rather more detail than the subjects of the first section. Accounts of water waves and electromagnetic waves are included.

As a whole the book is excellently written and profusely illustrated with carefully prepared diagrams of the type likely to command the interest of anyone with any notion of the subject. Practical applications also find a prominent place, but a departure is marked by the introduction of a great deal of modern physics, including quanta and relativity. The chapter on the latter is possibly the greatest triumph of the work, for it forms an accurate account of the problems of the æther and special relativity in clear simple language. One or two errors have crept into the book, such as the statement that the period of a pendulum is independent of its amplitude.

A. Morris Cassie.

Matter, Electricity, Energy. By Prof. Walter Gerlach. Translated from the second German edition by Francis J. Fuchs, Ph.D. [Pp. xii + 427.] (London: Chapman & Hall, Ltd., 1928. Printed in U.S.A. Price 30s. net.)

The first German edition of Prof. Gerlach's book appeared in 1923, and there is no doubt that it was well received. Its main purpose was to excite the interest of persons who were not professional physicists in the recent researches upon atomic phenomena. The subject-matter was presented in such a palatable manner that the translator felt that this clear description of recent work should be available to those whose knowledge of German was insufficient to enable them to read the original with pleasure. The contents of the book are presumably quite well-known to most teachers of physics. They show that the author has maintained contact with a very wide range of work, and the book is very valuable in that it draws the attention of its readers to many important pieces of research by foreign workers, which are liable to escape notice in the huge output of work with which they struggle at present. In this edition one or two diagrams need better reproduction.

The translator has done his work well, and there are only a few points which need revision. For example, the constant occurrence of the term " rest rays "—the inverted commas are not ours—tends to become irritating to the English reader. Again, such terms as " chemism " and " vacuized " are fortunately not very familiar to us.

Finally, when we come to consider the price of the book, we have to remember that it is a translation and that it should be available to a wide range of readers, and it appears to the reviewer that the price of the book is much too high.

L. F. B.

An Introduction to Crystal Analysis. By Sir William Bragg, K.B.E., D.Sc., F.R.S., Director of the Royal Institution and President of the British Association, 1928–9. [Pp. vii + 168, with 105 figures and plates.] (London: G. Bell & Sons, Ltd., 1928. Price 12s. net.)

Readers of *X-rays and Crystal Structure*—and the number of such must run into thousands—will eagerly welcome the appearance of another friendly

little book from the pen of Sir William Bragg. It would be difficult to find a word more appropriate than "friendly" to describe his writings and lectures; we feel irresistibly attracted to him, and through him to the subject which he has so closely at heart.

An Introduction to Crystal Analysis is no treatise on the subject. We often find the word "Introduction" attached to most forbidding tomes, but in this case innocent readers need have no fear. And yet this does not mean that no difficult points are touched upon—far from it. But Sir William Bragg has such a power of laying bare the fundamentals as clearly to you as to himself, that the rough runs not a bit more roughly than the smooth. As witness his almost childish deduction of the formula underlying the enormously important "rotation photograph." Students of the comprehensive works on crystal structure which have appeared during the last decade will fully appreciate the beautiful simplicity of such a proof. Crystal structure is undoubtedly a subject of many heart-breaking problems; but you would not suspect it from this book. At least, you would feel extremely hopeful about the whole business.

Which is just the impression one always receives from Sir William Bragg's discourses—an impression that things are not so difficult after all, in fact, extremely hopeful all round; and always very, very interesting.

The book is based on a course of lectures delivered at the University College, Aberystwyth, but its plan and scope are naturally somewhat broader than such a course would permit. It is really a rewriting of *X-rays and Crystal Structure* in the light of added experience. Indeed, one of the most fascinating chapters is the one in which the author shows the simple way in which we should now tackle the problem of sodium chloride, the problem of which the solution by the Braggs has opened up such fruitful fields of investigation. Such tremendous consequences have followed from that research that it is now almost essential in the scientific world to have at least a nodding acquaintance with crystal analysis. And though *An Introduction to Crystal Analysis* contains sufficient material to give a prospective researcher a very sound start in his subject, it is admirably adapted to the needs of workers in other subjects who feel the want of some good, readable book to tell them what it is all about. A better book for both these purposes than the one under review does not exist, and, we venture to state, could not be written but by Sir William Bragg. As in the opera, you have a feeling that "the story he tells you is true."

W. T. ASTBURY.

The Physics of Crystals. By ABRAM F. JOFFÉ, Ph.D., D.Sc., D.Eng., LL.D. Edited by LEONARD B. LOEB. [Pp. xii + 198, with 61 illustrations.] (London: McGraw-Hill Publishing Co. Ltd., 1928. Price 15*s*. net.)

THIS book consists of a series of seventeen lectures delivered before the University of California. They all deal with the subject of Crystal Physics, the first six from the point of view of mechanics and the rest from that of electricity.

Professor Joffé and his pupils have been engaged upon experimental researches in these directions for some five-and-twenty years. They have studied the nature of elasticity, deformation, breaking stress, electrical conductivity (both electrolytic and electronic), and of dielectric losses in crystals, as well as a number of subsidiary phenomena necessary to explain the apparent disagreement between theory and observation in mechanical strength and rupture by intense electric fields. All these problems, and a number of others, are discussed at considerable length and, for the most part, with commendable clarity.

The reader will not have studied this work carefully without remarking on the progress which the author has been able to make almost entirely on

the basis of classical conceptions. There is very little Quantum Theory indeed.

Such a simplification has decided merits (and Crystal Physics has long flourished on the old order), but the time cannot be far distant when invasion by the New Mechanics will set in with a vengeance, and no man can stem that tide. The elegant and careful work described in these lectures may well avail to weather the storm, albeit the interpretation of results may undergo revision. This would seem particularly likely, because a certain mechanistic handling of the behaviour of atoms or ions in crystal lattices can be traced throughout the volume, which suggests the mind of the engineer rather than that of the modern physicist. However difficult and mentally distressing it may be, lattice particles (whatever they are) cannot for much longer continue to present themselves to our minds as neat, deformable spheres; they must not expect to escape transformation—and in a sense sublimation—into those elusive joys of the mathematician, the ψ functions of the Wave Mechanics.

With these reservations, one may turn to a few points in the book itself. It is curious that in dealing with the equations of equilibrium (pages 14 and 15), no mention is made of the fact that the fundamental expression for the energy U in the form given refers to distances between unlike charges only, and that another similar equation (somewhat artificially afflicted for the time being with a constant equal to $-0{\cdot}5$) is required to deal with like charges. Again, Fig. 7 needs slight emendation in the light of equation (11).

Lecture VI deals with the important experiment of the quenching of a rock-salt sphere. Even after reading the beautiful theoretical analysis of this problem by Grünberg, one is left unconvinced that the tangential forces do not produce spurious cracks, and are not thus responsible to some extent for premature breakdown in a way as yet unknown.

Nevertheless, on the merits of all the work contained in this book, Prof. Joffé and his disciples are sure of places amongst the master-craftsmen of Crystal Physics.

F. Ian G. Rawlins.

Wien-Harms Handbuch der Experimentalphysik. Band XIII, 2 Teil. **Glühelektroden und Technische Elektronenröhren.** [Pp. x + 492, with 179 illustrations.] (Leipzig: Akademische Verlagsgesellschaft, m.b.H., 1928. Price 46 M. geb.)

The contributors to this volume are to be congratulated on the production of such an excellent work upon relatively new branches of physics and engineering. Their work is divided into three parts, in the first of which Prof. W. Schottky and Dr. Rothe deal with the mathematical physics of the emission of electrons from hot filaments. Every aspect of the problem is considered, and the authors trace the development of the Richardson-Schottky-Dushman equation from the original Richardson emission formula. It is shown how the former equation may be expressed entirely in terms of known physical constants, and this great advance is fully discussed. The summaries of the experimental constants which have so far been obtained indicate that in this branch experiment lags behind theory, and it is to be hoped that more experimenters will be induced to examine the implications of the new theoretical equation. This portion of the book also includes a discussion of the theoretical and practical work carried out on the influence of gas pressure, the nature of the gas, and the surface of the emitter and so forth.

In the second part, Dr. H. Simon describes the modern methods of manufacture of vacuum-tubes, and an excellent, if general, account of the various processes involved is given for all types of vacuum-tubes in general use. Vacuum-tubes in common use are divided into four classes, viz. those with

pure metal filaments, such as tungsten, those with a core metal and a thin metallic coating, such as thorium on tungsten, those with a core metal and a thicker coating, such as barium oxide on tungsten, and, finally, gas-filled tubes such as thorium filament tubes filled with argon and hydrogen mixture. More attention might have been paid to the present-day uses of these classes. The first class is usually employed for the largest power transmitters and the largest power rectifiers, and the second class for small transmitters and rectifiers and receiving valves of the more powerful types. The third class is chiefly used for receiving valves, particularly where small filament current is desired, and to some extent for more powerful receiving valves, but rarely for transmitting valves, whilst the fourth class is chiefly used for small power rectifiers.

The third portion of the book, by Dr. Rothe, deals with the applications of vacuum-tubes, and the author would have attempted a difficult task had he tried to describe all the numerous and ever-increasing uses of the valve. He has therefore chosen to give a general account of the principles involved when the valve is used as amplifier, generator, power rectifier, or detector.

We have already congratulated the authors on this excellent work, and it is earnestly hoped that it will receive the attention in England which it undoubtedly deserves.

L. F. B.

CHEMISTRY.

A Comprehensive Survey of Starch Chemistry. Compiled and edited by ROBERT P. WALTON. [Part I, pp. 240, Part II, pp. iv + 360, in one volume.] (New York: The Chemical Catalog Company, 1928. Price $10.00.)

THE importance of starch as a food for the human body, the many industrial uses to which it is put, and the fascination attached to the elucidation of its structure, make the production of this book of considerable interest.

The first part consists of a series of articles by eminent contributors from all over the world, dealing with the many different aspects of the subject. Some idea of the contents may be indicated: thermal depolymerisation, methylation, enzymic hydrolysis, bacterial degradation, colloid-chemical properties, X-ray spectography, bread-making, fermentation industries, manufacture of corn starch, potato starch, dextrin, gums, starch and flour adhesives, starch in the textile industry. In this part many references are given to the second part, which consists of a bibliography containing 3,485 references. These are classified under different headings, 46 in all, and the compiler must be congratulated on this work.

The book is well written and well printed, and should prove a most useful work of reference. Both author and subject indexes are included. Vol. II is promised to contain a survey of the patent literature of the subject.

J. N. E. D.

Organic Chemistry for Advanced Students. By J. B. COHEN, Ph.D., D.Sc., F.R.S. (Fifth Edition. Part I, "Reactions," pp. vii + 427. Part II, "Structure," pp. vii + 487. Part III, "Synthesis," pp. vii + 440.] (London: Edward Arnold & Co., 1928. Price 18s. net each volume.)

THE student who has completed an introductory course of organic chemistry may easily find himself bewildered by the vast mass of facts with which he is faced on continuing his studies. Each year the amount of newly published work becomes greater, and the task of discrimination more difficult.

The serious student who finds himself in this position may be well advised to make a careful study of the above volumes. The main problems of

organic chemistry are here discussed in a series of essays, which the student will find most helpful. The work is now so well known and well established that it is unnecessary to say more. The arrangement of this—the fifth—edition, is similar to that of the previous one. Much of the contents has been rewritten, and some parts condensed, in order to give room for a description of more recent work, thus bringing the volumes up to date. There is a net increase of fifty-eight pages in this edition, the price of which has been kept the same.

The scope of the work may be indicated by the titles of the chapters: historical introduction, valency of carbon, nature of organic reactions, dynamics of organic reactions, abnormal reactions, physical properties and structure, colour and structure, isomerism and stereoisomerism, stereochemistry of unsaturated and cyclic compounds, stereochemistry of nitrogen, isomeric change, the benzene theory, the carbohydrates, fermentation and enzyme action, the purine group, the proteins, terpenes and camphors, alkaloids.

J. N. E. D.

Organic Syntheses. Vol. VII. By FRANK C. WHITMORE, Editor-in-Chief. [Pp. vii + 105.] (New York: John Wiley & Sons; London: Chapman & Hall, Ltd., 1927. Price 7*s*. 6*d*. net.)

Organic Syntheses. Vol. VIII. By ROGER ADAMS, Editor-in-Chief. (Pp. vii + 141.] (New York: John Wiley & Sons; London: Chapman & Hall, Ltd., 1928. Price 10*s*. net.)

THE plan adopted in the previous volumes of this series, of giving detailed instructions for each preparation under three heads (1) Procedure, (2) Notes, (3) Other Methods of Preparation, is followed in these two volumes, which are a welcome and useful addition to the series. Before publication, each preparation is checked by independent chemists, thus ensuring, as far as possible, that the stated yields can be obtained by following the instructions given. The details provide for making larger quantities of chemicals than is usual in textbooks of organic preparations, the object being to enable the worker to obtain sufficient amounts to use as starting material for research. Collective Author and Subject Indexes to all the volumes are given, and there are convenient spaces for the addition of notes.

Vol. VII contains details for the preparation of anhydro-2-hydroxymercuri-3-nitrobenzoic acid, *p*-arsonophenoxyacetic acid, benzanilide, β-bromoethylphthalimide, 2-bromo-3-nitrobenzoic acid, chloroacetamide, *p*-chloromercuribenzoic acid, α-cyano-β-phenylacrylic acid, *p*-dimethylaminobenzophenone, 3:5-dinitroanisole, diphenic acid, ethyl bromomalonate, ethyl *n*-butylacetoacetate, furan, furfuralacetone, 2-furylmethyl acetate, guanidine nitrate, hydrogen cyanide (anhydrous), β-hydroxypropionic acid, *p*-iodobenzoic acid, methyl *n*-amyl ketone, α-methyl mannoside, nitroguanidine, 3-nitrophthalic acid, 3-nitrophthalic anhydride, pentene-2, phthalimidomalonic ester, triphenyl stibine, xanthone, xanthydrol, whilst Vol. VIII gives instructions for the preparation of acetamidine hydrochloride, allyl cyanide, anthrone, *l*-arabinose, benzalaniline, benzohydrol, benzophenone, benzoylhydroperoxide, benzylacetophenone, benzylaniline, *p*-bromodiphenyl, *m*-bromonitrobenzene, *tert*-butyl chloride, γ-chlorobutyronitrile, β-chloropropionic acid (two methods), dibenzoylmethane, di-*o*-nitrophenyl disulphide, ethyl *p*-aminobenzoate, ethyl benzoylformate, ethyl cyanoacetate, nitroanthrone, *m*-nitrophenol, phenylethylene, phenylsuccinic acid, sodium *p*-arsono-N-phenylglycinamide, styrene oxide, trimethylacetic acid (two methods), trimethylene chlorohydrin, triphenylamine.

J. N. E. D.

Industrial Catalysis. By STANLEY S. GREEN, M.A., A.I.C. [Pp. 505.] (London: Ernest Benn. Price 50*s*. net.)

THE last decade has witnessed an unparalleled rate of growth in the application of catalytic methods to industry. The problems confronting the industrialist in developing a catalytic laboratory process to the stage of a manufacturing plant are naturally different from those which the research chemist has to contend with. There are a number of excellent books which give a general review or which deal with some special field of catalytic operations in the research laboratory. It is clear that a complementary volume is necessary to describe how the industrial problems, such as pressure plant design, purification, heat exchange, gas liquid contact operations, catalyst preparation on a large scale, and the like are carried out. Such a volume might well be entitled "Industrial Catalysis." It was in the hope of reading such a book that the reviewer opened *Industrial Catalysis*, by Mr. Green. It must be confessed that the title must, from this point of view, be regarded somewhat as a misnomer. The volume contains twelve chapters, the first four containing a summary of the history and physico-chemical considerations as well as the theories of catalytic action. These are in some respects rather poor chapters in that the developments in the last few years of this phase of reaction kinetics find no place and certain statements are liable to give the reader a somewhat erroneous impression as to the true state of affairs; thus on p. 76 we find in a discussion on the question of equilibrium and the effect of a catalyst thereon that Reid utilising silica gel "obtained yields of 76 per cent. of ethyl acetate, notwithstanding that the equilibrium mixture at this temperature contains only 67 per cent." The possible effects of solvent on the equilibrium position do not seem to have been realised. Again, on p. 85, we find that "X-ray examination showed that metallic catalysts generally possessed the definite space lattice structure of the crystalline metal, but groups of surface atoms existed in which the crystallisation process was incomplete." Here again the writer does not seem to be aware of the limitations of the method of examination of crystals by X-rays. The subsequent chapters, viz. V to XII, are distinctly better, in which the various reactions which can be accelerated by catalytic methods are enumerated. The chapters on oxidation and combustion (V) and on the utilisation of coal (XII) draw attention to certain phases of catalytic activity which are especially prominent at the present time. Whilst a great deal of interesting material is incorporated in these chapters, the classification appears to be a little complicated and frequently certain sentences are found which even to a non-critical mind appear somewhat cryptic. Thus on p. 109 we find in the discussion on the Welsbach burner and the effect of ceria on the light emissivity of thoria that "the explanation of these facts is catalytic." A few errors are to be noted on the nomenclature; thus Goold and Adams for Goold Adams, p. 147, and the various investigators named Taylor have not been sufficiently well identified in the text. In spite of these criticisms, the book contains a great deal of very readable material and clearly represents the results of much labour; the printing and binding are good, although the adoption of capitals such as Dimethyl ether and Methyl alcohol for the first letters of the names of chemical substances is somewhat unusual.

ERIC K. RIDEAL.

Atomic Structure as Modified by Oxidation and Reduction. By WILLIAM COLEBROOK REYNOLDS, D.Sc., F.I.C., A.R.C.S. [Pp. vii + 128, with 11 figures and 1 table.] (London: Longmans, Green & Co., 1928. Price 7*s*. 6*d*.)

THE majority of writers on scientific subjects realise the responsibility that rests upon them. They are careful only to place before the public theories

that have been critically examined, and which clearly serve a useful purpose. The present writer cannot be numbered amongst the majority. On reading the first chapter we become alarmed at the prospect of some misguided student's taking this work as a textbook. It is not long, however, before we are reassured by the thought that any student who is so dense that he cannot see the quackery deserves to be misled. We are told, for instance, that, on the analogy of the effect of air currents on the velocity of sound, we must look for ether streams to account for refraction of light. "All the electron shells surrounding the nuclei act like centrifugal fans forcing (magnetic) streams of ether in definite directions; vibrations traversing the orientated ether permeating transparent matter must have the paths of the particles moving in a direction parallel to the wave-front increased. This increase may constitute the chief cause of the diminished velocity of waves through the material" (p. 117). This gem has been quoted as it occurs in the last chapter; and it seems unlikely that anyone other than a reviewer with a stern sense of duty will read so far.

R. K. Schofield.

The Colloidal Salts. By Harry Boyer Weiser. [Pp. xi + 404, with 40 figures and 54 tables.) (London: McGraw-Hill Publishing Co., 1928. Price 25s.)

The many research workers who have used the author's former work, *The Hydrous Oxides*, will welcome the appearance of a companion volume. Like the former, this work is comprehensive. Roughly half the space is devoted to the sulphides, that of antimony naturally taking pride of place on account of the large number of investigations made upon it. The first six chapters are devoted to systematic descriptions. The next four treat respectively of pigments, lithopone, flotation of ores, and the phosphorescent sulphides. The author then considers sulphates, halides, ferrocyanides, and silicates. The technical subjects considered under this head include photography, ultra-filtration, glass, and clay.

The very considerable amount of experimental work which the author has carried out in the field of colloidal chemistry has given him an exact knowledge of the subject which he now places at the disposal of his readers. This, coupled with the convenience of having a large amount of dependable information assembled in one volume, will recommend the book to those who are concerned with inorganic colloids.

When this amount of praise has been given it is only fair to point out the book has limitations. It is one of those which leaves no clear impression upon the reader beyond the feeling that the subject is elusive. This is because the book is a compilation of the work and views of numerous writers rather than a narrative. The work will chiefly be of use as a book of reference to those who already have a working knowledge of colloidal phenomena.

Though for the most part the author remains in the background, his chief activity is discountenancing any idea of compound formation. He is always inclined to favour "adsorption." While this is doubtless an eminently safe and prudent view to take, seeing that "adsorption" merely means that the molecule in question is held to the surface by a force of some kind, it shelves the all-important question of the nature of the adsorbing force.

R. K. Schofield.

Photochemical Process. By George B. Kistiakowsky. [Pp. 270, with 8 figures and 41 tables.] (New York: The Chemical Catalog Co., 1928. Price $5.50.)

The author and the editors of the American Chemical Society Monographs are to be congratulated on the production of a book which is a valuable addition to the series. The author has undoubtedly made good use of the

time available during the tenure of a fellowship at Princeton University in Prof. Taylor's laboratory. Photochemistry is a subject that is growing rapidly in volume and in importance, and a connected account of the recent work in this field is very welcome. It is already evident, and a perusal of this book will make it even clearer, that photochemistry cannot be regarded as a specialised branch that can safely be disregarded by all but those engaged in work upon it. It has already thrown so much light upon the fundamental problems of chemical kinetics that it is rapidly assuming a place as an integral part of theoretical chemistry. For instance, the high quantum yield in numerous photochemical processes has compelled a sceptical world to believe in the existence of reaction chains for which no conclusive evidence could be found from a study of thermal reactions. Again, the possibility in certain cases of obtaining the energy of activation from the absorption spectrum and comparing it with the value obtained from the temperature coefficient of the thermal (dark) reaction has greatly assisted in establishing the essential correctness of Arrhenius's activation theory. For the future, we may look with confidence to a closer study of absorption and emission spectra as a means of studying chemical processes in detail.

The sixth chapter deals respectively with light quanta, the equivalence law, chain reactions, photosensitisation, catalysis and inhibition, and the influence of the frequence of incident radiation. Once the doctrine of quanta has been set out in the first chapter, the equivalence law is seen to be axiomatic so far as the primary excitation is concerned. The problem is then a twofold one. First, it is necessary to find what is the nature and condition of the molecules (or atoms) produced by the primary adsorption of light quanta; and then to ascertain the sequences of changes which result from their formation. A certain amount of conflict in experimental evidence (due mainly to the high sensitivity of many of these reactions to traces of "impurities") and a still greater divergence in the details of the theoretical views of different workers make it difficult for the author to disguise the fact that the subject is one that requires careful study, if the true position is to be grasped. It is greatly to be hoped that this book will attract a wide circle of readers.

R. K. SCHOFIELD.

Radiation in Chemistry. By R. ALLAN MORTON, B.Sc., Ph.D., F.I.C. [Pp. xv + 284, 44 figures.] (London: Baillière, Tindal & Cox, 1928. Price 15s.)

THE author has taken a wide interpretation of his title, and deals with all manner of things, from quanta to glow-worms. The grill is none the worse, however, for being mixed. The number of those who will find subjects of interest to them is obviously large when those who are interested in neither quanta nor glow-worms may yet devour with avidity the section dealing with the way in which X-rays are used to distinguish old masters from fakes. A catalogue of all the topics discussed would look very like a page of a dictionary, but a list of the chapters will give the general plan of the book. These are: the laws of photochemistry, light sources, measurement of light intensity, photochemical reactions, the photographic and allied industries photoelectricity and luminescence, biochemistry and light and X-rays in chemical theory and practice.

There is always a tendency for scientists to specialise to an extent that makes it difficult for them to grasp far-flung analogies and wide generalisations. If this book makes one who is mainly concerned in chasing quanta take a lively interest in the activities of a glow-worm, or, vice versa, a great service may be rendered; for as likely as not they have more in common than is generally realised at the present time.

It is unlikely that any reader taking up this book will fail to find something in it that is new to him and still more that is of interest. The reviewer

finds himself rather at a loss to sort out from readers in general the particular class or classes of readers whose attention he would specially draw to this book. He himself has read it with interest and profit, and he hopes that many others will have the same experience. R. K. SCHOFIELD.

The Viscosity of Liquids. By E. HATSCHEK. [Pp. xii + 237.] (London: G. Bell & Sons, 1928. Price 15*s*. net.)

IN view of the many advances made in the physics of fluid motion in the last two decades, this book is very welcome. The author deals with one aspect of viscosity, that which is probably the more familiar to students of physics, the flow of liquids between closely confining boundaries. The cognate problems connected with the flow of fluid limited by solids at certain points only seem to have been purloined by writers on aeronautics.

The historical development of the concept of viscosity is briefly outlined —many physicists will be surprised to learn that the word is derived from the mistletoe on account of the stickiness of its berries—and then follows a detailed treatment of the two methods which form the source of practically the whole of the results discussed in the book, viz. the capillary tube and the concentric rotating cylinders. The design of these viscometers is fully discussed. Variation of viscosity with temperature and with pressure—including Bridgeman's important investigations—form the subjects of the succeeding chapters, while the relations between viscosity and physico-chemical properties are covered with a fullness which should appeal to the chemist. As the author is an authority on the viscosity of colloids, it is only fitting that a chapter on this interesting subject is included—one would have liked more. In a colloid the viscosity is a function of the velocity gradient, so that one can scarcely speak of a viscosity coefficient, at least one cannot justifiably derive such a coefficient from measurements in viscometers where the velocity gradient varies. The present state of knowledge of colloid viscosity is well summed up.

Not the least valuable part of the book is the large number of references both English and foreign given at the end of each chapter. E. G. R.

Colloid Chemistry. By THE SVEDBERG, Professor of Physical Chemistry at the University of Upsala. [Pp. 302, with 167 figures.] (New York: The Chemical Catalog Company, 1928. Price $5.50 net.)

IN this, the second edition, Prof. Svedberg's book on Colloid Chemistry has been considerably enlarged to include accounts of much new work done in the course of the last four years. As before, the author has laid special stress on quantitative investigations rather than on qualitative experiments as the best method of achieving real advance in the understanding of the subject.

The book is still chiefly concerned with the work carried out by the author and his assistants in his own laboratory, and it is therefore to be expected that, of the additions made in the present edition, predominance should be given to a detailed account of the theory and use of the ultracentrifuge. This instrument, whereby the settling out of particles under centrifugal fields can actually be viewed in progress, proves a powerful weapon in the hands of the author and his collaborators for determining the size distribution of particles in a suspension and has also been used in the measurement of the molecular weights of proteins.

Additions are also made in the sections concerned with the application of X-ray analysis to study of sols and gels; with oriented coagulation, and with the photographic process from the standpoint of the colloid chemist; while the section on osmosis and membrane equilibrium has been rewritten.

W. W. B.

GEOLOGY

The Determination of Minerals under the Microscope, with Special Reference to the Interpretation of Interference Phenomena. By J. W. EVANS, C.B.E., D.Sc., F.R.S. [Pp. xii + 110, 4 plates, 51 figures.] (London: T. Murby & Co., 1928. Price 7s. 6d. net.)

THE student of mineralogy and petrology coming fresh to these subjects is often baffled by the optical phenomena he encounters in the determination of minerals under the microscope. Dr. Evans's little volume, which deals mainly with interference phenomena, has been written especially to help students in the optical study of minerals, and to explain simply, avoiding the use of advanced mathematics, the optical principles which underlie the practice. It goes almost without saying that Dr. Evans has been entirely successful in his aims. Beginning with a chapter on the petrological microscope, and following with a chapter on the nature and properties of light, he passes immediately to the examination of minerals in polarised light, phenomena between crossed Nicols, the object image, the directions image, and dispersions in the directions image. A few other determinations, such as the thickness of the rock or mineral slice, birefringence, and refractive index, are dealt with briefly in a final chapter, and the book ends with a brief summary of procedure.

In a work such as this there is little need or scope for criticism; but while there may be a mathematical reason for the procedure adopted, for clearness' sake should not the birefringence have been rendered as $\tau - \sigma$, instead of $\sigma - \tau$ (p. 14), which is a negative quantity according to the statements made on p. 12? Similarly, considering that the book is concerned with optical phenomena, should not the term "isotropic" be rendered as "optically isotropic"?

Throughout the book clear, concise, and simple descriptions of the procedures involved in the optical determination of rock-forming minerals are given, and these are supplemented by a number of clear diagrams. The excellent polarisation colour scale recently published by Dr. W. R. Jones and Dr. A. Brammall is utilised as a frontispiece. No student of mineralogy or petrology can afford to be without this work. G. W. T.

Elements of Optical Mineralogy. An Introduction to Microscopic Petrography. Part II. Descriptions of Minerals, with Special Reference to their Optic and Microscopic Characters. By N. H. WINCHELL and A. N. WINCHELL. Second Edition. Entirely rewritten and much enlarged by A. N. WINCHELL. [Pp. xvi + 424, with 333 figures.] (New York: J. Wiley & Sons; London: Chapman & Hall, 1927. Price 27s. 6d. net.)

THIS second edition of a well-known reference work has been entirely rewritten and rearranged by Prof. A. N. Winchell. The author appears to have ransacked every available source of information, American and European, for his data, and the material here compiled can be relied upon as thoroughly up-to-date and exhaustive. As Prof. Winchell points out in his preface, the work is not merely a compilation, but includes a great deal of new information which has been won by comparative studies of the existing data. New ideas are stated with reference to the chemical constitution of many of the isomorphous groups of rock silicates, and as to correlations between variations in composition and optical properties within these groups. Many of these new relations have been expressed by means of diagrams, which will prove to be of great assistance in mineralogical and petrographical determinations.

While on first sight the book seems to be a mass of figures, yet the arrangement of the matter is so good, and the index so full, that the required infor-

mation can be obtained with great ease and rapidity. The more general matter, and that dealing with the commoner minerals, are printed in larger type than that referring to the less important and less abundant minerals. The only criticism that the reviewer would like to make is that, for him, the type of a large portion of the book is rather too small and fatiguing to the eye. The figures are numerous and admirably clear. It is indicative of the importance of the felspar group amongst the rock-forming minerals that no fewer than 64 pages out of 407, and 74 figures out of 333, are devoted to this group. This book contains easily the best accounts of the optical properties of the felspars and other isomorphous groups of rock-forming minerals.

The arrangement of the minerals was alphabetical in the first edition of this work; but as this arrangement proved not entirely successful in practice, it has been changed in the present edition to the scientific chemical arrangement which we owe to Dana. A short introduction describes the drawings, diagrams, and projections that are used.

Extensive dipping into the data failed to discover any errors or misprints. We are of opinion that the book is a first-rate work of reference which should find a place in the library of every working mineralogist and petrographer.

G. W. T.

Monographs of the Geological Department of the Hunterian Museum, Glasgow University. II. The Vertebrate Fossils from the Glacial and Associated Post-glacial Beds of Scotland in the Hunterian Museum, University of Glasgow, and their Evidence on the Classification of the Scottish Glacial Deposits. By J. W. Gregory, D.Sc., F.R.S., President of the Geological Society, and Ethel D. Currie, B.Sc., Ph.D., F.G.S. [Pp. 25, 3 plates, 6 figures.] (Glasgow: Jackson, Wyllie & Co., 1928.)

The difficulty in correlating the Scottish glacial and post-glacial deposits with those of England and the Continent is due to the scarcity of fossils and their sporadic occurrence, and also to the absence of evidence of the occupation of the country by Lower Palæolithic Man. The correlation suggested by the authors in the present volume is based largely on the mammalian remains in the Hunterian Museum and on the information available regarding the horizons in the Scottish glacial and post-glacial drifts from which they were derived.

The most important localities are Kilmaurs (near Kilmarnock), Bishopbriggs and Torrance (near Glasgow), Croftamie (Drymen, near the southern end of Loch Lomond), Cowdon Glen (near Barrhead), and Lanark.

The oldest Scottish glacial horizon affording definite palæontological evidence is that of Kilmaurs (reindeer and mammoth; marine shells with species now extinct in British waters). The associated boulder-clay is the second of the three Scottish boulder-clays, and the authors correlate the horizon with the Third Terrace of the Thames Valley. The Bishopbriggs and Torrance horizon (mammoth and woolly rhinoceros) is correlated with the Late Middle Terrace of Crayford and Erith, the associated boulder-clay being interpreted as the oldest of the Scottish boulder-clays.

The remaining Scottish horizons are of post-Mousterian age, the oldest occurring at Croftamie. Here the fauna is reindeer without mammoth, and marine shells of species now living in British waters; the overlying boulder-clay is interpreted as the Upper Boulder-clay. These deposits are correlated with the Maximum Glaciation (*vide* Hinton). The post-glacial horizons are correlated with the Lower Terrace of the Thames Valley sequence. They are represented by deposits at Cowdon Glen and drift filling the preglacial valleys of the Clyde, near Lanark. At Cowdon Glen the underlying boulder-clay is the Upper Boulder-clay of Croftamie, and the overlying boulder-clay is interpreted (after Craig and Strain) as a slipped mass of the

underlying clay. The fauna at Cowdon Glen is *Bos primigenius*, red deer, and horse of the Steppe type; at Lanark, horse of the Plateau type.

Other localities, which cannot be enumerated here, are correlated with those mentioned above.

The catalogue of the specimens is well illustrated by plates and by figures in the text. The authors have had the assistance of numerous specialists in the determination of the material. There is a bibliography of the glacial and post-glacial palæontology of Scotland.

JOHN WEIR.

BOTANY AND AGRICULTURE

Pioneers of Plant Study. By ELLISON HAWKS, F.R.A.S., in collaboration with the late Prof. BOULGER. [Pp. x + 288, with 15 plates.] (London: The Sheldon Press, 1928. Price 12*s*. 6*d*. net.)

THE authors of this work have covered a long period of time from the plants of ancient Egypt where the field poppy was already a weed of cultivation more than 2,000 years B.C. to the middle of the nineteenth century when the dominance of morphology and taxonomy began to give place to the physiological and applied aspects of modern times.

Despite the claim for the Chinese Emperor Chin-nung that he founded Chinese medical botany nearly 5,000 years ago, it is not till the time of Theophrastes (370–387 B.C.) that botanical knowledge can be said to have assumed a scientific aspect, a period that is considered in Chapter VIII. There follows a consideration of the contributions of Pliny, Dioscorides, Albertus Magnus, and other pioneers who flourished before the invention of printing gave to the world the herbals of Schoffe (1433), Brunfels (1530), Fuchs (1543), Matthioli (1544), Turner (1551), Gerard (1597), and Parkinson (1640). Just as this output of herbalistic literature in the sixteenth century gave an impetus to taxonomy that reached its zenith in the nineteenth century, so, too, the introduction of the compound microscope into England in 1619 initiated the study of plant anatomy and enabled Hooke (1665), Grew (1671), and Malpighi (1675) to discover the cellular structure of organisms and the vascular conducting tracts of plants. The study of vegetable physiology may be said to have begun with the writing of *Vegetable Staticks* by Hales in 1727 and from then on the dominating position of taxonomy was threatened.

The sequence in development of botanical thought is interestingly presented, and though the length of period covered necessarily involves a selective and often cursory treatment, the main trends are adequately indicated even if the relative importance of the contributions of the past are not sufficiently emphasised.

E. J. S.

The Romance of the Apothecaries' Garden at Chelsea. By F. DAWTREY DREWITT, M.A., M.D. Third Edition. [Pp. xvii + 175, with 15 plates.] (Cambridge: At the University Press, 1928. Price 7*s*. 6*d*. net.)

THE first edition of this work appeared in 1922, a second two years later in which was included the letter from Gronovius that showed it was the Apothecaries' Garden which was the chief object of Linnæus's visit to England. The present edition contains additional matter of which perhaps the most interesting is the correspondence between Sir James Smith and William Jones respecting the proposed Linnean Society.

The book contains much that is of interest regarding the history of the gardens which to-day, under the curatorship of Mr. William Hales, play a definite part in London Education, and thus, despite the change of ownership, still furthers the advancement of botany, for which purpose the garden

was originally leased by the Apothecaries' Company in the reign of James I.

The gardens have been associated with many famous men of science, and horticulture owes no small debt to the work there carried on. Here it was that Phillip Miller wrote the well-known *Gardener's Dictionary* published two years after his appointment as gardener, and later William Curtis, when Demonstrator at the gardens, compiled his *Flora Londiniensis* and initiated the *Botanical Magazine*, which survives to the present day.

E. J. S.

A Textbook of Systematic Botany. By Prof. Deane B. Swingle. [Pp. xiii + 254, with frontispiece and 62 figures.] (London: The McGraw-Hill Publishing Co., 1928. Price 10*s*. net.)

It is, as the author points out, a curious fact that in the branch of botany which has been longest studied there are practically no textbooks. The first part of this work aims at setting forth the general principles of taxonomic study and is divided into ten chapters treating respectively of Purposes of Systematic Botany; Evolution in Relation to Taxonomy; Principles of Taxonomy; Difficulties in Classification; Development of Systems of Taxonomy; Phylogeny of Spermatophytes; Nomenclature; Preparation of Herbaria; The Terminology of Systematic Botany; The Literature of Systematic Botany.

The second half of the book deals with a selection of families of angiosperms. This section incompletely covers ground that has been more adequately treated in other works, and one cannot but regret that the space so occupied was not devoted to an expansion of the earlier part which concerns subjects with which most students are too little familiar; but the work is a welcome addition to botanical literature, and it may be hoped that further editions will be called for in which a more complete treatment can be presented.

E. J. S.

An Economic and Financial Analysis of Fifteen East Anglian Farms, 1926-1927. By R. McG. Carslaw, M.A., and W. H. Kirkpatrick, C.D.A. [Pp. 17.] (Cambridge: W. Heffer & Sons, 1928. Price 1*s*. net.)

The above is one of the series of reports on the conditions of agriculture in the Eastern Counties prepared by the Farm Economics Branch of the University of Cambridge. In farming, as in other forms of business, the final test is the financial showing of the enterprise, and judged from this standpoint these fifteen typical arable farms in 1926–7 present a rather dismal picture. Of the total output, *i.e.* receipts less payments for raw materials and depreciation, wages took 71 per cent., rent and rates 26 per cent., and profit, the farmers' reward, only 3 per cent. The interest on the farmers' capital was only ·9 per cent.; while, if a charge was made for his own supervision, this minute profit was changed to a substantial loss. A heavy loss on cattle caused by low prices during the "meat war" was almost balanced by a series of smaller gains on crops of which sugar beet and barley were the most profitable. Milk production on arable land was not very remunerative. Of the side-lines to farming, poultry showed consistent profits amounting to 100 per cent. of the invested capital. The season covered was the worst encountered since these yearly analyses had been undertaken, and the authors conclude on the note that farmers must look to adaptability and resource within the industry rather than to the Government for a way out of their difficulties. In any case, they say "it is unlikely that it will ever be possible to feel assured that farming provides a gilt-edged security."

H. V. G.

ZOOLOGY

Experimental Embryology. By Thomas Hunt Morgan. [Pp. xi + 766, with a coloured frontispiece and 263 text-figures]. (Columbia University Press, 1927. In England Mr. Milford. Price 37s. 6d. net.)

Prof. T. H. Morgan is so well known as to need no introduction, and the high level of his previous work is fully maintained in the present volume. In the past his writings have not formed light or easy reading, but have been noted for the information and ideas they have contained and the same is true of the book here considered. While of late Prof. Morgan's name has been more associated with work in genetics that has made *Drosophila* a household word to all biologists, yet it is obvious that he has kept closely in touch with the developments in experimental embryology that have taken place since his *The Development of the Frog's Egg* in 1897, which by the way was the first of its kind in the English language.

The aim and contents of the 25 chapters of the book are best indicated by the author's own words: "In the present volume many of the problems are concerned with the first changes and movements in the cytoplasm prior to its cleavage and with the movements of groups of cells following cleavage—in a word, with the so-called formative changes in the egg and with their initiation. These various topics are dealt with from the more or less morphological point of view, although from a variety of angles. Prof. Morgan adds, however: "It is my intention to consider the more obvious physiological changes in another volume, where such topics will be discussed as growth, reflex actions and tropistic movements of larvæ, sex-determination, embryonic grafting, the influence of the environment on the development of the embryo, the source of the energy of development, etc. The incompleteness of some of the topics in the present volume will then be supplemented, and a more rounded treatment of the present condition of experimental embryology will be attempted.

The book is furnished with a very good index and a bibliography. This latter is quite extensive and consists of 101 pages of references arranged under chapter headings, so that it gives ready access to a very extensive literature of which the text itself is largely a detailed and critical study.

It is refreshing, in view of the somewhat arrogant claims often made by experimental workers, to read the warnings given by one who is not out of sympathy with the work but who may with justice be considered a master. Of the complex terminology which is often employed as if it explained phenomena he says that frequently "In reality, some obscure event has only been given a name that conveys no more meaning than the event itself. Of the procedure adopted he remarks: ". . . many of the 'experiments' do little more than describe what happens to the developing egg or embryo under a variety of new conditions imposed by the investigator, in the hope they will do something that may throw light on some of their properties not revealed under the conditions in which they 'normally' develop."

Within the limits of a short review it is not possible to do justice to such an excellent and all-embracing work. Perhaps it would not be unfair to say that the author views the changes constituting development as due to the interaction of three sets of forces: (1) external environment either of the organism as a whole or of a cell or a cell group, (2) internal environment or changes within the cytoplasm itself, and (3) the nuclear domination of the cytoplasm. Of these the last is the most important. In answer to those who claim an all-important rôle for the cytoplasmic activities in determining the main outlines, leaving the chromosomes to decide what may be called the trivial characters, he would probably be closely in agreement with Wilson, who says, ". . . the cytoplasmic organisation of the egg is itself the product of an antecedent process of epigenetic development in the course

of which, as we have every reason to believe, the chromosomes have played their part—thus the chromosomes are as much concerned in the determination of the so-called preformed or cytoplasmic characters as in any others."

This book is outstanding and invaluable to any student of modern biology, and many will have cause to thank the author for his assistance in guiding them through a diffuse and extensive literature as well as for his illuminating suggestions. It is well printed and free from obvious errors and slips.

C. H. O'D.

Bibliography of Sponges, 1551–1913. By the late C. G. J. VOSMAER. Edited by G. P. BIDDER and C. S. VOSMAER-ROELL. [Pp. xii + 234.] (Cambridge: At the University Press, 1928. Price 15*s*. net.)

ALL workers on sponges will be grateful for the very efficient Bibliography now published. Gualtherus Carel Jacob Vosmaer was professor in zoology in the University of Leiden from 1903 to the time of his death in 1916. At the beginning of his career he studied sponges with F. E. Schulze, and was faithful to them all his life, giving to the world perhaps the most lasting and important work ever published on these simple organisms. Zoologists owe much to the author of the dissertation on *Leucandra aspera*, the Sponges in Bronn's *Klassen und Ordnung*, the monograph on *Spirastrella*, and many other works. They look forward to his forthcoming monograph of Naples Sponges in the *Fauna and Flora* series, on which he was working to the end of his life, and which had taken thirty-four years to finish.

The present Bibliography was begun in 1880, and, edited by Dr. Bidder, with the help of Madame Vosmaer-Röell, and at his suggestion, is now published independently of the Monograph. The editing itself is exceedingly laborious, necessitating reference to many almost inaccessible works dating from as early as 1551; but it has been very carefully carried out, and the spongologist now has before him a bibliography of literature on his own subject which has been revised by experts, and which is of the greatest possible use to all those engaged in the study of these most interesting creatures.

M. V. LEBOUR.

The Basis of Sensation. The Action of the Sense Organs. By E. D. ADRIAN, M.D., F.R.C.P., F.R.S. [Pp. 122, with 31 text-figures.] (London: Christophers, 1928. Price 7*s*. 6*d*. net.)

THE first and last words in the present volume may well be repeated, for they constitute its challenge and its justification. The first are: "The basis of sensation may well seem an ambitious title to a book. . . . I do not feel that its title needs an apology." The last are: "It does not bridge the gap between stimulus and sensation, but at least it shows that the gap is a little narrower than it was before." Anyone reading it will admit that the last claim is fully justified, for it does undoubtedly mark a very distinct advance in our understanding of the function of the sensory nerves.

Much work has been done on the activity of the motor nerves as revealed by a study of their end results, contraction in a muscle, etc., and Sherrington and his school have gone further and brought into the picture the contribution of the central nervous system to the correlation of such results. Contemporaneously there has been a considerable amount of investigation of the electrical disturbances that accompany the transmission of impulses along the motor nerves. These disturbances have been termed action currents.

The first method of attack, *i.e.* by the study of the end results, is not possible at present, because no method of measuring sensation has yet been devised. The second method also had to await refinement in technique. This has now been provided by the utilisation of the amplification obtained from three electrode valves, a special capillary galvanometer, and a photo-

graphic film record. In the present work the amplification employed is 1,700–1,800. Another advance in technique is along morphological lines, and it was found possible to utilise a strip of the sterno-cutaneous muscle of the frog served by a single sensory nerve fibre. This was of great use in analysing the results obtained in more complex nerves.

The results of the investigations may be summarised, but without full justice being done them, by saying that the functional activity of the sensory is similar to that of the motor nerves: a noteworthy fact. Also it is suggested that the sensation is proportional to the impulse discharge in the sensory nerve, which is determined by the excitatory process in the receptor mechanism.

The book is well and clearly written. Its argument is developed in a logical manner that is easy to follow, partly because, unlike many books of like nature, it is not loaded up with innumerable references to all the papers remotely connected with the subject, a procedure that only too often serves to confuse a reader not actually engaged in a similar line of work.

C. H. O'D.

Evolution and the Spirit of Man. By J. Parton Milum, B.Sc., Ph.D. [Pp. 228.] (London : The Epworth Press, 1928. Price 7*s*. 6*d*. net.)

This book strikes the reviewer as decidedly good of its kind and for its size. Its aim is not to discuss scientific problems, but to investigate the significance of present tendencies in scientific thought for metaphysical problems. The author states: " The object of our study is the significance of the evolutionary world-view for man himself." His endeavour is " to re-read the facts of scientific research in the spheres of biology, geology, anthropology, and psychology." That is to say, his book is solely concerned with interpretation, whereas the primary attitude of the man of science is that of the investigator. The difference between these two attitudes is not always clearly appreciated. The chief criterion of the value of a theory for the investigator is heuristic success. But heuristic success is no guarantee of truth. Accordingly this criterion is not sufficient for the interpreter whose primary concern is with the truth of theories.

In the Introduction the author attempts to rebut the scepticism regarding the unity and order of the world which has sprung from the tendency towards subjectivism which had its origin in Descartes. The treatment is too short to shake the sceptic's " faith " in his own scepticism, but on this point the average scientific reader will probably share the convictions of the author, however much he may differ from the latter's interpretation of the nature of that world-order. But there are one or two passages here which deserve the most careful consideration, even from the scientific standpoint. For example, on p. 21 is written: " Science speaks to us of unalterable, unchanging laws, and then goes on to speak of an unceasing change called evolution. How can uniform laws and forces produce continuously new developments ? " And on p. 29: " The dilemma of modern science is this: it is bound to take the ' disinterested ' view of nature—it must disregard anything of the nature of purposes or values; only so can it impartially deal with things. On the other hand, in venturing to teach evolution, it has thereby avowed as master principle a conception in defiance of its own canons. . . . If science had stayed content by the doctrine that ' all things change,' it would have been consistent. But the concept of evolution is inseparable from progressive accumulation and synthesis and the notion of ' higher ' and ' lower '; in short, in passing from the idea of mere ' change ' to evolution the rubicon is crossed from the world of fact to the world of values."

The author begins, as everyone (scientific or otherwise) begins who attempts to construct a comprehensive world-view, with certain basal assumptions. Such assumptions or beliefs fall into two kinds: they are either

optimistic or pessimistic. For some reason or other it is commonly believed that only fundamental assumptions of the pessimistic variety ("tough-minded" as William James called them) can be called scientific, possibly because they are the most successful for heuristic purposes. But from the point of view of truth there does not seem any good reason for believing that one type is antecedently more probable than another. The author of this book states his basal belief to be "that personality has value for the universe." He then tries to show that there is nothing to contradict this belief in the present state of natural scientific knowledge, but on the contrary he thinks he can find something to strengthen it. Chapters I, II and III, dealing with biological topics, appear to the reviewer to be better than the psychological Chapter IV. In the remaining three chapters there is much that is of interest and deserving of impartial consideration. And whatever view the reader may take of this author's opinions, he can hardly fail to acknowledge his sincerity, fairness, and breadth of view.

J. H. W.

The Frog: An Introduction to Anatomy, Histology, and Embryology. Twelfth Edition. By the late E. MILNES MARSHALL, M.D., D.Sc., M.A., F.R.S., edited by H. G. NEWTH, M.Sc. [Pp. x + 182, with 40 illustrations.] (London: Macmillan & Co., Ltd., 1928. Price 6*s*.)

THIS new edition has been little altered since the eleventh edition appeared in 1912 under the editorship of the late Prof. F. W. Gamble, F.R.S. It has, however, been brought up to date and the sections dealing with microscopical technique, the germ-cells, and the early stages of development have been modified. The size of the book and general style remain unaltered. There are no changes in the illustrations, except that two have been removed and have not been replaced. The small amount of revision necessary after sixteen years is, in itself, ample proof of the excellence of the book.

F. W. R. B.

The Biology of Spiders. By THEODORE H. SAVORY, M.A. [Pp. xxx + 376, with 16 plates and 121 text-figures.] (London: Sidgwick and Jackson, 1928. Price 16*s*. net.)

THIS is another volume in the interesting and useful series of biological handbooks published under the general editorship of Prof. J. A. Thomson, and it fully maintains the high level set by its predecessors. The first four chapters treat of the characters of the Arachnids and the structure of spiders. This is done in a very straightforward and easily comprehensible way. At the same time it is not in the least childish and the author introduces and uses technical terms in a very satisfactory manner. As the book is not a morphological treatise these chapters are not exhaustive, but they provide sufficient anatomical information to furnish a sound basis for the discussion of the various organs of the animal as a whole, and of the characters used in classification. Chapter xii deals with the development, chapter xiii with the fossil history, and chapter xv with the general evolution and classification of spiders. The sixteenth chapter gives a brief survey of other groups of the Arachnida. The remainder and larger part of the book is devoted to the many different biological activities of these fascinating creatures.

The illustrations are good; the plates are well chosen and reproduced, and while many of the text-figures are simple they are quite adequate to elucidate the points for which they are used.

The question of nomenclature is at best a vexed one, and as well left alone unless a muddle is straightened out or some solution of a difficulty suggested. In a book of this sort a slip in nomenclature could well be overlooked, but on pp. 8 and 9 the author takes up the question of the generic name *Araneus*.

He there brings forward certain irrelevant data and treats the whole issue as if it were a matter of personal preference, much as was done in the *Cambridge Natural History*, which he appears to have followed, paying no attention to the international code. He states that Linné uses *Araneus* for all spiders, whereas in the 10th edition of *Systema Naturæ*, which in this case is the only one that matters, we find on p. 619 the genus termed *Aranea* and the first and therefore, unless for some reason it be unavailable, the type species is given as *A. diadema*. The name of the genus is therefore well established, and by derivation the family is the Araneida. In the index the correct trivial name *diadema* is used, but throughout the text it is given as *diademata*.

On p. 180 the author deals with longevity, and, after pointing out that most spiders live only one season, states that five years is probably " the limit for all those spiders that survive more than a single season." However, Baerg has shown very recently that the male *Eurypelma californica* does not become mature until the tenth or eleventh year.

The book is furnished with a useful bibliography arranged under subjects, but it is a pity that it does not give references to McCook's volumes on the spinning work of American spiders nor to the recent papers on spermatogenesis by Warren.

For some reason spiders are somewhat neglected both by professional and amateur naturalists, but, as the present book clearly shows, they are an extremely interesting group. We hope that the author will have the gratification of seeing his work help to rectify this neglect, and a better introduction to their study would be hard to find. Zoologist and naturalist alike will have cause to thank the author for providing a very sound, useful, and well-written account of the biology of spiders.

C. H. O'D.

Life in Inland Waters. By Kathleen E. Carpenter. Textbooks of Animal Biology. Edited by Julian S. Huxley. [Pp. xviii + 267, with 12 plates and 97 figures.] (London: Sidgwick & Jackson, 1928. Price 12*s.* net.)

This series of textbooks provides introductions to different branches of biological science, which are intended to be of direct use to the undergraduate student, but in reality, besides fulfilling this purpose most effectively, cover a much wider range. They bring together the important matter of each subject dealt with in such a way that the specialist as well as the amateur may read or refer to them with equal benefit. The present volume will be much used by all interested in freshwater biology. Dr. Carpenter has succeeded admirably in her task and has produced a most attractive account of the life in inland waters and the modern methods employed in its investigation. It is a book written by a naturalist who knows her subject by constant personal work in collecting and experimenting and whose love for it permeates every chapter.

The present-day naturalist wishes to investigate the physical and chemical characters of the fresh waters and the adaptations of the organisms contained therein to their environment, making a biological whole of the study of life in pond, lake, or stream. Here he is shown how to do this, and he is also shown most of the animals and plants which he may expect to find, with clear illustrations of these.

The book is well planned. Chapter I deals with a general survey of freshwater life and its probable origin, with a discussion on the physiology of adaptation to the change of medium. Later the activities of the fauna are described, nutrition and respiration; and interrelations between the organisms. Later some chapters on the relations of the fauna and flora to the chemical and physical properties of the medium, reproduction and life-histories, dispersal and migration. These subjects occupy about half the book, the other half containing descriptions of the life in various kinds of

fresh waters—running streams, rivers, lakes, estuaries, ponds, marshes, subterranean waters and springs, and finally organically polluted waters. At the end of each chapter is a good and up-to-date bibliography. This book can be recommended confidently to all freshwater biologists, who will find in it much that is new and a great deal that is not usually found in the literature relating to freshwater life.

M. V. Lebour.

Barnacles in Nature and in Myth. By Edward Heron-Allen, F.R.S., [Pp. xv + 180, with 53 figures.] (London: Oxford University Press, 1928. Price 15*s*. net.)

This barnacle epic is a scholarly and most entertaining piece of work. Originally a discourse delivered before the "Sette of Odd Volumes," it expanded to such an extent on being written, by reason of the enormous amount of research into ancient literature which was necessary for references, that it resulted in a valuable treatise on this small crustacean around which superstition has revolved since the earliest times.

It was said, and for ages was believed, that the Barnacle Goose was not hatched, as is an ordinary bird, from an egg, but that it was developed inside the shells of little sea animals found in drifting wood or from the leaves of certain trees, which, falling into the sea, swim away as birds. The Barnacle was the goose from which the crustacean was named on account of this legend. So much faith was placed in that story that special dispensation was given to eat the Barnacle Goose in Lent and on fast days as, because of its origin, it was neither flesh nor fowl.

The author begins by giving a clear account of our two common barnacles, the Rock Barnacle and the Goose Barnacle, and their life histories as they are known at the present day. It is the latter animal, also known as the Ship's Barnacle, which is the subject of the "Myth." The flexible stalk by which it attaches itself to wood was supposed to be the neck of the developing bird; the cirri which sweep in food were regarded as feathers.

Most of the book is given up to a complete history of this "Myth," beginning with Neckam and Giraldus Cambrensis about the tenth or eleventh century, and only ending about the seventeenth or beginning of the eighteenth, when clear thinkers and observers were actually proving the falseness of the legends. The tracing of the "Myth" through these labyrinths is exceedingly interesting and much involved and the figures collected from the old authors form a wonderful gallery of illustration.

The last chapter gives a full account of the Barnacle in relation to ancient art. Here it is shown that the "Barnacle Myth" served as a subject for decoration at least as early as the Octopus theme.

Zoology, archæology, and classical literature all have a share in this very attractive book.

M. V. Lebour.

The Elasmobranch Fishes. By J. Frank Daniel. Second Edition, 1928. [Pp. xi + 332, with 270 figures.] (Berkeley, California: University of California Press; Cambridge: at the University Press, 1928. Price 27*s*. 6*d*. net.)

The first edition of *The Elasmobranch Fishes* was published in 1922, and now only six years later comes another edition: the very best proof that such a book is wanted and used. This has the exact appearance and size of the former volume, but there are certain alterations and additions which are undoubtedly improvements. It is a work of very real value and of the greatest possible use to both student and teacher, whether beginner or expert, embracing as it does a thorough introduction to the anatomy of cartilaginous fishes in general and a detailed account of one. This one is *Heptanchus*

maculatus. To this detailed account has been added still more, especially with regard to the blood system, and several more illustrations have been included. The most important of these additions is an account of A. B. Key's recent work on the hypobranchial arteries and their derivatives in *Hexanthus corinus*, with three plates. This adds to and supplements the researches of Professor Daniel himself, and, as he states, "makes it more certain that the blood supply to the pectoral area in primitive vertebrates was from the hypobranchial system rather than from the dorsal aorta (the sub-clavians), as is true for higher vertebrates."

Amongst other alterations which are all to the good are the abandonment of the term "nephridia" for the kidney tubules, and the enlargement of some of the figures, which, clear as they were previously, are now even clearer. The book is handsomely got up and beautifully illustrated throughout, the diagrams and drawings, especially those of the blood systems, being models of what anatomical drawings should be.

We only regret that there is not more about the life-histories of these most interesting fishes, the few notes about eggs and young being hurried into two or three pages at the end of the volume. M. V. Lebour.

The Natural History of Wicken Fen. Edited by Prof. J. Stanley Gardiner. F.R.S. Part IV. [Pp. 117, with 3 text-figs. and 3 plates.] (Cambridge: Bowes & Bowes, 1928. Price 6*s.* net.)

This part contains an interesting account by Prof. Gardiner of Wicken Fen as it is to-day and as it has been in the past. The importance of the 600 acres of fen land held by the "National Trust for Places of Historic Interest or of Natural Beauty" is dealt with in some detail. More than half of this area has not only never been cultivated, but has never been deliberately drained. It is, therefore, "a real bit of the old Fenland" with its rich and characteristic fauna and flora, which are rapidly dying out in other places with the improvements in drainage and reclaiming of the fens. We learn that the fauna and flora of this area are not strictly natural, but the result of a primitive type of cultivation applied for many hundreds of years, the cutting of the sedge for house-thatching every fourth year. Now the demand for cut sedge has vanished, but the cutting must be continued if the fauna and flora are to be maintained. If left alone, most of Wicken Fen has been proved to become bush or else reed-swamp, with a consequent reduction of the number of species of plants to about one-third and of animals from 6,000 species to less than 2,000. The task of maintaining the 600 acres of fen in their original condition and of preserving them is therefore not light.

The present part contains eleven other papers on special groups of animals and plants inhabiting the fen. These contain much new material of general interest, those on the Coleoptera by J. Omer Cooper, M. G. L. Perkins, and C. E. Tottenham, and on the Copepoda by A. G. Lowndes being of special interest. F. W. R. B.

Biology of the Vertebrates: A Comparative Study of Man and his Animal Allies. By Herbert Eugene Walter. [Pp. xxv + 788, with 687 figs.] (New York: The Macmillan Company, 1928. Price 21*s.* net.)

This textbook contains a large amount of useful matter concerning the comparative anatomy, physiology, embryology, etc., of the Vertebrates. It is distinctly useful as such, and contains many excellent figures. We think that too much space has been accorded to the general principles of animal biology and to the elementary principles of zoology. General zoology must be taught in connexion with invertebrate as well as vertebrate animals, and consequently it should be confined to books dealing with both. Textbooks, such as the present one, dealing with a single phylum, should be based on

the assumption that the readers are acquainted with elementary general zoology. This criticism is directed chiefly against the first part of the book, which furnishes an introduction to the other two. The second part deals with the Mechanism of Metabolism and Reproduction, and the third part with the Mechanism of Motion and Sensation. These sections are good and compensate for the deficiencies of the first. The figures are numerous, and, on the whole, good, especially those dealing with skeletal structures. Two or three, such as Fig. 120, p. 166, and Fig. 485, p. 542, are grotesque and partake of the characteristics of problem pictures.

F. W. R. B.

Fundamentals of Biology. By ARTHUR W. HAUPT, Ph.D. [Pp. xii + 358, with 256 figures.] (London: McGraw-Hill Publishing Co., 1928. Price 15s.)

THIS book aims at presenting the principles of General Biology in an elementary form, suitable for those who have had no previous biological training. Those aspects of life which are common to both plants and animals are specially emphasised, and detailed descriptions of the structure of types are reduced as much as possible. The treatment of the material is clear and the book is well balanced. The printing and binding are distinctly attractive, and all the 256 figures are excellent. The book is one of the best of its kind, and is suitable for use in schools, or for a set course for entrance prizes and scholarships to Universities. It is one that can be recommended to the layman who wishes to become acquainted with the outlines of biological theory without having to labour through detailed descriptions of a large number of representative forms of animals and plants.

F. W. R. B.

MEDICINE

Une Grande Page de l'Histoire de la Médecine: La Découverte de la Transmission du Paludisme par less Moustiques. Par Sir RONALD ROSS, K.C.B., K.C.M.G., F.R.S. Préface et Traduction de l'anglais par le Dr. Charles Broquet. (Pp. 174, with 8 plates and 7 figures.) Paris: Norbert Maloine, 1929. Price 20 fcs.)

THIS book is a French translation of Sir Ronald Ross's Nobel Lecture originally delivered in Stockholm in 1902, printed by the Nobel Committee in 1905, and reproduced in the *Journal Royal Army Medical Corps*, April, May, and June 195. It has already been translated into German by C. Schilling (G. Fischer, Jena 1905) and into Italian by F. Maiocco (Lib. Editrice Universitaria, Torino, 1905). The French translation now given is very ably done; and the Preface especially is perhaps the best account of Sir Ronald Ross's work ever published, and is particularly free from the numerous misstatements which have appeared about that work in England, Italy, and elsewhere. The Preface concludes with the following words:

"La splendide découverte de Sir Ronald Ross restera dans la mémoire des hommes comme le plus bel exemple de ce que peut créer l'intelligence aidée de la volonté, de la persévérance et du labeur, ces trois constituants eternels, du génie. Mais l'œuvre de Sir Ronald est loin d'être achevée. En 1923, pour lui permettre de la poursuivre, la reconnaissance publique, répondant a l'appel de noms éminents, a élevé par souscription à Putney Heath à Londres et a remis aux mains du grand pionnier un Institut de recherches et un Hôpital de maladies tropicales. Dans ces maisons de la science les travailleurs viendront dévoiler les problèmes qui restent encore à résoudre pour que les terres coloniales, débarrassées enfin du paludisme et des grandes maladies qui les dévastent, ne soient plus pour le blanc un objet de crainte et pour l'indigène une source de misère et d'infortune. La présence

de son Altesse Royale le Prince de Galles à l'inauguration de l'Institut était le plus bel hommage que le Gouvernment britannique pouvait rendre à cette fondation humanitaire et à son chef. Le Ross Institute sera dans la capitale de l'Empire le témoignage de la gratitude publique à l'égard du maître anglais pendant qu'il est encore vivant, et restera après sa mort un souvenir durable de l'un des plus illustres bienfaiteurs de l'humanité."

History of the Physiological Society during its First Fifty Years, 1876-1926. By Sir EDWARD SHARPEY-SCHAFER, F.R.S. [Pp. 198.] (London: Cambridge University Press, 1927. Price 15s. net.)

THIS history of the Society by one of its original members, who happily is still one of its most active and valuable members, was compiled to celebrate the Jubilee of its foundation. The Society was founded at a meeting held on March 31st, 1876, at Dr. Lauder Brunton's house. He himself was in the Chair and the other members present, formed a group of men either already famous or destined to become so, amongst whom were Darwin, Huxley, Galton, Geo. H. Lewis, Michael Foster, Shafer, and many others. Indeed in those early days it would appear that to have been elected to the Physiological Society was a sure and certain indication of future fame. The brilliant work of the biologists of those days attracted to that branch of scientific work many of the younger generation, just as now the great advances made in the physical sciences are making them powerful rivals in that respect to the biological studies.

The actual inaugural meeting was celebrated by a dinner at the Criterion Restaurant, Regent (now Piccadilly) Circus. This custom of dining at the meetings has prevailed to the present day, since the meetings at which the formal business of the Society is done are still the dinner meetings, though now between these a number of laboratory meetings are arranged. Following the early precedent, the meetings have remained very informal; there is no regular chairman, each meeting being presided over by the head of the laboratory in which the meeting takes place. This has undoubtedly helped to preserve in the meetings of the Society the tradition of its early days of a group of friends coming together for the discussion of matters which concern them as physiologists. Prof. Schafer has brought the history right down to the present day and made of it a story intensely interesting to physiologists and other biologists, and with a great appeal to all who are proud of the leading position which this country has been able to take in the advances of physiology, in the making of which the Society has played a great and living part.

W. C. CULLIS.

The Pressure Pulses in the Cardiovascular System. Monographs on Physiology. By CARL J. WIGGERS, M.D. [Pp. xi + 200, with diagrams and illustrations.] (London, New York, Toronto: Longmans, Green and Co., 1928. Price 14s. net.)

IN a preface, the new editors, Profs. Lovatt Evans and A. V. Hill, pay a tribute to the wisdom of Prof. Starling, who promoted and for eleven years edited these Monographs on Physiology. Recognising that the advances in a given science are not likely to take place equally at all points, but now at one point and now at another, he aimed at presenting in the Monographs in a compact and stimulating form the progress of physiology in those chapters in which the forward movement had been most pronounced. Each monograph is written by a leading investigator in the selected field, and for the author of the present volume certainly no better choice could have been made than that of Prof. Wiggers. For many years he and his colleagues in the School of Medicine of Western Reserve, Cleveland, Ohio, have been working in the field of cardiodynamics, and Prof. Wiggers is himself responsible for

much of the best of the apparatus now used in the investigation of these difficult problems. The interpretation of tracings obtained by such instruments has always been a difficulty, as witness the classic controversy as to the sequence of the events in the ventricular contraction as deduced from different curves. One of the greatest assets of this present work is the reliance that the reader comes to place on the interpretations of the author, having regard to his wide experience, and to the exceedingly careful and scrupulously fair-minded manner in which his own work and that of other investigators in the field are criticised.

The early chapters, which will be of great assistance to those specialising in this subject, are devoted to a discussion of technique both from the practical and theoretical aspects. Then come chapters which will be of greater interest to the non-specialist, and which deal with the pulse pressures in the cavities of the heart, and give accounts of the form and significance of the arterial pulse and of the venous pulse. In the later chapters there are fruitful discussions on the response of the heart to the physiological variables increased venous return and aortic resistance, on the intimate character of the ventricular contraction, and on the pressure pulses in certain abnormal types of ventricular contraction. Finally, there are some very suggestive pages on the Refractory Phase in cardiac muscle, which are of great interest when considered in the light of the recent work upon skeletal muscle. The book includes an excellent Bibliography. Altogether a very good example of what can be achieved in such a monograph. It would be well to include somewhere in the book a list of the other volumes in the series.

W. C. Cullis.

Catalogue of Manuscripts in the Library of the Royal College of Surgeons of England. By Victor G. Plarr, M.A., Librarian to the College. (Pp. 76.) 1928.

This catalogue gives information, hitherto not available in print, of the great treasures in manuscript possessed by the Royal College of Surgeons. It includes all the manuscripts except those dealing with John Hunter, of which a list was published in 1891. It is, however, in part a supplement to this last-mentioned volume, since it contains various records of Hunter acquired since 1891. Such are the highly important "Hunter-Baillie Collection," of which descriptions appeared in 1926 in the *British Medical Journal* and elsewhere. Such also are the writings of William Clift, secretary to Hunter, who copied many of his papers which would otherwise have been lost, and who later became Conservator of the College Museum.

The catalogue also contains many interesting manuscripts of Sir Astley Cooper, W. H. Flower, Sir Everard Home, William Hunter, Edward Jenner, Lister, Sir Richard Owen (who was son-in-law to Clift), Sir James Paget, Quekett, and Sir John Simon, the hygienist.

Other interesting items are a fifteenth-century manuscript of Archile de Verona, Barnard Davis's original manuscript of his collection of crania, Greenhill's Lexicon of Greek medical terms which has never been published and whose contents were not included in the new edition of Liddell and Scott, and five volumes of letters from Pasteur to Lister.

The cataloguing of manuscripts is far from a straightforward undertaking. The author says "manuscripts have been in most cases catalogued under the names of their true originators," but it is seldom clear if the manuscript is original or not (*e.g.* is the undated Collegium Physiologiæ of Albinus in the handwriting of Albinus, or merely a copy ?). There is no apparent order in the entries under each author: they are neither alphabetical nor by dates. It is very difficult to trace anonymous items: *e.g.* "Account (The) of the dissection of the Body of the Honble. Mr. Howard," would, if wanted, probably be sought under "Howard," but is under "Account," and with

no cross-reference from Howard. Another criticism is that the notes are in the same type as the entries themselves, and the two are difficult to distinguish.

The work is, however, a memorial of Mr. Plarr's industry in collecting unique material, often from outlying parts of the building, and recording its existence, thereby rendering it available to the research student of medical and biological history.

WILFRID BONSER.

A Short History of Medicine. By CHARLES SINGER, M.A., M.D., etc. [Pp. xxiv+368, with 142 illustrations.] (Oxford : at the Clarendon Press, 1928. Price 7s. 6d. net.)

THE average medical student never reads about the history of his subject, chiefly because he is not interested in it, but partly because until recent years there were no short and readable histories of medicine in the English language. This is a most unsatisfactory state of affairs; because the student learns nothing about the origin of the ideas he uses, he is apt to suppose that the present state of knowledge represents the very last word on everything, and may pass through his entire career without departing from this naïve attitude. But these defects are easily removed by a little historical knowledge. Perhaps the greatest service the history of science can do is in pointing out the dangers of intellectual complacency and of supposing that the present state of knowledge is in any way exhaustive.

Dr. Singer's book is a welcome addition to the small number of short histories of medicine at present available. But it is not only addressed to the medical student. Dr. Singer is of opinion that all educated men and women should have some knowledge of medical science and that history is a good way of conveying such knowledge. The book covers the whole field, from the Greeks to quite recent times, in a very comprehensive way considering its small compass. Every branch of the subject, not excluding nursing, receives attention, and modern developments of bacteriology, including epidemiology, are treated in considerable detail. Elementary histories of science are apt to degenerate into anecdotal biography and sentimental hero-worship, but Dr. Singer successfully avoids this mistake, and saves his space for the proper emphasis of all the more important things.

Dr. Singer seems to express surprise that Aristotle should have attached so little importance to the brain. But it seems plain that this was simply because Aristotle persisted in the (to us) extraordinary notion that the "noblest" parts must be placed in the centre of the body. There are also one or two statements which appear to be rather misleading in view of the type of reader to whom the book is addressed. On p. 32 it is stated that "very recent embryological research goes some way to confirm" Aristotle's view of "the 'accidental' character of the material contribution of the male" in sexual reproduction. If by this it is intended to suggest that the experiments of Loeb and others on artificial parthenogenesis, and the interpretations put upon them, represent a return to the Aristotelian view of reproduction, then this suggestion as it stands is misleading since the resemblance is rather superficial. Also the statement that Aristotle's view of the soul as "that which gives form," that this "activity of the soul" "has an important place in modern biological theory," and that the latter has "swung definitely in the direction of the Aristotelian position"—all this seems rather too optimistic from Dr. Singer's point of view. It is true that there is in progress a revolt against the simple-minded cocksureness of the last century, but it by no means follows from this that the only alternative is a return to Aristotle. In fact, it might be said that the present century, in successfully overcoming the limitations of the Aristotelian logic, has cast

off the last shred of that great thinker's influence which survived the Renaissance. We have to look forward, not back.

On p. 202 it is stated that the working hypothesis of sanitarians of the first half of the nineteenth century was "that filth and ill-drainage were direct factors in the production of epidemic disease." It is added that "The view is now untenable." What is meant is, presumably, that further analysis has led to the substitution of the precise notion of the pathogenic organism for the vague term "filth." But as a useful empirical generalisation it still remains true that filth and ill-drainage are direct factors in the production of epidemic disease, since where such conditions prevail epidemic disease also prevails. Consequently the statement that such a view is no longer tenable may possibly convey an erroneous impression to the lay reader.

Dr. Singer does well to mention the importance of contemporary "philosophical atmosphere" in relation to the development of science, but he makes the mistake common to histories of science in over-emphasising observation and experiment at the expense of thought. On p. 103 it is said that "The acceptance of Observation and Experiment as the only method of eliciting the Laws of Nature reaches an ever-widening circle." But if the reader is led to suppose that Galileo founded the science of mechanics by observation and experiment—by simply dropping cannon-balls from the campanile at Pisa—he will carry away a very erroneous impression. It was primarily by thinking that Galileo reached the concept of acceleration upon which the revolution in physics depended. He thought out many different alternative mathematical laws and *then* proceeded to test them. So much was this the case that even his Aristotelian critics accused him of devoting more years to philosophy than months to physics. Now in the biological sciences observation and experiment have played a much larger part, and thought, of the revolutionary Galilean kind, a correspondingly smaller one, and this may be responsible for the difficulties of biology to which Dr. Singer draws attention.

Dr. Singer has provided his book with a good index, but unfortunately no bibliography. Apart from the above points Dr. Singer's book is a valuable one which deserves to be read by all medical students and should appeal to a wide circle of non-medical readers. The illustrations are copious and very good, and the whole book has been beautifully produced by the Clarendon Press.

J. H. W.

MISCELLANEOUS

Race and Civilisation. By Friedrich Hertz, D.Ec. Translated by A. S. Levetus and W. Entz. [Pp. xii + 328.] (London: Kegan Paul, Trench, Trübner & Co., 1928. Price 18*s*.)

In this interesting work Dr. Hertz makes a powerful attack on those theories of racial origins which lead to the assertion of a natural right of one race to dominate others or to restrict by force their possibilities of existence. Such a theory was that worked out by H. S. Chamberlain, which sought to prove the racial superiority of the "Nordics" and which was therefore used by the Pan-German party as an important political weapon, and Dr. Hertz is no doubt justified in asserting that reasoning of this kind has become a great danger to the peace and civilisation of the world.

In so far as it is possible to demolish such theories by argument from history without the aid of statistical and biometric data—for such are conspicuous by their absence from the book—the author makes out a strong case for his point of view, which may be briefly stated by two quotations from the Preface: "What history and ethnology seem to teach is that

the fundamental traits are the same in all races, and that the adaptability of individuals of one race to social and cultural conditions created by other races is not limited by inherited qualities. But probably there is at least a diversity of temperament between certain races, and even small differences may sometimes have great consequences." . . . "Generally environment only offers certain opportunities, and it depends on the historical level and the cultural individuality of a people how it is used, though, of course, opportunity may be a very strong factor." Dr. Hertz assembles a good deal of evidence against the idea that north European peoples are predominantly "Nordic" or "Aryan" in origin, and shows that the present population of Europe is the result of some thousand years of migrations and crossings of races. The roots of Roman civilisation, for example, which preserved Europe from becoming Hunnic or Mongolic, reach deeply down into the pre-Aryan cultures of the Mediterranean peoples and of the ancient Orient, nor were the cultural foundations of ancient Greece nor the Jewish contributions to moral culture of Aryan origin.

Dr. Hertz also seeks throughout the book to demolish the belief that genetic intermixture of diverse races is generally productive of disastrous consequences or that it had anything to do with the fall of ancient civilisations. Thus he sums up his conclusions on these points as follows (p. 151): "The allegation that classic culture perished through racial deterioration contrasts clashingly with historic truths. . . . The vicissitudes of the classical world, on the contrary, are with absolute certainty to be traced to social processes, economic disarrangements more particularly, and to their effects on the military constitution." And again (p. 153): "All nations of any importance in history show a mixture of different races. As early as the dim past of history racial mixtures on an extensive scale must have taken place, as is proved by the fact that skulls of the most diverse racial types are found in the very same prehistoric gravefields." . . . He supports Fischberg's conclusion that there exists no such thing even as a homogeneous Jewish type and states, for example, that "the German Jews are much more like the other Germans than they are to their co-religionists in Palestine. The similarity appears in the head form, the proportions of the body, pigmentation, and facial traits." In support of this he states that the purely brunet type has been preserved by only about half the European Jews, whilst the "typical" Jewish nasal features only occur in about 14 per cent.

In the chapter "On Racial Capacity for Progress," amongst other evidence of the same kind, Prof. Macdonald's investigations on the intelligence of American and negro boys in Washington are quoted as showing that under conditions favourable to full development of their mental capacities the negro boys seem to have been actually superior in their response to certain intelligence tests, 47 per cent. of some 7,500 boys being classed as "bright" as compared with 37·5 per cent. of some 22,000 American boys.

In the closing chapter on "Psychology and Ethics of the Race Theories," Dr. Hertz attacks the methods of reasoning used by certain advocates of inherent racial inequalities in an outspoken and fearless manner. Thus (p. 311): "I do not object to having descended from the ape, but in view of the moral and intellectual level of many race theorists, I should not exactly like to have descended from the same ape as they."

Whilst feeling that, in his enthusiasm for levelling humanity, the author tends to go rather too far and whilst there is much in the work with which we cannot agree, there is a great deal that is salutary and the book provides refreshing and profitable reading to anyone who is not inclined to require a statistical proof of all the statements it contains. The references at the close of each chapter, though chiefly from German sources, are a useful aid to the reader and are amplified by notes. β_4.

(*a*) **The Symbolic Process and its Integration in Children.** By JOHN F. MARKEY, Ph.D., Research Professor of Sociology, Experiment Station, Connecticut Agricultural College. [Pp. xii + 192.] (London: Kegan Paul, Trench, Trübner & Co., Ltd., 1928. Price 10*s*. 6*d*. net.)

(*b*) **Judgment and Reasoning in the Child.** By JEAN PIAGET, D.Sc., Director of the Institut Rousseau and Professor at the University of Geneva. Translated by MARJORIE WARDEN. [Pp. viii + 257.] (*Ibid*., 1928. Price 10*s*. 6*d*. net.)

BOTH volumes are in the *International Library of Psychology, Philosophy, and Scientific Method*.

(*a*) The reader who is more interested in facts than theories should begin this book at Chapter IV. Like most of the following chapters, it summarises a number of experiments and observations concerned with the beginnings of children's speech. "First words" are usually uttered before the first birthday, and the number used increases rapidly, so that the vocabulary for a normal child aged six may be 2,500 words as compared with 20,000 for a college student. The considerable differences found by various observers are supposed to be due mainly to the differing environmental conditions. The "content of symbols" is next considered. The child is said to be far more interested in the use of things than in their appearance. An analysis of a large number of responses to certain nouns suggests that about 80 per cent. can be assigned to the "obvious use and action" category before six years of age, while the frequency declines rapidly to about 30 per cent. at age fifteen. The so-called "refined action content" is meanwhile becoming more evident. The correlation of these factors with age is found to be almost perfect for the data given. Since proper names have usually been neglected in compiling children's vocabularies, an inquiry is made into the use of personal pronouns. Again the statistical data are almost too perfect. Eight graphs for individual children are given showing the number of new words acquired at different ages. Almost all the more salient peaks in the irregular curves are found to correspond with the times when the personal pronouns were first used. The order of appearance of these pronouns is normally first person, second person, third person, according to the records of several observers, while, in general, the singular form appears before the plural. The "subject reference" is used before the "possessive reference." The question of the use that the child makes of the pronouns in his social life is then considered. The average number of words spoken in a day by a normal child appears to be about 12,000. An artificial analysis into parts of speech provides questionable material which is treated by questionable statistical methods, but there is a clear suggestion that the "self" pronouns are less used after three years of age. The decrease in their use is said to be negatively correlated with the number of words spoken per minute. Experiments involving a delayed reaction suggest that "the social-vocal behaviour situation apparently furnishes the requisite mechanisms for true symbolic integration when associated with a sufficiently complex-behaviour system." The only empirical data considered are those which are presented in Chapters IV–VIII. They are taken to indicate the important part played by social behaviour in "symbolic development." The first two and the last four chapters deal with more abstruse matters which are enlightened, in the author's opinion, by the simple experiments he has described.

(*b*) This book is also concerned with an analysis of children's speech: it is a sequel to *Language and Thought of the Child*. The method of approach is somewhat different, however, since the author is more interested in the genesis of logical thought than in social behaviour, and his treatment is more dialectical than statistical. The implications of a number of original observations and experiments are discussed at considerable length. The children

were first given verbal tests such as incomplete sentences involving the use of the word "because." By considering a large enough population, an attempt was made to determine the age at which the average child can reason well enough to use such conjunctions in the way adults do. Modified forms of some of the Binet-Simon tests were also applied to test the nascent ability to draw logical inferences. Another method used was the direct one of asking the child to explain why he had answered certain questions in a particular way. Simple arithmetical problems were found to be most satisfactory for this purpose. Numerous examples are given to show how "thought becomes more and more conscious of itself." An analysis of definitions given by children shows that in early years they ignore equally the adult need for considering either physical causality or logical necessity. The idea of utility is the first which appeals to them in this connexion, as in answering the question, "What is a mother?" by "For cooking the dinner." From material of this kind the author arrives at certain conclusions regarding the nature of juvenile thought. These deductions are presented in the last chapter, and it is not possible to summarise them in a few lines.

The essential conclusions of these two books are in accord. The thought of the young child is found to be almost entirely self-centred ("ego-centric") and it develops by becoming more and more capable of perceiving objective relations and the ideas and feelings of other people: the change is from the purely individual to the social point of view. Both researches are alike, too, in being essentially concerned with the evidence of empirical data. The extent to which the methods used will prove fruitful would seem to depend on improvement in the technique of experimentation and observation, rather than on further analysis of the material already available, or on the accumulation of exactly similar material. Psychology will gain when methods such as these can be made to depend less on personal bias of the observer and when they can lead to a more exact quantitative expression.

β_2.

Silvicultural Systems. By R. S. Troup, D.Sc. (Oxon), F.R.S. [Pp. xii + 199, with 86 figures.] (Oxford: Clarendon Press. Price 21s. net.)

Another contribution has been added to forestry literature in Prof. Troup's *Silvicultural Systems*. The book details at considerable length the systems which have been practised on the Continent for decades past, and it should be noted that each system as enumerated in this book is peculiar to certain districts, and they are not of general practicability. Few, if any of them, can be put in practice in English woodlands, as they one and all assume that the woodlands must be more or less in normal condition before the systems can be tried. The book is therefore of value more to students of forestry who are studying for the future in different parts of the world where conditions are similar to those where the methods have been evolved. Apart from the fact that the area of woodlands in which any of the systems enumerated, other than the uniform system, could be applied, the book is no less interesting, as serving to show the systems which might be applied when we have woodlands of an extent and of a nature to make it possible. It will at the same time serve as a very useful guide for any travellers in continental forests, as it will enable them to interpret systems which would be otherwise difficult to understand. It should always be borne in mind, however, that the information is gleaned from departments which have had woods for long under their control, and where the woods themselves have been subjected to an intensive form of management, perhaps under forest systems for a century past and more.

As usual with the work of the Clarendon Press, the book is extremely well produced, and the photographs are highly instructive.

W. Dawson.

Probability and its Engineering Uses. by THORNTON C. FRY, PH.D. [Pp. 476 + xiv.] (London: Macmillan & Co., 1928. Price 30s.)

DR. FRY's admirable book is, to quote from his preface, "the outgrowth of a set of notes originally prepared for one of the 'Out-of-Hour Courses' of the Bell Telephone Laboratories and subsequently revised for use in a course of lectures delivered at the Massachusetts Institute of Technology in its Department of Electrical Engineering." It is therefore not surprising, though it is regrettable, that the applications he makes of the theory are mostly to problems of telephony, especially those of the automatic exchange.

The theories of probability and of statistics have of late years become of increasing importance and have invaded not only the sciences which at one time were thought too fluid for mathematical treatment but also those, like Physics, which might have been thought too exact to need statement in terms of the merely probable. The engineer, dealing as he always must with variable and uncertain material and with his structures and machines subject to uncertain calls on their strength and capacity, ought long ago to have felt the need for statistical methods and should gain considerably from their use. A growing realisation of this is reflected in recent engineering literature. Statistical analysis of the results of tests are becoming more common, while such analyses of flood data are proving of value to the hydraulic engineer. But the value of statistics other than the mean is not yet generally realised. For example, the results of experiments on the ultimate strengths of materials are always stated in a form which gives us no way of estimating the probability of fracture at any given stress less than the tabulated mean, and, in consequence, factors of safety are still no more than expressions of (frequently justifiable) prejudice.

Dr. Fry, though he gives us some fascinating glimpses of applications on the economic side of engineering, such as the control by sampling of the qualities of manufactured products, fails to show the full extent of the possible applications of his theory. In consequence, the reader who is not interested in telephone exchanges may feel that the book is overloaded on the purely mathematical side. In this he would be mistaken, for there is very little theory here that cannot be put to use in more than one branch of engineering.

The author's presentation of his material is very clear and always interesting. He is refreshingly concerned with the logic of his subject and is always ready with a problem or a paradox in illustration of a difficult idea. Starting from fundamentals, he gives a definition of "probability," ranging himself with the "insufficient reasonists" and arguing their case effectively. He then develops the algebraic theory and proves the theorems of Bernoulli and Bayes. After a discussion of continuous variables, he devotes three chapters to the theory of statistics, dealing with averages and distribution functions in a thorough way. The discussion of the foundations of Poisson's Law is particularly good. The problem of fitting a frequency distribution to given data is dealt with at some length, very full consideration being given to the χ^2 test of goodness of fit.

Up to this point, the end of the ninth chapter, applications have been merely illustrative. The last two chapters are concerned wholly with applications. Chapter 10 deals with a fascinating group of exchange problems, while the final chapter gives some account of the Kinetic Theory of Gases and of the Schottky Effect.

More than 40 pages of Tables add greatly to the value of the book; factorials and their logarithms, binomial co-efficients, the error function, and the principal distributions are all included.

The whole is well produced and printed and free from important errors. We hope that its considerable merits will lead to an early demand for a new edition and that Dr. Fry, by extending the scope of his applications, will then make it the important and influential work that it might easily become.

R. C. J. HOWLAND.

Mosquito Surveys: A Handbook for Anti-malarial and Anti-mosquito Field Workers. By MALCOLM E. MACGREGOR. [Pp. 282, with 59 figures and 3 maps.] (London: Baillière, Tindall & Cox, for the Wellcome Bureau of Scientific Research, 1927. Price 15s. net.)

SIR RONALD ROSS, in a short Foreword, shows how especially qualified the author is to deal with the subject of this book. He further calls attention to the fact that, while we stand aghast at the mortality caused by the recent war, little or no heed is paid when a similar mortality is caused by a disease each year and every year, although its prevention is possible.

The author endeavours to provide information for the officers and members of the Medical and Health Department of Mauritius and Rodriguez with regard to the species of mosquitoes at present known to inhabit these islands. He further attempts to furnish both a brief explanation of the anatomy and bionomics of mosquitoes to non-entomological persons associated with anti-mosquito work, and also an introduction to the study of mosquitoes in general.

The book is divided into three parts. In a few pages at the beginning of the first part, Mr. MacGregor gives brief descriptions of the islands of Mauritius and Rodriguez and discusses possible origins of the mosquitoes in the two islands. He comes to the conclusion that it is safer to account for the mosquitoes by the immigration theory rather than assuming these islands were part of one large continent. He then goes on to give a good and well-illustrated account of the morphology of mosquitoes, dealing successively with the adult, egg, larva, and pupa.

In the second part he deals with the mosquitoes of Mauritius and Rodriguez in detail, both the classification and bionomics receiving attention. For the former the keys of Mr. F. W. Edwards are extensively quoted.

In the final part laboratory and field technique are explained in simple and clear language. This part will be specially useful to persons engaged in anti-malaria work who have had no biological training. In addition there is a useful list of publications bearing on the subject and an adequate index.

We consider that Mr. MacGregor has succeeded in producing a handbook which will be of great value to all anti-malaria and anti-mosquito workers. It is well arranged and illustrated, and deals with the subject so clearly that anyone, trained entomologist or otherwise, should be able to grasp thoroughly the entire subject; this should result in anti-malaria campaigns being carried out with more understanding and scientifically. It is a great pity the price of the book is so high.

H. F. B.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Sechstellige Tafel der Trigonometrischen Funktionen. Enthaltend die Werte der sechs Trigonometrischen Funktionen von Zehn zu Zehn Bogensekunden des in 90° Geteilen Quadranten und die Werte der Kolangente und Kosekante für jede Bogensekunde von O° o′ bis 1° 20′. Von Prof. Dr. J. Peters. Berlin und Bonn: Ferd. Dümmlers Verlag, 1929. (Pp. viii + 293.) Price 48 M. geb. 52 M.

A History of Mathematical Notations. By Florian Cajori, Ph.D., Professor of the History of Mathematics, University of California. Volume I, Notations in Elementary Mathematics. London: The Open Court Company, 86 Strand, W.C.2. (Pp. xvi + 451, with 106 figures.) Price 25s. net.

Elementary Algebra for Schools. By R. C. Fawdry, M.A., B.Sc., and H. C. Beaven, M.A. Part I. London: A. & C. Black, 4 Soho Square, W.1, 1929. (Pp. vii + 221.) Price 3*s.*, without answers, 3*s.* 6*d.* with answers.

Astronomy for Surveyors. By M. K. Rice-Oxley, A.C.G.I., A.M.I.C.E., and W. V. Shearer, B.Sc., A.M.I.C.E. London: Methuen & Co., 36 Essex Street, W.C. (Pp. ix + 208, with Star Charts.) Price 8*s.* 6*d.* net.

Conduction of Electricity through Gases. By Sir J. J. Thomson, O.M., F.R.S., Professor of Experimental Physics, Cambridge, and G. P. Thompson, M.A., Professor of Natural Philosophy in the University of Aberdeen. Third Edition, Volume I. Cambridge: at the University Press, 1928. (Pp. 491.) Price 25*s.* net.

The General Properties of Matter. By F. H. Newman, D.Sc., A.R.C.S., F.Inst.P., and V. H. L. Searle, M.Sc. London: Ernest Benn, Bouverie House, E.C.4, 1928. (Pp. 388.) Price 25*s.* net.

Intermediate Heat. By R. A. Houstoun, M.A., D.Sc., Lecturer in Natural Philosophy in the University of Glasgow and Examiner in Physics in the Universities of Edinburgh and St. Andrews. London: Longmans, Green & Co., 1928. (Pp. ix + 112.) Price 3*s.* 6*d.* net.

Wave Mechanics and the New Quantum Theory. By Arthur Haas, Ph.D., Professor of Physics in the University of Vienna. Translated from the German Edition (Materiewellen und Quantenmechanik), by L. W. Codd. London: Constable & Company, 10 Orange Street, Leicester Square, W.C.2, 1928. (Pp. xviii + 124.) Price 7*s.* 6*d.* net.

Elements of Geophysics as applied to Explorations for Minerals, Oil and Gas. By Dr. Richard Ambronn, Göttingen. Translated by Margaret C. Cobb, M.A., Ph.D. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xi + 372.) Price 25*s.* net.

La Théorie d'Einstein, ou la Piperie Relativiste. By Raymond Leredu. Ingénieur des Arts et Manufactures, 1928. Lille: Douriez-Bataille, 140 Rue Nationale. (Pp. 113.)

Patterns for the Construction of Crystal Models representing Actual Minerals. Designed and Drawn by Frank Smithson, Ph.D., F.G.S. London: Thomas Murby & Co., 1 Fleet Lane, E.C.4; New York: D. van Nostrand & Co., 8 Warren Street, 1928.

Cantor Lectures on Fatigue Phenomena. With Special Reference to Single Crystals. By Herbert John Gough, M.B.E., D.Sc., Ph.D. (Engineering Department, National Physical Laboratory, Teddington.) Delivered before the Royal Society of Arts during February 1928. London: F. J. Parsons, 237 Bank Chambers, 329 High Holborn, W.C.1, 1298. (Pp. 108.) Price 3*s*. net.

Statistical Mechanics. The Theories of the Properties of Matter in Equilibrium. Based on an Essay awarded the Adams Prize in the University of Cambridge, 1923–4. By R. H. Fowler, M.A., Fellow and Lecturer of Trinity College and Stokes Lecturer in Mathematics in the University of Cambridge. Cambridge: at the University Press, 1929. (Pp. 570.) Price 35*s*. net.

Die Adsorption von Otto Blüh und Nandor Stark. Sammlung Vieweg. Tagesfragen aus den Gebieten der Naturwissenschaften und der Technik. Heft 93. Braunschweig: Fredr. Vieweg und Sohn. Akt.-Ges. (Pp. 136, with 30 figures.) Price 7·75 R.M.

A Class Book of Physical Chemistry. By T. Martin Lowry, C.B.E., M.A., D.Sc., F.R.S., Professor of Physical Chemistry in the University of Cambridge, and Samuel Sugden, D.Sc., A.R.C.Sc., A.I.C., Reader in Chemistry at Birkbeck College, University of London. London: Macmillan & Co., St. Martin's Street, 1929. (Pp. vii + 436.) Price 6*s*. 6*d*. net.

Wave Mechanics. Being one Aspect of the New Quantum Theory. By H. T. Flint, Ph.D., D.Sc., Reader in Physics in the University of London. London: 36 Essex Street, W.C. (Pp. ix + 117.) Price 3*s*. 6*d*. net.

Infra-red Analysis of Molecular Structure. By F. I. B. Rawlins, M.A., Cambridge: at the University Press, 1929. (Pp. xv + 176.) Price 10*s*. 6*d*. net.

Homogeneous Catalysis. A General Discussion held by The Faraday Society, September, 1928. (Pp. 545–740.) Price 12*s*. 6*d*. net.

Colloid Symposium Monograph. Papers presented at the Sixth Symposium on Colloid Chemistry, University of Toronto, June 1928. Edited by Harry Boyer Weiser, Professor of Chemistry, The Rice Institute. New York: The Chemical Catalog Company, 419 Fourth Avenue, at 29th Street, 1928. (Pp. 346.) Price $6.50.

Alkaline Accumulators. By J. T. Crennell, B.A., and F. M. Lea, M.Sc., A.I.C. With illustrations and diagrams. London: Longmans, Green & Co., 1928. (Pp. x + 132.) Price 10*s*. 6*d*. net.

Starch. Its Chemistry, Technology, and Uses. A Handbook for the Student, the Analyst, the Consultant, and the Technologist concerned in the Manufacture and Application of Starch and Starch Products. By Lewis Eynon, B.Sc., F.I.C., and J. Henry Lane, B.Sc., F.I.C. Cambridge: W. Heffer & Sons, 1928. (Pp. viii + 256.) Price 12*s*. 6*d*. net.

La Classification Helicoïdale des Éléments Chimiques. By Charles Janet Beauvais: Imprimerie Départementale de l'Oise, 26 Rue de Malherbe, 1929. (Pp. 78.)

Annual Survey of American Chemistry. Volume III. Edited by Clarence J. West. New York: Chemical Catalog Company, 419 Fourth Avenue, 1928. (Pp. 395.) Price $3 net.

The Colloid Chemistry of Protoplasm. By L. V. Heilbrunn. Assistant Professor of Zoology, University of Michigan. Berlin: Verlag von Gebrüder Burntraeger, W.35 Schöneberger Ufer 12a, 1928. (Pp. viii + 356, with 15 illustrations.) Price 19 R.M., bound 21 M.

Inorganic Chemical Technology. By W. L. Badger and E. M. Baker, London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. viii + 228.) Price 12*s.* 6*d.* net.

An Introduction to Qualitative Chemical Analysis and the Related Chemical Principles. By D. P. Smith and H. K. Miller. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xii + 275.) Price 11*s.* 3*d.* net.

Optische Methoden der Chemie. Von Fritz Weigert, A. O. Professor an der Universität Leipzig. Leipzig: Akademische Verlagsgesellschaft, M.B.H., 1927. (Pp. xvi + 632, with 16 plates and 341 text figures.) Price 36 M., bound 38 M.

Colloid Chemistry. Theoretical and Applied. By Selected International Contributors. Collected and edited by Jerome Alexander. Volume II: Biology and Medicine. New York: The Chemical Catalog Company, 419 Fourth Avenue, at 29th Street, 1928. (Pp. 1029.) Price $15.50.

The Constitution of Sugars. By Walter Norman Haworth, D.Sc., Ph.D., F.R.S., Professor of Chemistry and Director of the Department of Chemistry in the University of Birmingham. London: Edward Arnold & Co., 1919. (Pp. vii + 100.) Price 8*s.* 6*d.* net.

The Origins and the Growth of Chemical Science. By J. E. Marsh, M.A., F.R.S., Fellow of Merton College. London: John Murray: Albemarle Street, W. (Pp. x + 161, with 15 plates.) Price 5*s.* net.

An Etymological Dictionary of Chemistry and Mineralogy. By Dorothy Bailey, Sc.B., Ph.D., and Kenneth C. Bailey, M.A., Sc.D., Fellow of Trinity College, Dublin. London: Edward Arnold & Co., 1929. (Pp. viii + 308.) Price 25*s.* net.

Handbuch der Regionalen Geologie. (The Union of South Africa.) Band VII, 7a Abteilung, 27 Heft. Herausgegeben von Prof. Dr. G. Steinmann und Prof. Dr. O. Wilckens. By A. W. Rogers, A. L. Hall, P. A. Wagner, and S. H. Haughton. Heidelberg: Carl Winters Universitätsbuchandlung, 1929. (Pp. 231, with 53 figures and 3 plates.) Price, 17 Mk.

The Scientific Principles of Plant Protection. By Hubert Martin, M.Sc., A.R.C.Sc., with a Foreword by Sir Daniel Hall, K.C.B., F.R.S. (Pp. xii + 316.) Price 21*s.* net.

The Plant-life of the Balkan Peninsula. A Phytogeographical Study. By W. B. Turrill, M.Sc., F.L.S. Assistant in the Herbarium and Lecturer in Botany at the Royal Botanic Gardens, Kew. Oxford: at the Clarendon Press, 1929. (Pp. xxiv + 487, with 17 plates and 11 maps.) Price 30*s.* net.

Biology of the Vertebrates. A Comparative Study of Man and his Animal Allies. By Herbert Eugene Walter, Professor of Biology, Brown University. New York: The Macmillan Company, 1928. (Pp. xxv + 789, with 687 figures.) Price 21*s.* net.

Creation by Evolution. A Consensus of Present-day Knowledge as set forth by leading Authorities in Non-Technical Language that all may understand. Edited by Frances Mason. New York: The Macmillan Company, 1928. (Pp. xx + 392.) Price 21*s.* net.

The Culture Value of Natural History. By Prof. J. Arthur Thomson, M.A., LL.D., Regius Professor of Natural History, University of Aberdeen. The Norman Lockyer Lecture, 1928. (Pp. 16.) London: British Science Guild. Price 1*s.* net.

Queer Fish. Essays on Marine Science and other Aspects of Biology. By C. M. Yonge, D.Sc., Ph.D., Leader of the Great Barrier Reef Expedition. London: George Routledge & Sons, 68 Carter Lane, E.C., 1928 (Pp. viii + 193.) Price 5*s.* net.

Blue Blood in Animals and other Essays in Biology. By H. Munro Fox, Professor of Zoology in the University of Birmingham. London: George Routledge & Sons, 68 Carter Lane, E.C.3, 1928. (Pp. vii + 205.) Price 5*s.* net.

The Downtonian and Devonian Vertebrates of Spitzbergen. Part K. Family Cephalaspidæ. A. Text. Skrifter om Svalbard og Nordishavet. Resultater av de Norske Statsunderstottede Spitsbergenelspeditioner. Nr. 12. Erik A. son Stensiö. Oslo: Det Norske Videnskaps-Akademi, 1 Kommisjon Hos Jacob Dybad, 1927. (Pp. xii + 391, with a second volume of plates only, 111 in number.)

Destructive and Useful Insects. Their Habits and Control. By C. L. Metcalf, M.A., D.Sc., and W. P. Flint, B.S. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. xii + 918, with 561 figures.) Price 37*s.* 6*d.* net.

Typical Flies. A Photographic Atlas of Diptera. By E. K. Pearce. Series III. Cambridge: at the University Press, 1928. (Pp. xv + 54, with 162 figures.) Price 10*s.* net.

Échinides du Musée Indien à Calcutta. Par René Koehler. Correspondant de l'Institut, Professeur de Zoologie à l'Université de Lyon. III. Échinides Reguliers. Calcutta: The Zoological Survey of India, 1927. (Pp. 158, with 27 plates.) Price 25 rupees.

Science of the Sea. An Elementary Handbook of Practical Oceanography for Travellers, Sailors, and Yachtsmen. Prepared by the *Challenger* Society for the Promotion of the Study of Oceanography. Originally edited by G. Herbert Fowler, B.A., Ph.D. Second edition, edited by E. J. Allen, D.Sc., F.R.S. Marine Biological Association of the United Kingdom, Plymouth. Oxford: at the Clarendon Press, 1928. (Pp. xxiii + 502, with 220 illustrations.) Price 15*s.* net.

Report on Mussel Purification. Being an Account of the Establishment of a System of Purification of Polluted Mussels; of the Experimental Work upon which it is based; and of certain general Considerations and Suggestions regarding the Sewage Pollution of Shell Fish in its Public Health Aspect. By R. W. Dodgson, M.D., M.R.C.P., M.R.C.S., Director of the Ministry's Shell Fish Research Station, Conway. Ministry of Agriculture and Fisheries, "Fishery Investigations," Series II, Vol. X, No. 1, 1928. London: Published by His Majesty's Stationery Office, 1928. (Pp. xvi + 498, with 15 plates.) Price 21*s.* net.

Darwinism and What it Implies. By Prof. Sir Arthur Keith, M.D., D.Sc., LL.D., F.R.S. London: Watts & Co., Johnson's Court, Fleet Street, E.C.4, 1928. (Pp. vii + 56.) Price 1*s.* net.

The Problems of Applied Entomology. By Robert A. Wardle, M.Sc., Professor of Zoology, University of Manitoba. Manchester: at the University Press, 1929. (Pp. xii + 587.) Price 30*s*. net.

The Principles of Applied Zoology. By Robert A. Wardle, M.Sc., Professor of Zoology, University of Manitoba. London: Longmans, Green & Co., 1929. (Pp. xii + 427, with 55 figures.) Price 21*s*. net.

Studies on Malaria. By Sir Ronald Ross, K.C.B., K.C.M.G., F.R.S., F.R.C.S. London: John Murray, Albemarle Street, W. (Pp. xi + 196, with 4 illustrations.) Price 5*s*. net.

Aspects of Age, Life, and Disease. By Sir Humphrey Rolleston, K.C.B., M.D. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1928. (Pp. 504.) Price 10*s*. 6*d*. net.

Researches in Polynesia and Melanesia. An Account of Investigations in Samoa, Tonga, the Ellice Group, and the New Hebrides, in 1924 and 1925. Parts V–VII (Relating to Human Diseases and Welfare). By Patrick A. Buxton, M.R.C.S., D.T.M. & H., Director, Department of Medical Entomology, London School of Hygiene and Tropical Medicine. London: London School of Hygiene and Tropical Medicine, W.C.1., 1928. (Pp. xi + 139, with 27 plates.) Price 9*s*. net.

Blood. A Study in General Physiology. By Lawrence J. Henderson, Professor of Biological Chemistry in Harvard University. Newhaven: Yale University Press; London: Oxford University Press, 1928. (Pp. xix + 397.) Price 28*s*. net.

The Fuel of Life. Experimental Studies in Normal and Diabetic Animals. By John James Rickard Macleod, M.B., LL.D., D.Sc., F.R.S., Professor of Physiology, University of Toronto, Canada. Lectures delivered at Princeton University, March 1928. Princeton: Princeton University Press, 1928. (Pp. ix + 147.) Price 11*s*. 6d. net.

Anatomy and the Problem of Behaviour. By J. E. Coghill. Lectures delivered at University College, London. Cambridge: at the University Press, 1928. (Pp. xii + 113.) Price 7*s*. 6*d*. net.

Kraemer's Scientific and Applied Pharmacognosy. Third Edition, thoroughly revised by the following named Editorial Committee: Edwin L. Newcomb, P.D., Ph.M., Phar.D., Editor-in-Chief; Leasure K. Darbaker, Phar.D., Earl B. Fischer, B.S., Edmund N. Bathercoal, Ph.D., Co-Editors. New York: John Wiley & Sons; London: Chapman & Hall, 1928. (Pp. xxxvii + 893, with 405 figures.) Price 37*s*. 6*d*. net.

How to stain the Nervous System. A Laboratory Handbook for Students and Technicians. By J. Anderson, Head Laboratory Assistant at the National Hospital, Queen Square, for Diseases of the Nervous System. With an Introduction by J. H. Greenfield, M.Sc., M.D., F.R.C.P., Edinburgh: E. & S. Livingstone, 16 Teviot Place, 1929. (Pp. x + 139.) Price 5*s*. net.

Practical Bacteriology. An Introduction to Bacteriological Technic. By Fred. W. Tanner, Ph.D., Professor of Bacteriology and Head of the Department University of Illinois. New York: John Wiley & Sons; London: Chapman & Hall, 1928. (Pp. xiv + 235, with 67 figures.) Price 12*s*. 6*d*. net.

The Problem of Fermentation. The Facts and the Hypotheses. By M. Schoen, Institut Pasteur, with an Introduction by Prof. A. Fernbach. A Monograph of the Institut Pasteur, translated from the French by H. Lloyd Hind, B.Sc., F.I.C., and revised and enlarged by the Author. London: Chapman & Hall, 111 Henrietta Street, W.C.2, 1928. (Pp. xii + 211.) Price 21*s.* net.

An Historical Introduction to Modern Psychology. By Gardner Murphy, Ph.D. With a Supplement by Heinrich Klüver, Ph.D. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1929. (Pp. xvii + 470.) Price 21*s.* net.

Experiments with Handwriting. By Robert Saudek. London: George Allen & Unwin, Museum Street. (Pp. 394.) Price 18*s.* net.

British Floods and Droughts. By C. E. P. Brooks, D.Sc., Hon. Secretary, Royal Meteorological Society, and J. Glasspoole, M.Sc., Ph.F., Professorial Associate, the Institution of Water Engineers, with an Introductory Note, by Hugh Robert Mill, LL.D., D.Sc. London: Ernest Benn, Bouverie House, E.C.4. (Pp. 199, with 15 figures.) Price 10*s.* 6*d.* net.

Automaton: or, The Future of the Mechanical Man. By H. Stafford Hatfield. London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1928. (Pp. 100.) Price 2*s.* 6*d.*

Diamond. A Descriptive Treatise. By J. R. Sutton, M.A., Sc.D. London: Thomas Murby & Co., 1 Fleet Lane, E.C.4; New York: D. van Nostrand Company, 8 Warren Street, 1928. (Pp. xii + 118, with 111 illustrations.) Price 15*s.* net.

Engines. A Book founded on a Course of Six Lectures (adapted, in the old Phrase, to a Juvenile Auditory), delivered at the Royal Institution of Great Britain. By E. N. de C. Andrade, D.Sc., Ph.D., Fellow of University College, London. London: G. Bell & Sons, 1928. (Pp. xv + 267, with 75 figures.) Price 7*s.* 6*d.* net.

Scientific Papers of William Bateson. Edited by R. C. Punnett, M.A., F.R.S., in two volumes. Cambridge: at the University Press, 1928. (Pp., Vol. I, viii + 452; Vol. II, viii + 503.) Price 42*s.* each volume.

The Passion for Life. By John Lewis, M.A., Ph.D. Newhaven: Yale University Press; London: Oxford University Press, 1928. (Pp. iv + 122.) Price 9*s.* net.

Objectives and Problems of Vocational Education. Edited by Edwin A. Lee, Ph.D. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1928. (Pp. viii + 451.) Price 15*s.* net.

Studies of Quality in Cotton. By W. Lawrence Balls, Sc.D., F.R.S., Botanist to the Khedivial Agricultural Society of Egypt and to the Egyptian Government, 1904–14 and 1927. London: Macmillan & Co., St. Martin's Street, 1928. (Pp. xxvii + 376, with 130 figures.) Price 20*s.* net.

What is Life? By Augusta Gaskell. Introduction by Karl T. Compton and Raymond Pearl. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1928. (Pp. 324.) Price 16*s.* net.

Greek Thought and the Origins of the Scientific Spirit. By Léon Robin. Professor at the Faculty of Letters of the University of Paris. London: Kegan Paul, Trench, Trübner & Co.; New York: Alfred A. Knopf, 1928. (Pp. xx + 409.) Price 21*s.* net.

Travels and Settlements of Early Man. A Study of the Origins of Human Progress. By T. S. Foster, M.D., Reader in Education, University of Bristol. London: Ernest Benn, Bouverie House, Fleet Street. (Pp. 319.) Price 21*s.* net.

Practical Colour Photography. By E. J. Wall, F.C.S., F.R.P.S. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1929. (Pp. vii + 280.) Price 15*s.* net.

Field and Colliery Surveying. A Textbook for Students of Mining and Civil Engineering Surveying. By T. A. O'Donoghue, F.G.S., M.I.Min.E., F.S.I. and T. G. Bocking, M.I.Min.E., F.S.I. New and Revised Edition. London: Macmillan & Co., St. Martin's Street, 1928. (Pp. xvi + 327, with 215 figures.) Price 10*s.* 6*d.* net.

Everyday Science. A Course of General Science related to Human Activities. By. L. M. Parsons, D.Sc., D.I.C., F.G.S., F.R.Met.Soc., Science Master, Westminster City School. London: Macmillan & Co., St. Martin's Street, 1929. (Pp. xi + 695, with 381 figures.) Price 8*s.* 6*d.* net.

Some Notable Surveyors and Map-makers of the Sixteenth, Seventeenth, and Eighteenth Centuries, and their Work. A Study in the History of Cartography. By Sir Herbert George Fordham. Cambridge: at the University Press, 1929. (Pp. xiii + 99, with 9 figures.) Price 6*s.* net.

INDEX TO VOL. XXIII (1928–1929)